Capital Gains Taxes and Real Corporate Investment: Evidence from Korea

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Abstract

This paper assesses the effects of capital gains taxes on investment in Korea, where capital gains tax rates vary at the firm-level by firm size. Following a reform in 2014, firms with a tax cut increased investment by 34 log points and issued more equity by 9 cents per dollar of lagged revenue, relative to unaffected firms. Additionally, the effects were larger for firms that appeared more cash-constrained or went public after the reform. Taken together, these findings are consistent with the "traditional view" predicting that lower payout taxes spur equity-financed investment by increasing marginal returns on 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. A traditional class of models, sometimes referred to as the "old view," predicts that lowering payout tax rates would increase investment by increasing 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 payout taxes will have no effect on investment. The "new view" assumes that firms make marginal investment choices out of retained earnings, so lowering payout 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 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 tax effects from business cycle effects, we need a control group of firms not affected by the tax change. Capital gains tax rates, however, vary at the investor-level, but not at the firm-level, in most settings, making it difficult to find a control group when estimating the effects of capital gains taxes on firm-level outcomes.

This paper studies 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 reduces the tax rates for firms affected by the changes in regulations. In Korea, capital gains tax rates vary primarily by firm size. 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 30 percent on short-term gains, and 20 to 25 percent on long-term gains. In 2014, the government unexpectedly changed the regulations on firm size. Due to this change, a set of large firms initially confined by the old regulations became reclassified as small firms. 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 to unaffected listed firms, I find that the affected firms increased investment by 34.2 log points within five years after the reform, with an implied medium-run investment elasticity of 2 with respect to the net of tax

[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), Ohrn (2018), Liu and Mao (2019), and Maffini, Xing and Devereux (2019).

rate. Furthermore, newly issued equity increased by 8.5 cents per dollar of lagged revenue for the affected firms, consistent with a class of the traditional view models of payout taxation which predict that lowering payout taxes reduces the cost of capital and spurs investment financed through equity. The estimates imply that firms with the tax cut increased investment and newly issued equity by roughly 2.7 million dollars and 2.8 million dollars on average after the reform, respectively, compared to unaffected firms.

I proceed to examine how the response to the tax cut varies by firm characteristics. Firms that were relatively cash-constrained, based on their past retained earnings, exhibited a significantly higher investment response, with an implied elasticity of 2.9, than firms that were relatively cash-rich. 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 (Myers 1984; Alstadsæter, Jacob and Michaely 2017; Zwick and Mahon 2017).

Moreover, I investigate whether lowering capital gains taxes generates different investment responses depending on whether firms go public after a tax cut. Since small shareholders in listed firms are exempt from taxes on realized gains, private firms with the tax cut could further decrease their effective capital gains tax rates through initial public offerings. I estimate that the share of firms that went public increased significantly after the reform within the treated group, relative to the control group. Treated firms that went public may have experienced an additional decrease in their cost of capital through diversification and trading cost reduction (Amihud and Mendelson 1986). I find that the investment response from treated firms that went public after the reform was substantially higher than the response from treated firms that made a decision to go public before the reform. These findings suggest that reducing capital gains taxes may be correlated with other firm-level changes that may further reduce the cost of capital and amplify the investment response.

I supplement the investment analysis by adding private firm data to the main analysis sample, and find that the affected firms, both listed and private, increased investment by 25.6 log points on average, with an implied elasticity of 1.5 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 for the affected firms led to about a 2 billion dollar increase in aggregate investment, which is roughly 1.2 percent of total investment on physical capital assets among all firms in my datasets after the policy change. This is a notable response in aggregate investment, considering that the reform was not intended as a stimulus and affected a small portion of firms, whose pre-reform investment comprised a 3 percent of total investment within all listed and private companies in my datasets.

This paper's main contribution to the existing literature is three-fold. While realized capital gains from stock sales are as important as dividends in the aggregate, evidence on how taxing capital gains will affect investment is scant. To 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 traditional view. Although a prior study by Yagan (2015) finds null effects on investment from the 2003 dividend tax cut in the U.S., which is in part consistent with the "new view," his finding on positive payout responses calls for more empirical evidence to test the competing theories of payout taxation.[2] My paper's key contribution is that it provides a set of coherent results on investment, equity issuance, and payout responses that are, on average, consistent with the traditional view. Second, my paper provides evidence that cash constraints matter for firms' investment decisions in the context of payout taxes. Third, this paper shows that lowering capital gains taxes may be correlated with other firm-level changes that further reduce the cost of capital and amplify the investment response. These findings have policy implications that lowering capital gains tax rates spurs equity-financed investment, and policymakers may benefit from considering firms' capital structure when designing an effective payout tax system.

Besides contributing to the literature on payout taxation, 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. Furthermore, my results are consistent with the conclusions from a growing empirical literature that has found substantial investment responses to corporate tax incentives (Ohrn 2018; Chen et al. 2019; Giroud and Rauh 2019; Liu and Mao 2019; Maffini, Xing and Devereux 2019) and large innovation responses to corporate income taxes (Mukherjee, Singh and Zaldokas 2017) as well as personal income taxes (Akcigit et al. 2018).

The remainder of the paper is organized as follows. Section 2 describes the institutional background. Section 3 discusses a conceptual framework. Section 4 defines my empirical design. Section 5 presents results, while Section 6 discusses economic interpretations. 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 unique empirical framework are that (1) 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 firms due to the new regulations. Note that I use a conversion ratio of 1,000 Korean won to 1 U.S. dollar throughout the paper to describe the setting and interpret the findings.

[2] I will discuss potential explanations behind the differences between my results relative to those of Yagan (2015) and other studies based on dividend taxes in Section 6.

2.1 Capital Gains Taxes in Korea

In Korea, capital gains tax rates differ primarily by firm size. There are four types of firm sizes: (1) small, (2) small-medium, (3) medium-large, and (4) large. Since I focus on the differences between small-medium and medium-large firms in my analysis, I label small-medium firms as "small" and medium-large firms as "large" for simplicity. An investor in a small firm faces a capital gains tax rate of 10 percent on realized gains when selling a stock.[3] By contrast, an investor in a large firm faces a capital gains tax rate of 30 percent when selling a stock within a year, and 20 to 25 percent for a stock held for more than one year. The tax rate depends on the size of firm when selling a stock. In Appendix A.3, I discuss the extraneous benefits that small firms are eligible to claim relative to large firms and show that the main results were not driven by these additional benefits.

An important consideration is the share of tax-exempt investors in Korea. If the majority of investors was tax-exempt (i.e., small shareholders or foreigners), then lowering capital gains taxes may not affect firms' incentives to invest because the marginal investor may not be affected by a tax cut. The definition of small shareholders depends on the share or the market value of their stocks (see Appendix A.4 for details). During the sample period, an investor was considered large if he or she owned more than 1 to 5 percent of a firm's stock. While there is a step-up basis in capital gains, inheritors still pay inheritance taxes, typically set at a higher rate than the top marginal capital gains tax rate. In my analysis sample, the share of foreign shareholders (exempt from taxes on capital gains) is about 5 percent on average during the sample period. Furthermore, the combined share of small shareholders and foreign investors in listed firms (both exempt from capital gains taxes) is about 35 percent on average during the sample period. Therefore, the majority of shareholders pay taxes on realized gains in my setting. By contrast, the share of tax-exempt shareholders in the U.S. is about 75 percent on average (Rosenthal and Austin 2016). I discuss how these differences might matter for external validity in Section 5.6.

2.2 Rules on Firm Size

From 2009 to 2014, the government enforced the following rules for determining firm size: For the main sectors (see Section 4.2) used in the analysis, a firm has to jointly satisfy the following criteria by December of year t to be classified as small in March of year $t + 1$: (1) total revenue below 100 million dollars, (2) average number of employees below 300, (3) total capital below 100 million dollars, and (4) total assets below 500 million dollars. The average number of employees is defined as the sum of daily workers employed over the entire operating days, divided by the

[3] In 2016, the tax rate increased to 20 percent for large shareholders in small firms. I account for this change when computing changes in effective capital gains tax rates for affected firms, relative to unaffected firms, in Appendix A.4.

sum of operating days in each year. The definition of employees excludes managers, researchers, and outsourced workers. Firms can outsource workers using other non-subsidiary firms to avoid the regulation on labor size. It can be costly, however, to rely heavily on outsourced workers, as evidenced by firms bunching at the labor cutoff (see Appendix A.2). Firms must report the number of employees and operating days to the government every quarter. For tax purposes, a parent firm's accounting variables incorporate its subsidiary's accounting variables by multiplying their values by its ownership rate. If the parent firm has at least 50 percent ownership, then the subsidiary's accounting variables are directly added to those of the parent firm (see Appendix A.2 for details).

To figure out which of the four criteria is most binding for firm size, I first examine the conditional probabilities in Appendix A.2 (see Table A.1). As illustrated in the table, the two most binding running variables are total revenue and the average number of employees. For example, 97 percent of firms that were jointly below the revenue and labor thresholds were classified as small firms. The main advantage of focusing solely on revenue and labor thresholds is reducing the complexity of the pre-reform rules governing firm-size classification to build my empirical framework. Incorporating other thresholds does not affect the main results, given that less than three percent of firms jointly below the labor and revenue cutoff were classified as large firms prior to the reform. Therefore, I build my empirical strategy using the revenue and labor thresholds.

In 2014, the government unified the regulations on firm size by eliminating labor and total capital thresholds and by setting a new threshold – "average revenue" based on the current and past two years. Although the reform eliminated the labor threshold for all sectors as a requirement to remain small and further changed the revenue threshold into the average revenue, it increased the average revenue threshold to 150 million dollars only for certain industries within the manufacturing sector. I provide more details on how the reform affected different sectors and the sectoral and industrial compositions of firms in Appendix A.2. 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 on firm size. This reform was discussed by government officials in early 2014, its approval was announced in August 2014, and it was implemented by the end of 2014. Furthermore, investors did not fully know which firms were affected by this reform until firm size was announced through annual reports in March 2015, as evidenced by stock price responses (see Section 5.3). Moreover, there was no other major reform that differentially affected treated and control firms in 2014. I describe how I use this reform for identification in Section 4.1.

3 Conceptual Framework

In this section, I describe a simple theoretical framework to derive comparative statics on how capital gains taxes affect firms' investment, equity issuance, and payout decisions. I begin with a two-period investment model that nests both the traditional view and the "new view," closely following the model framework by Chetty and Saez (2010).

Consider a firm that has initial cash holdings of C at the first period. The manager can use C to (1) pay out embedded capital gains through share repurchases, R, (2) pay out dividends, D, or (3) invest in a project, I, that yields profits in the second period. The firm can raise additional funds by issuing new equity, E.[4] In period 2, the firm generates net profits $f(I)$, where f is strictly concave. The firm then returns its profits by paying out embedded gains either through share repurchases (α) or through dividends $(1-\alpha)$ and principal to shareholders. Those embedded gains are taxed at the capital gains (τ_g), dividend (τ_d), and corporate (τ_c) tax rates, respectively. The manager can also buy a government bond that yields a fixed, untaxed interest rate of $r > 0$.[5] In period 1, the firm's manager chooses $\{I, R, D, E\}$ to maximize firm value such that $I + R + D = C + E$. In period 2, net-of-tax profits are distributed to shareholders. Therefore, the manager's problem is:

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

$$+ \; (1-\alpha)\frac{\overbrace{(1-\tau_d)[(1-\tau_c)f(I) + C - R - D]}^{\text{net-of-tax return to shareholders through dividends}}}{1+r} + \frac{E}{1+r}$$

I derive comparative statistics below assuming that firms pay out embedded gains to investors entirely through share buybacks ($\alpha = 1$) in period 2 since the top marginal τ_g is lower than the top marginal τ_d. The predictions are qualitatively similar with $\alpha < 1$.

[4] Firms can also raise funds through borrowing. I assume that the only source of financing is new equity in this simple model. In theory, lower capital gains tax rates may increase debts by increasing firm value and lowering the interest rates at which firms borrow from banks.

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

3.1 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 not issue new equity and repurchase shares at the same time. If a firm both issues new equity and initiates share buybacks, it can strictly increase its firm value by reducing both equity issuances and share repurchases, and lowering its tax bill by $\frac{\tau_g r}{1+r}$. Furthermore, since the marginal after-tax returns on investment is less than the risk-free interest rate, firms will not issue new equity, so the optimal level of E is zero.[6] Then, the optimal choice of share buybacks satisfies:

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

Therefore, the "new view" predicts that the capital gains tax rate (τ_g) does not distort share repurchase (R), new equity (E), or investment (I) decisions.[7] By contrast, corporate tax rates τ_c impact the firm behavior by changing the marginal benefit of investment.

3.2 The Traditional View Prediction

The traditional view considers firms that have retained earnings (C) such that $(1-\tau_c)f'(I) > r$. In this case, a firm will not repurchase shares since the marginal value of buying back shares when $E = 0$ is strictly negative given that the marginal after-tax return in investment is greater than the risk-free interest rate:

$$\frac{\partial V}{\partial R}(E=0) = (1-\tau_g)\frac{r-(1-\tau_c)f'(C)}{1+r} < 0 \tag{3}$$

Intuitively, the cash-constrained firm does not initiate share buybacks because the marginal benefit of investment exceeds the marginal benefit of payouts. Then the optimal choices of investment and equity issuances are given by:

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

[6] We have the following first order condition:

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

which implies that the firm's optimal level of new equity is 0.

[7] With a dividend payout option ($\alpha < 1$), cash-rich firms that buyback shares ($R > 0$) would set dividend payouts D such that $\tau_g = \tau_d$, such that lowering capital gains tax rates would decrease dividends (D), creating a partial substitution between dividends and share repurchases.

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

Lower capital gains tax rates increase the marginal return on investment, so that reducing τ_g induces higher investment through higher equity issuances. Corporate taxes have similar effects because they also impact the marginal return on investment. In summary, the "new view" predicts that capital gains taxes do not affect firms' new equity issuances, investment, or share repurchases, whereas the traditional view predicts that capital gains taxes affect firms' new equity issuance and investment decisions, without affecting share repurchases.

4 Empirical Strategy

This section describes my empirical strategy and data to identify the effects of capital gains taxes on corporate outcomes. Identifying tax effects on investment is challenging in part because the tax rate is potentially correlated with firms' unobservable characteristics which may impact their investment.

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 after the reform to a set of unaffected firms. I first define which firms were affected and which firms were used as the 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 the labor and total capital thresholds, so firms initially above the labor cutoff but below the other thresholds experienced a reduction in tax rates. Second, the revenue threshold became the average of revenues over the current and past two years. Lastly, the average revenue cutoff increased from 100 million to 150 million dollars, so firms initially above the original revenue threshold but below this new cutoff experienced a reduction in tax rates. I define these firms that experienced a tax reduction as the main treated group.

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 is evidence of bunching at both thresholds (See Appendix A.2). If labor and capital inputs were complementary, then eliminating the labor constraint may 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.[8] 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 a tax reduction.

On the other hand, firms above the new threshold, whose size was unaffected by the reform, serve as the control group, given that there was no change in their incentive to invest.[9] Therefore, my main analysis sample consists of the first type of treated firms that experienced a tax cut, 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 exclude big conglomerates (i.e., Samsung) and the top one percent of firms based on revenue because they may be differentially affected by an aggregate shock due to their size and business networks. I run a separate analysis for the second type of treated (bunching) firms in Appendix D. Figure 1 illustrates the reform and the two types of treated groups as well as the control group.

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

$$y_{it} = \sum_{\tau=2009}^{2019} \theta_\tau \mathbb{1}[t = \tau] \times Treated_i + \alpha_i + \alpha_t + \epsilon_{it} \tag{6}$$

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 rates, and 0 otherwise.[10] 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. θ_{2014} is normalized to be zero.

I compute and summarize the main estimates of the average tax effects on firms' outcomes by

[8] I chose firms 5 percent below the labor cutoff (between 285 and 300) and 10 percent below the revenue cutoff (between 90 million and 100 million dollars) as part of the additional, but separate, treated group. The reason is that the growth rates of labor and revenue below each threshold were 5 percent and 10 percent on average prior to the reform, respectively.

[9] 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 firms 5 percent above the new average revenue cutoff to mitigate this potential issue. My results are quantitatively similar when I drop firms in the range between 1 to 10 percent right above this new cutoff.

[10] In Appendix D, I estimate a similar specification by additionally controlling for X_i – a vector of firm characteristics, which consists of (1) basic controls, namely quartics in firm age and industry dummies interacted with year dummies, (2) additional controls, namely, dummies for each pre-reform (2014) operating profit quintile interacted with year dummies, and (3) firm value controls, namely, dummies for each pre-reform (2014) price-to-book value ratio quartile interacted with year dummies. The results from this specification are reported in Appendix D.

estimating the following difference-in-differences model:

$$y_{it} = \alpha + \theta Treated_i \times Post_t + \alpha_i + \alpha_t + \epsilon_{it} \tag{7}$$

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 (6). I report the estimates from this equation (7), as well as those from equation (6) 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 four 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. By contrast, control firms in my sample may decrease investment to go below the new cutoff, which could overstate my estimates in the short-run and could attenuate my estimates in the medium-run if they increase investment again after a tax cut. If either of these cases were prevalent, then my difference-in-differences estimates would yield a lower or upper bound on the investment elasticity by holding the definition of $Treated_i$ fixed throughout the sample period. I discuss how I address this potential issue 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, affected and unaffected firms exhibit parallel trends for key outcomes prior to the reform. Second, stock price responses show that the reform was unanticipated, and there was no other major reform that would have differentially affected treated firms relative to control firms around the end of 2014. Lastly, I conduct placebo tests defining a reform date with a year (i.e., 2007 with the global financial crisis) prior to the actual reform date. 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 2019, where I observe detailed accounting, financial, and ownership 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) Information Services. I focus on the 2009 – 2019 time period because the rules for determining firm size remained the same throughout that period, except in 2014. While I have data prior to 2009, I do not include these earlier years in the main analysis because some of the treated firms were still small firms prior to 2009, and the parallel trend starts to break down once I include them. In my sample period, firms in these sectors account for about 81 percent of all listed companies and 78 percent of all private firms.[11] Furthermore, firms in these sectors account for about 75 percent of total revenue in the entire sample. Moreover, for private firms, expenditures on physical capital are more frequently observed in these sectors than in other sectors, such as retail. I find qualitatively similar results when I run a separate analysis including firms in other sectors (see Appendix D).

I also use an accounting and financial data set for private firms from KLCA. 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 12 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 ownership rates, are missing, so I use private firm data primarily to supplement my analysis of the tax effects on investment.[12]

I use data on firms' ownership of their subsidiaries to adjust accounting values for accurately measuring firm size, which I acquired from another data company called Korea Information Service (KIS), a subsidiary of National Information and Credit Evaluation (NICE). This data set is important for correctly defining firm size (for tax purposes) and includes ownership information across publicly listed and private firms that were part of KIS database. Finally, I use the information from the Korean National Pension Service database as well as the survey data conducted by NICE to fill in any missing observations on the number of employees listed on companies' financial statements.

4.3 Variable Definitions

The main data set based on listed firms contains accounting and financial variables necessary for empirical analysis: assets, revenues, average number of employees, physical capital (tangible) assets, intangible assets, capital expenditures on physical assets and intangible assets, employee salaries, dividends, equity issuances, profits, total capital, debts, and stock prices. Furthermore, the data set is matched to a separate data set on the firms' ownership structure: ownership rates

[11] The top five sectors in my analysis sample are (1) Manufacturing, (2) Construction, (3) Information Services, (4) Retail, and (5) Professional, Scientific and Technical Services, which account for about 94 percent and 96 percent of the entire sample of publicly listed and private firms, respectively.

[12] One can also acquire data sets on both publicly listed and private companies from Korea Information Service, given that it also derives the information on these companies from the same audit reports and financial statements that KLCA uses to construct its data sets.

(aggregated at the firm-level) for anyone with more than 5 percent share, foreign investors, and managers.

The key outcome variables are investment, equity issuances, payouts, and stock prices. I define investment as the log of the firm's capital expenditures on tangible assets (i.e., plant, property, and equipment). I also use different measures of investment, such as scaling capital expenditures by pre-reform average tangible 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 scaled by lagged revenue (Yagan 2015), and use measures of payouts, such as dividends and share buybacks, scaled by lagged current profits, directly from the balance sheet data. I define stock prices as the closing prices at the end of each period. Since equity issuances and payouts are missing for most private firms in my sample, I run the analysis on capital structure only for publicly listed companies. Similarly, I use publicly listed firms to run the heterogeneity analyses, such as by firms' cash constraints, along with all related and additional tests in Appendices A, C, D and E. I use both publicly listed and private firms for analyzing tax effects on investment. I winsorize main outcome variables at the fifth and ninety-fifth percent levels, except for dividends and buybacks which I winsorize at the first and ninety-ninth percent levels as they have more skewed distributions. In Appendix D, I do a robustness check without any winsorization. Finally, I drop firms that have expenditures in total investment more than twice their lagged tangible assets. I do a robustness test by including these excluded firms and find results that are qualitatively similar to the main estimate (see Appendix D).

4.4 Descriptive Statistics

I summarize the main variables, such as total revenue, total assets, the average number of employees, 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 dollars on average, while the control firms' revenues are above 150 million dollars on average. Even though expenditures on physical capital assets are lower for treated firms than for control firms, the difference in their expenditures, when scaled by lagged tangible assets, is not statistically different from zero. Finally, listed firms are larger than private firms on average, in terms of their total assets and total capital. The treated firms account for about 3 percent of total revenue and 3 percent of total investment among all publicly listed and private firms in my datasets during the pre-reform period (2009 – 2014).

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 in Figure 2 plot raw means of log(investment) and equity issuances from 2009 to 2019, where the solid line tracks the mean of treated firms and the dashed line tracks the mean of control firms in each year. Each outcome is normalized to be zero in 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 issuances relative to control firms after 2014 suggest that the reform affected firms that experienced a reduction in capital gains tax rates.

Panel A in Figure 3 plots the coefficients θ_τ, where $\tau \in (2009, ..., 2019)$, for log(investment) as in equation (6). The graph shows a parallel pre-trend on investment between the affected and unaffected firms, as the coefficient estimates are close to zero prior to the reform. Moreover, positive and statistically significant coefficients after 2014 indicate that the tax reduction induced the affected firms to increase investment.

Table 2 presents the difference-in-differences estimation results on investment, equity issuances, and payouts using the sample of publicly listed companies. Column (1) shows that the coefficient is 0.342, with a 95 percent confidence interval of (0.181, 0.503). These estimates imply that listed firms in the treated group increased investment by roughly 2.7 million dollars on average after the reform compared to listed firms in the control group.

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

$$\epsilon_{y,1-\tau} = \frac{\%\Delta y}{\%\Delta(net\ of\ tax\ rate)} = \frac{\Delta y}{y_0} * \frac{(1-\tau_0)}{\Delta\tau} \tag{8}$$

The estimated elasticity is 1.99, with the 95 percent confidence interval of (1.06, 2.93), which implies that a 1 percent increase in the net of tax rate would increase investment by 2 percent. This computation assumes that the marginal investor in the affected firm is a large shareholder who sells stock within a year and experiences a reduction in the capital gains tax rate from 30 percent to 18 percent on average after the reform. This approach yields a lower bound on the investment elasticity with respect to the net of tax rate (Yagan 2015). Other elasticities (in the range between 2 and 6) based on alternative assumptions are presented and discussed in Appendix A.4.

Column (2) of Table 2 shows that the affected firms increased new equity issuances by 8.5 cents per dollar of lagged revenue, which is consistent with the investment response. Panel B in Figure 3 plots the coefficients θ_τ, where $\tau \in (2009, ..., 2019)$, 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 listed firms in the treated group raised new equity by roughly 2.8 million dollars on average after the reform, compared to listed firms in the control group. Note that the increase in the dollar amount of new equity issuances was comparable to the increase in the dollar amount of investment for the affected firms on average after the reform.

By contrast, the effects of lower capital gains taxes on dividend payouts are neither economically nor statistically different from zero and the effects on share buybacks are not 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 traditional view, which predicts that lowering payout taxes induces equity-financed investment without affecting corporate payouts in the short- to medium-run.

I supplement the investment analysis by adding private firms to the main analysis sample. Table 3 presents the difference-in-differences estimation results on investment, using the sample that includes both publicly listed and private firms. Column (3) shows that the estimated coefficient is 0.256, with an implied elasticity of 1.49 with respect to the net of tax rate, which is comparable to the estimate based on the sample of only listed companies. Figure 4 plots the coefficients θ_τ, where $\tau \in (2009, ..., 2019)$, 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.

Using a static investment model framework, I check whether my estimates fall within the range predicted from the model. I describe the cost minimization approach and equilibrium conditions in Appendix B to derive and compute the investment elasticity with respect to the net of capital gains tax rate (τ_g) below.

Investment Elasticity A large decrease in τ_g from τ_g^0 to τ_g^* increases capital from K^0 to K^* as follows (from one steady-state to the other):

$$(9) \qquad \frac{dK/K^0}{(\tau_g^0 - \tau_g^*)/(1-\tau_g^0)} = \left(\left(\frac{1-\tau_g^*}{1-\tau_g^0} \right)^{\frac{\left(1-(\alpha_L+\alpha_K)(\frac{1}{\epsilon})-\alpha_L\right)}{1-(\alpha_L+\alpha_K)\left(\frac{1}{\epsilon}+1\right)}} - 1 \right) * \frac{1-\tau_g^0}{\tau_g^0 - \tau_g^*}$$

Since investment I equals δK at the steady-state, we can also express the change in investment as follows:

$$(10) \qquad \frac{dI/I^0}{(\tau_g^0 - \tau_g^*)/(1-\tau_g^0)} = \frac{1}{\delta} \frac{dK/K^0}{(\tau_g^0 - \tau_g^*)/(1-\tau_g^0)} = \frac{1}{\delta} \left(\left(\frac{1-\tau_g^*}{1-\tau_g^0} \right)^{\frac{\left(1-(\alpha_L+\alpha_K)(\frac{1}{\epsilon})-\alpha_L\right)}{1-(\alpha_L+\alpha_K)\left(\frac{1}{\epsilon}+1\right)}} - 1 \right) * \frac{1-\tau_g^0}{\tau_g^0 - \tau_g^*}$$

Assuming that output elasticities are $\alpha_K \in (0.05, 0.25)$ and $\alpha_L \in (0.45, 0.65)$, and that the elasticity of product demand is $\epsilon \in (-10, -1)$, the predicted capital stock elasticity falls within $(1.05, 2.58)$. Assuming that the depreciation rate is $\delta \in (0.25, 0.45)$, then the predicted investment elasticity falls within $(2.33, 10.32)$. Based on the sample of treated listed firms, the difference-in-differences estimates of the capital stock elasticity and investment elasticity with respect to the net of tax rate are 1.08 and 1.99 respectively, assuming that the change in effective capital gains tax rates is from 30 to 18 percent (see Appendix D), which fall close to the lower bound of the model prediction.

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 taxes depending on their cash-constraints (see Section 5.5). Thus, the difference-in-differences estimates of the investment and capital stock elasticities in this paper may be smaller on average than the predicted estimates from this static investment model. Incorporating dynamic prices, adjustment costs, and firm-level heterogeneity through a general equilibrium model to predict and quantify long-run aggregate investment responses would be an interesting extension of this paper.

5.2 Initial Public Offerings

Lowering capital gains taxes may induce privately held firms to go public as initial public offerings may further reduce effective capital gains tax rates and the cost of capital. This is because small shareholders in private firms become exempt from capital gains taxes when their firms become *publicly listed*. To test the effects of the tax cut on the probability of going public, I estimate equations (6) and (7), replacing the outcome variable as the share of listed firms in a given year.

Figure 5 plots the coefficients on the $Treated \times Time$ dummies on the share of firms that went public in a given year. As the graph shows, the parallel pre-trend on the share of firms that went public in a given year 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 the share of firms with the tax cut that went public increased after the reform.

Table 3 reports the difference-in-differences estimates of the tax effects on the share of firms that went public. Column (4) shows that 14.4 percent of treated firms went public before the reform among the treated group, and a tax cut is associated with a 10 percentage points increase in the share of firms that went public after the reform, relative to the control group. In Section 5.5, I compare and discuss different investment responses for firms that went public before or after the reform, which may help us understand the underlying mechanism behind the investment results.

5.3 Stock Prices

Lowering capital gains tax rates for a given stock may increase its demand for that stock, holding everything else equal, by increasing the net present value of future returns on the stock. If investors were ambivalent about capital gains taxes or were unaware of the reform in 2014, then the reform may 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 (6) and (7), replacing the outcome variable with log(stock prices), replacing year dummies with month dummies and restricting the time interval from the beginning of 2014 to the end of 2015 (a two-year window). I also restrict the sample to be a balanced panel to control for a changing composition that can affect stock prices.

Figure 6 plots the coefficients on the $Treated \times Time$ dummies for log(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 a tax cut experienced an increase in their market value. Moreover, these results suggest that the reform was unexpected, as stock prices reacted only after the reform was implemented.

Table 4 reports the difference-in-differences estimates of the tax effects on stock prices. Column (1) shows that a tax reduction is associated with a 11.7 log points increase in stock prices, with an implied elasticity of 0.68 with respect to the net of tax rate, suggesting that lowering capital gains taxes positively affects firms' market value. Note that the number of treated firms is smaller relative to the one reported in Table 2. This is because I do not have pre-reform stock price data for firms that went public after the reform. The number of treated and control firms is further reduced as I restrict the sample to have non-missing stock price information during the entire two year period.

5.4 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 (7) with basic and additional controls (see Section 4.1) and find qualitatively similar results. One valid concern is that firms in the treated group were smaller, in terms of revenue and labor size, relative to firms in the control group, and the difference in firm size could drive the difference in investment responses if there was any unobserved shock that differentially affected firms of different sizes around the reform year. Relatedly, since treated firms were smaller than the control group, there could be a mean-reversion that overstates the investment response based on the difference-in-differences estimation. To address this potential issue, I non-parametrically control for firms' pre-reform profitability and implement a DFL-reweighting method to make the treated and control groups more comparable in terms of size (DiNardo, Fortin and Lemieux 1996). I also conduct a placebo test defining the reform year as an earlier year (i.e., the global financial crisis in 2007) and fail to reject the null hypothesis that the effects on the main outcomes are not statistically different from zero. Second, I repeat the analysis by winsorizng outcome variables at 1 percent and 99 percent levels or without any winsorization and find that the results are qualitatively similar. Third, I find qualitatively similar results when I include firms in other sectors. Lastly, I use different measures of investment, such as scaling capital expenditures by pre-reform average tangible assets and find qualitatively similar results. The investment elasticity based on this set of robustness tests falls within $(1.5, 2.5)$. Results from these robustness tests are reported in Appendix D.

To address a concern over a few potential outliers driving the main results, I estimate jackknife coefficients by using the main difference-in-differences model in equation (7), where I leave out one treated firm at a time and find that those coefficient estimates are very similar to the main estimate. Furthermore, I conduct a set of block permutation tests on the main outcomes similar to those used in Chetty, Looney and Kroft (2009). In each of these tests, my main estimate is in the tail of the distribution of the permutations, consistent with my results not being driven by a few outlier firms (see 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. 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 tax rate. By contrast, if these firms had a pent-up supply of projects that were held back due to higher taxes, then these firms might invest aggressively after the cutoff became no longer binding. Using the sample of both publicly listed and private firms, I find that the investment response is lower than the one from the firms with a tax cut, consistent with the idea that firms that were bunching did so because they did not have investment opportunities to justify crossing the thresholds (see Appendix D).

5.5 Heterogeneity Analysis and Mechanisms

In this subsection, I discuss and empirically test potential mechanisms for investment responses to a reduction in capital gains tax rates. This is primarily achieved by studying the heterogeneity in firm responses to tax changes. In addition to shedding light on mechanisms, the heterogeneity analysis also bolsters the credibility of the main results by showing that firms induced to issue more equity in response to tax changes also disproportionately increase investment.

Understanding the potential mechanisms behind investment responses would be important for policymakers designing an effective payout tax system. The first channel is the cash-constraint: cash-constrained firms that raise funds through new equity potentially face a higher marginal cost of investment than do firms that use internal funds (Myers 1984), therefore the effects of lower capital gains taxes might be larger for more cash-constrained firms. The second mechanism relates to firms going public, in which they face even lower effective capital gains tax rates and cost of capital, as small shareholders in listed firms are exempt from taxes on realized gains.

5.5.1 Cash-Constraint Channel

My main results are consistent with the traditional view predictions that lowering payout tax rates would induce firms to increase investment by issuing new equity. By contrast, the "new view" assumes that firms use retained earnings as a marginal source of investment and predicts that lowering payout 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, since external financing is costly. Therefore, cash-constrained firms may react more aggressively to lower capital gains taxes.

Identifying which firms are cash-constrained is difficult. For example, lagged revenue, as used in (Yagan 2015), directly determined whether a firm was treated by the reform in my setting, so defining cash-constraints by measures of firm size may incorrectly attribute firm size effects to the cash-constraint effects. Instead, I use past retained earnings scaled by assets averaged over the current and past two years at the time of the reform as a proxy for cash-constraints (Becker, Jacob and Jacob 2013).[13] 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). 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 similar when I use different percentiles (i.e. tercile) for the dummy variable (see Appendix D). I then estimate the following triple-difference model:

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

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.

Panel A in Figure 7 plots the coefficients on the $Treated \times Time$ dummies for log(Investment) in each year, separately for cash-constrained firms (in dark navy line) and for cash-rich firms (in red line). As the graph shows, the parallel pre-trend on investment for the affected and unaffected firms holds reasonably well for both types of firms, as the coefficient estimates are close to zero prior to the reform. The effects of lower tax rates on investment are positive and statistically significant, although the effects appear to be smaller for cash-rich firms. Panel B in Figure 7 plots the estimates for equity issuances. As shown in the graph, the parallel pre-trend holds reasonably well for both types of firms, and the effects of lower taxes on equity issuances are positive and statistically significant, although the effects look smaller for cash-rich firms.

Table 5 shows the results for this triple-difference estimation. Column (1) shows that the investment response is greater for cash-constrained firms, with an implied elasticity of 2.9. On the other hand, the coefficient on the triple interaction term is negative and statistically significant, meaning that when we add this coefficient to the coefficient estimate for cash-constrained firms, the investment elasticity is smaller for cash-rich firms, with an implied elasticity of 1.1. Moreover, increases in equity issuances are larger for cash-constrained firms. These results are consistent

[13] I use other measures of cash-constraints, as used in Yagan (2015), such as firm age, dividend payouts, leverage, and cash (liquid assets), and find qualitatively similar results (See Appendix C.1). In Korea, the mean and median firm age among the sample of treated firms is 22 and 19, respectively, while the mean and median firm age among the sample of control firms is 26 and 24, respectively. In the sample used in Yagan (2015), the mean and median age for the U.S. C-corporations and S-corporations are 26 and 22, and 27 and 23, respectively.

with the idea that cash constrained firms may face a relatively higher marginal cost of investment and may react more aggressively to lower taxes than do cash-rich firms.

In terms of dollar amount, investment and equity issuances increased by roughly 3.9 million dollars and 3.5 million dollars, respectively, on average after the reform for the treated firms among the cash-constrained group. By contrast, investment and equity issuances increased by about 1.6 million dollars and 1.9 million dollars, respectively, on average after the reform among the cash-rich treated firms. Therefore, these heterogeneity results support the argument that firms that increased investment after the reform did so by increasing equity issuances, as firms that increased investment more also issued more equity.

To provide evidence that investment and equity issuance responses to the tax cut are linked at the firm-level, I bin firms into terciles based on (1) their pre-reform cash scaled by total assets and (2) their age, creating a total of 9 bins, and estimate a separate difference-in-differences model for firms under each bin. Panel A in Figure 8 plots the coefficients in a binned scatter and predicts their relationship using a linear fit. The estimated coefficient on the slope of this fitted line is 3.63, with the standard error of 1.2, which implies that there is a positive and statistically significant relationship between investment and equity issuance responses by the treated firms. Moreover, the midpoint of the fitted line matches the average estimates on investment and equity issuances, implying that if I were to extrapolate these estimates across my sample, I would find that firms that issued more equity also increased investment more in response to the tax cut. Therefore, this result supports that investment and equity issuance responses to the tax reduction are linked at the firm-level.

I interact an additional measure of cash-constraints to repeat a similar exercise. Specifically, I bin firms into above and below the median based on (1) their pre-reform retained earnings scaled by total assets, (2) their age, and (3) their pre-reform cash scaled by total assets, creating a total of 8 bins. Panel B in Figure 8 shows that the estimated slope of the fitted line is 3.6, with the standard error of 1.6, implying that there is a positive and statistically significant relationship between investment and equity issuance outcomes, consistent with Panel A in Figure 8.

As shown in Section 5.1, the investment elasticity from private firms is comparable to the one from listed firms. In theory, private firms' investment response can be smaller since private firms typically have a concentrated ownership structure, which can dampen their investment response to a payout tax cut (Chetty and Saez 2010) – see Appendix C.2 for details. On the other hand, private firms may be more cash-constrained than listed firms, which can lead them to invest aggressively after the tax cut. I find that the tax effect is smaller for private firms, consistent with the idea that private firms may have more concentrated ownership on average.

5.5.2 Initial Public Offerings Channel

A related channel can generate heterogeneous investment responses to a payout tax cut: firms going public. Lowering capital gains taxes may induce privately held firms to go public, as initial public offerings may further reduce effective capital gains tax rates and the cost of capital. In turn, firms that go public may find it more attractive to increase investment as they experience an additional decrease in their cost of capital through a reduction in risk and trading costs. By contrast, going public may discourage investment by engendering agency conflicts. Empirical evidence on the relationship between initial public offerings and firms' investment is mixed (Asker, Farre-Mensa and Ljungqvist 2015; Maksimovic, Phillips and Yang 2019).

Going public may decrease firms' cost of capital by reducing the risk of investment and by lowering trading costs. By allowing investors to trade stock in public, listed firms may face a lower required rate of return on their investment through diversification. Furthermore, being able to trade shares on the stock exchange increases the liquidity of stock, which in turn lowers trading costs and the cost of capital (Amihud and Mendelson 1986). Moreover, large private firms reduce tax rates for small shareholders from 20 percent to 0 percent just by going public, which may significantly decrease their effective capital gains tax rates depending on their share of small shareholders before going public. Since I do not have ownership data for private firms, I estimate the change in effective capital gains tax rates for private firms that went public after the reform using the ownership data on large shareholders in listed firms and assuming that large shareholders' ownership rates remained constant on average after going public (see Appendix A.4).

While going public has the benefit of removing capital gains taxes for small shareholders, initial public offerings are not always ideal. First, there is a cost of disclosing proprietary information, especially to competitors. Second, reporting and filing with a security exchange commission are costly. Third, there are costs of corporate control: outside stockholders can pressure managers towards maximizing shareholders' interests, even if they only represent a minority position.

The reform provided tax incentives for privately held treated firms to go public. By going public, treated private firms can further reduce their effective capital gains tax rates; these incentives might be stronger for private firms that had a larger share of small shareholders. As Section 4.2 shows, the share of listed firms within the treated group increased substantially after the reform.

To separately test the tax effects on investment from the effects of initial public offerings, I estimate the difference-in-differences model (7) based on whether the treated firms went public before or after 2015. I use the same baseline control group in both regressions because the number of control firms that went public after the reform was very small, so the share of listed firms among the control group stayed roughly the same after the reform.

Panel A in Figure 9 separately plots the coefficients on the *Treated* $\times$ *Time* dummies for *log*(Investment) in each year for treated firms that went public before 2015 (in red line) and for treated firms that went public after 2015 (in dark navy line), relative to the baseline control group. 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 lower taxes on investment are also positive and statistically significant, although the effects appear to be larger for firms that went public after 2015. Similarly, Panel B in Figure 9 separately plots the estimates for equity issuances for treated firms that went public before 2015 and for treated firms that went public after 2015, relative to the same baseline control group. As the graph shows, the parallel pre-trend on equity issuances for the affected and unaffected firms holds reasonably well, and the effects of lower tax rates on equity issuances are also positive and statistically significant, although the effects look stronger for treated firms that went public after 2015. Note that when to go public is an endogenous decision, so the results from this estimation show how the effects of the tax cut are correlated with the IPO decision, rather than a direct causal effect of IPO on investment.

Table 6 presents the results of these separate estimations. Columns (1) and (2) show that the tax cut is associated with a 24.1 log points increase in investment, and newly issued equity increased by 5.5 cents per dollar of lagged revenue for treated firms that made a decision to go public before 2015. Columns (3) and (4) show that the tax cut is associated with much higher increases in investment and equity issuances for treated firms that made a decision to go public after 2015. In terms of dollar amount, investment and new equity issuances increased by roughly 2.2 million dollars and 1.2 million dollars on average after the reform for treated firms that decided to go public before 2015, respectively. On the other hand, investment and new equity issuances increased by roughly 4 million dollars and 5 million dollars, respectively, on average after the reform for treated firms that went public after 2015. These findings suggest that reducing capital gains taxes may be correlated with other firm-level changes that further decrease the cost of capital and amplify the investment response.

5.5.3 Correlation Between Cash-Constraints and Initial Public Offerings

The correlation between the dummy for whether a firm is a cash-rich firm and the dummy for whether firms went public after 2015 is about -0.1 in my sample, implying that there is a slightly less chance for cash-rich firms to go public after the reform. Column (5) of Table 5 shows that the difference in the share of firms that went public is not statistically different from zero between cash-constrained firms and cash-rich firms. Furthermore, Appendix D shows the results by firms' cash-constraints, separately for firms that went public before 2015 and for firms that went public after 2015 (see Table D.6). The effects appear to be stronger for more cash-constrained firms in both cases, although the differences are not statistically significance due to smaller sample sizes. Therefore, I conclude that initial public offerings channel is a closely related (as it also affects investment through reducing the cost of capital), but independently important channel that may explain different investment responses to a payout tax cut.

5.6 External Validity

Arguably, an external validity concern is that these effects might be local to only Korean firms. I use firms in Korea because the unique institutional setting provides reasonable variation to identify the tax effects. While the implications from this paper's findings may be relevant for other countries, as capital gains taxes are important sources of tax revenues in many developed countries, there are institutional differences, such as the share of tax-exempt shareholders, and underlying differences in firm characteristics, such as cash-constraints, that can generate different investment responses to a payout tax cut across different settings. For example, the share of tax-exempt shareholders in the U.S. is about 75 percent on average (Rosenthal and Austin 2016), while the portion of shareholders exempt from capital gains taxes is at most 35 percent in Korea. Furthermore, extrapolating any local average treatment effects to a broader population in a different setting should be done with caution. In Appendix F, I provide a more detailed discussion on to what extent the Korean setting may or may not translate to other countries.

6 Economic Interpretations

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 discusses potential explanations for the magnitude of this estimate, compared to estimates from the existing literature.

The hypothesis that a payout tax cut would significantly increase investment is based on a class of models representing the traditional view (Harberger 1962; Feldstein 1970; Poterba and Summers 1983). In these models, a payout tax cut would reduce the cost of capital for firms that finance marginal investment with newly issued equity. For example, a decrease in capital gains tax rates 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,

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

where r, τ_c, τ_d, τ_g, and ρ are the expected rate of return, corporate income tax rate, dividend tax rate, capital gains tax rate, and share of 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 -0.81. I compute this elasticity by setting $\tau_d = 0.42$ and $\rho = 0.07$, because the top marginal dividend tax rate was 42 percent, and dividend payouts as a share of net income was 7 percent on average among treated listed firms before the reform. The estimated investment elasticity of 1.49 from the sample of both listed and private firms implies the user cost elasticity of -1.84. Based on the estimates from the sample of U.S. firms, Zwick and Mahon (2017) find the investment elasticity with respect to the cost of capital between -0.8 and -3.3, where my estimate falls within this range.[14]

The estimates in this paper are larger than the estimates in other studies based on dividend taxes.[15] I propose two potential explanations for the differences. First, as discussed in Section 2, the share of tax-exempt investors may be far greater in other settings, such as the U.S., than the one in Korea. If the majority of investors is exempt from payout taxes, then firms' investment may be less responsive to tax incentives. In Korea, the majority of shareholders pays taxes on realized gains, which can explain a larger investment response to a tax reduction in my setting. Second, the affected group in my analysis sample mostly consists of small firms that recently became large. This implies that these firms were likely to be cash-constrained and to respond to a tax cut more aggressively than larger firms (Zwick and Mahon 2017).

Another key difference is that while I find noisy positive effects on share buybacks on average, prior studies have found positive and statistically significant effects of reducing dividend taxes on dividend payouts in the United States (Chetty and Saez 2005; Brown, Liang and Weisbenner 2007; Blouin, Raedy and Shackelford 2011; Yagan 2015). I propose two reasons for these differences. First, share repurchases are much more uncommon relative to dividend payouts in my sample and in the United States. One may expect that reducing capital gains taxes may increase share

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

[15]The estimated effects of dividend taxes on investment differ across several studies. For example, Yagan (2015) finds no investment responses to the 2003 dividend tax cut in the U.S., while Love (2021) finds positive responses on equity issuances and "non-capital" investment (i.e., R&D) among publicly listed companies based on the same setting. Furthermore, Boissel and Matray (2021) find positive investment effects of a divided tax hike in France, while Bach et al. (2021) find weakly negative investment effects using a similar setting, but a different dataset. Alstadsæter, Jacob and Michaely (2017) find overall null effects of a dividend tax cut in Sweden, but also find that cash-constrained firms increased investment after the tax cut.

buybacks, but the average buyback as a share of net income is about 3 percent among treated firms before the reform. By contrast, the average dividend payout as a share of net income is about 7 percent among treated firms before the reform. Even though buybacks are relatively infrequent events, capital gains taxes still matter because investors sell their share both on the stock exchange and in private markets. Furthermore, investors are highly responsive to capital gains taxes, as evidenced by large stock price responses (Section 5.3). Therefore, reducing capital gains taxes may not increase share buybacks at least in the short- to medium-run, contrary to significant and immediate dividend payout responses to a dividend tax cut in the United States.

Second, there might be country-specific institutional differences that generate heterogenous payout responses to a tax cut in general. For example, if a large share of firms in a given country consists of cash-rich or dividend-paying firms, then one may expect to see a larger payout response to a dividend tax cut than an investment or equity issuance response. By contrast, if a large share of firms has to rely on raising equity to finance marginal investment (typically younger firms with low cash or dividend payout ratio), then one may expect to find a larger investment and equity issuance response to a payout tax cut. Furthermore, certain countries that had made buybacks difficult to implement (i.e., France) or illegal (i.e., Germany and Sweden) until the early 2000s (Becker, Jacob and Jacob 2013) may have a greater proportion of firms with stronger investment responses to a capital gains tax cut, relative to other countries. These heterogenous responses, either driven by different firm characteristics or institutional settings, may be observed in different proportions across different countries over time, which can lead to different observed aggregate effects.

7 Conclusion

This paper exploits a unique institutional setting, a policy reform, and proprietary 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 jointly by revenue and labor thresholds. In 2014, the government changed the 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 equity issuances increased significantly for the affected firms. Additionally, I find that investment responses are larger for firms that appeared more cash-constrained. These results are robust across various specifications and consistent with a class of the traditional view models predicting that lowering payout tax rates spurs equity-financed investment.

The findings in this paper have important policy implications. In terms of aggregate dollars, the total investment was roughly 9.3 billion dollars among the treated firms after the reform. Then the aggregate increase in investment is 2.1 billion dollars within the main analysis sample, which is roughly 1.2 percent of total investment within all publicly listed and private firms after the reform.[16] Even though a 1 percent increase in aggregate investment by the treated firms may seem like a small change, this investment increase is large considering that the share of affected firms was relatively small and the reform was not intended as a stimulus. Since the share of treated firms was relatively small in the Korean economy, the aggregate increase in investment may still appear to be small. Moreover, the large investment response to the tax cut was concentrated among firms that appeared more cash-constrained, so policymakers may benefit from considering firms' capital structure when designing an effective payout tax system.

[16]This calculation is based on a formula, $I_{actual} = I_{counterfactual} \times e^{\theta}$, where $\theta = 0.25$ is from the difference-in-differences estimation of the tax effects on investment using the sample of both listed and private firms (Table 3). The change in aggregate investment is computed by $\Delta I = I_{actual} \times (1 - e^{-\theta})$. Note that the aggregate amount is based on the entire sample of my datasets, which includes both publicly traded and private firms across all sectors, but excludes firms that are too small to be audited (assets below 12 million dollars in a given year).

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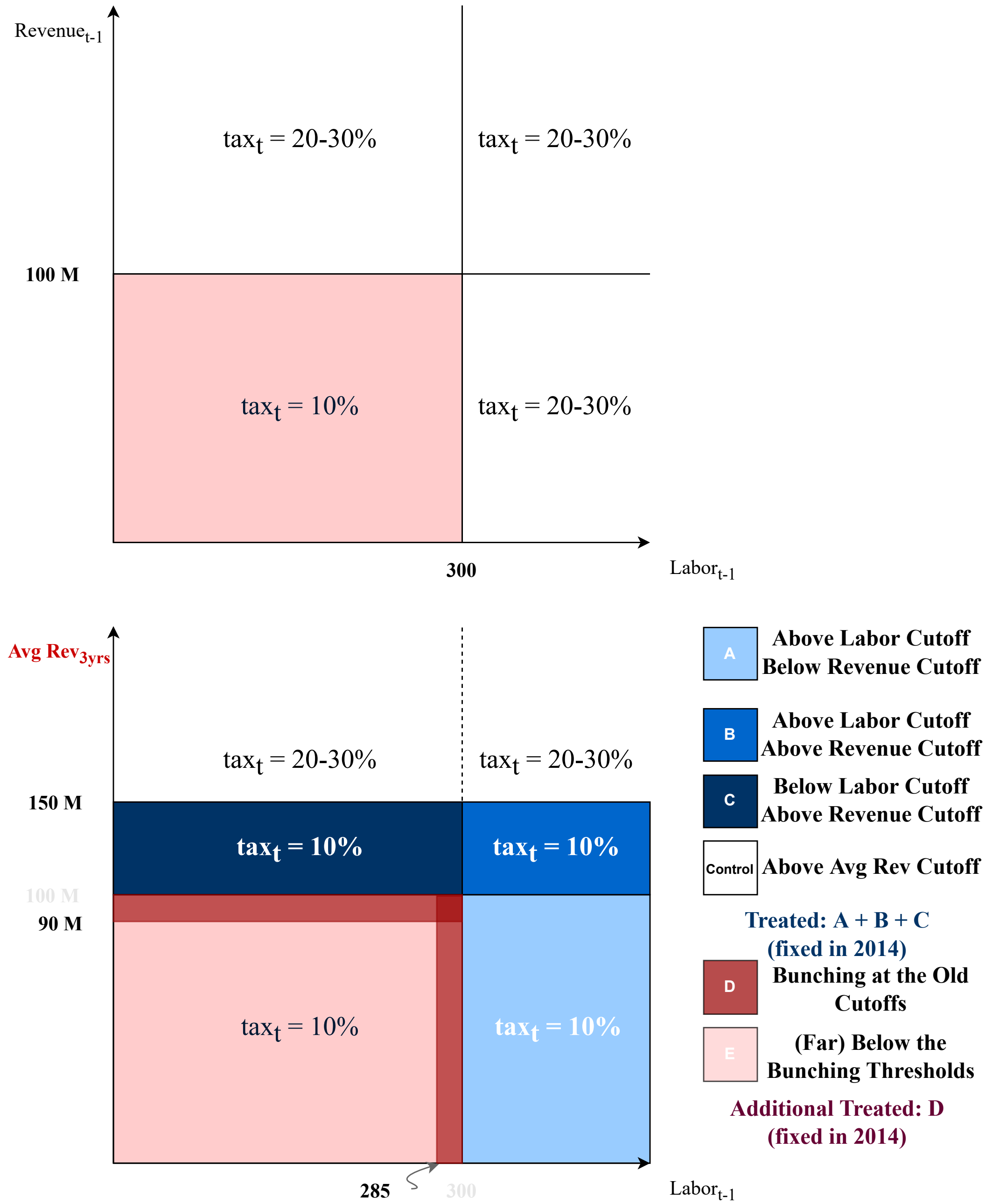
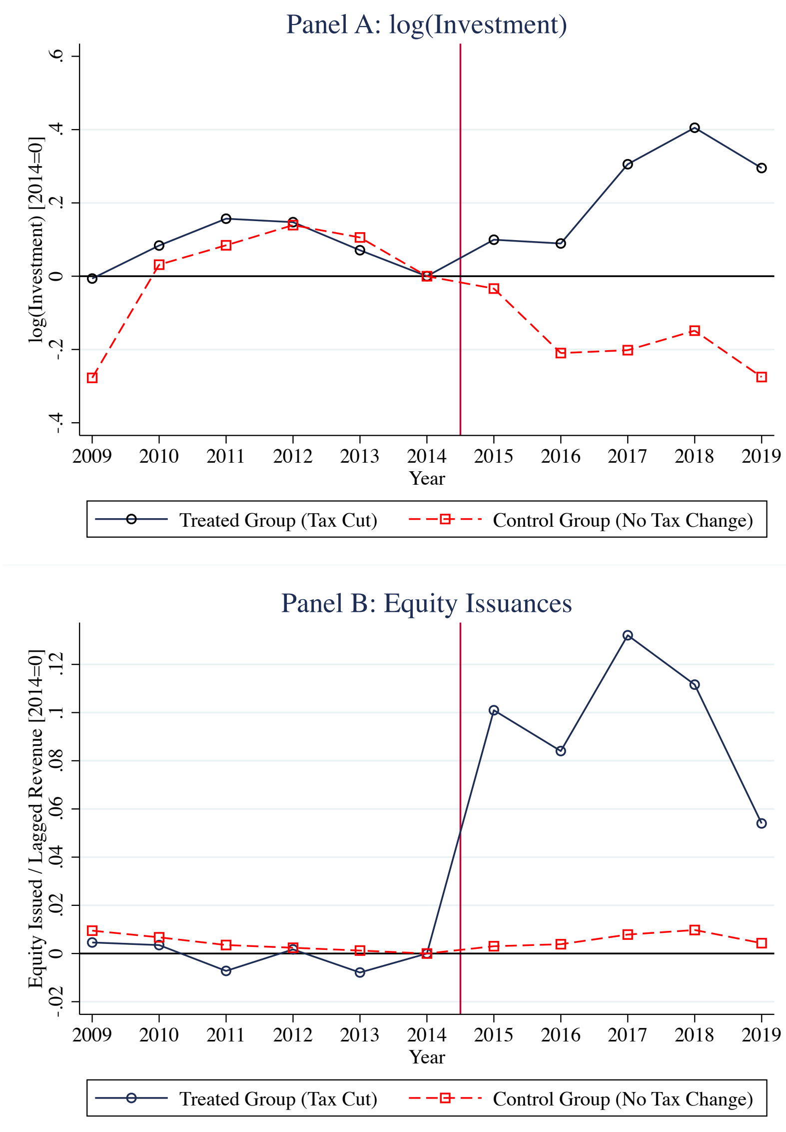
Figure 1: Policy Reform 2014 and Treated vs. Control Groups



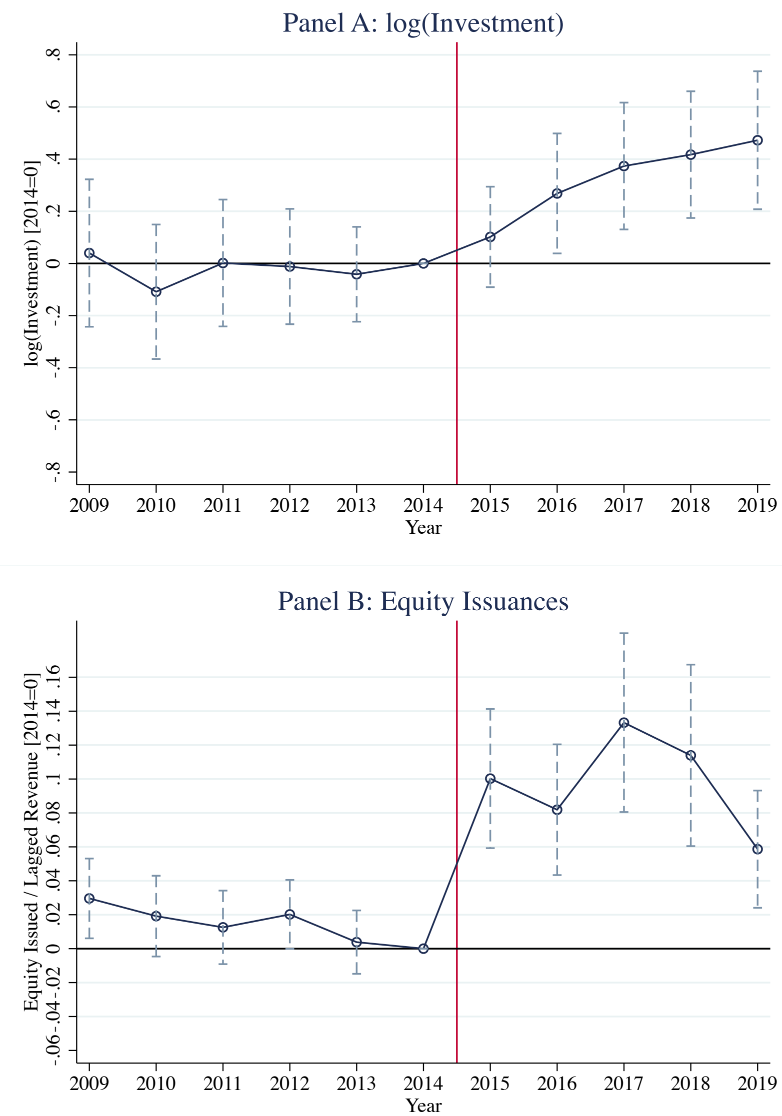
Notes: This figure illustrates how the reform in 2014 assigned firms into the treated or control groups. The figure on the top shows the initial rule on firm size prior to the reform, where firms in the pink area are jointly below labor and revenue threshold at time $t-1$ and face a tax rate of 10 percent. 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) 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 (in the white areas above the new revenue cutoff). Firms in the pink area were not directly affected 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 were removed after the reform. The percentiles in brackets are based on the sample of publicly listed firms.

Figure 2: Raw Means of Investment and Equity Issuances of Affected and Unaffected Firms



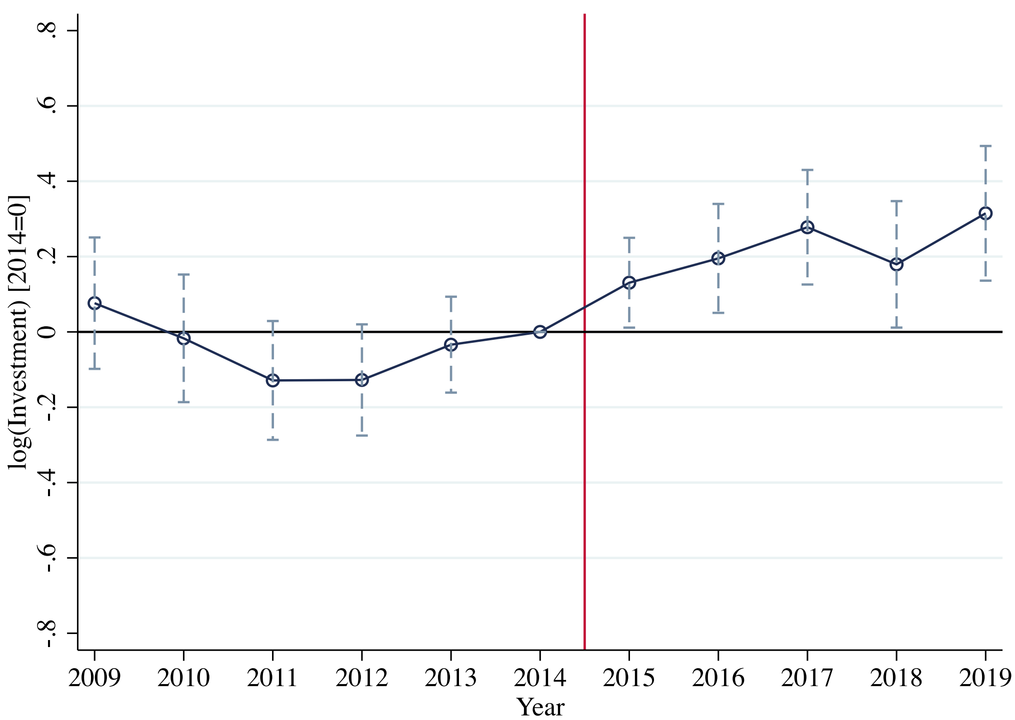
Notes: Panel A in this figure shows raw means of *log*(investment) of treated (solid line) and control (dashed line) firms in each year, from 2009 to 2019. Means of each outcome for each group are normalized to be zero in year 2014, when the reform was implemented. Panel B shows raw means of equity issuances, defined as non-negative annual changes in total paid-in capital, scaled by lagged revenue. The sample is restricted to publicly listed firms.

Figure 3: Tax Effects on Investment and Equity Issuances



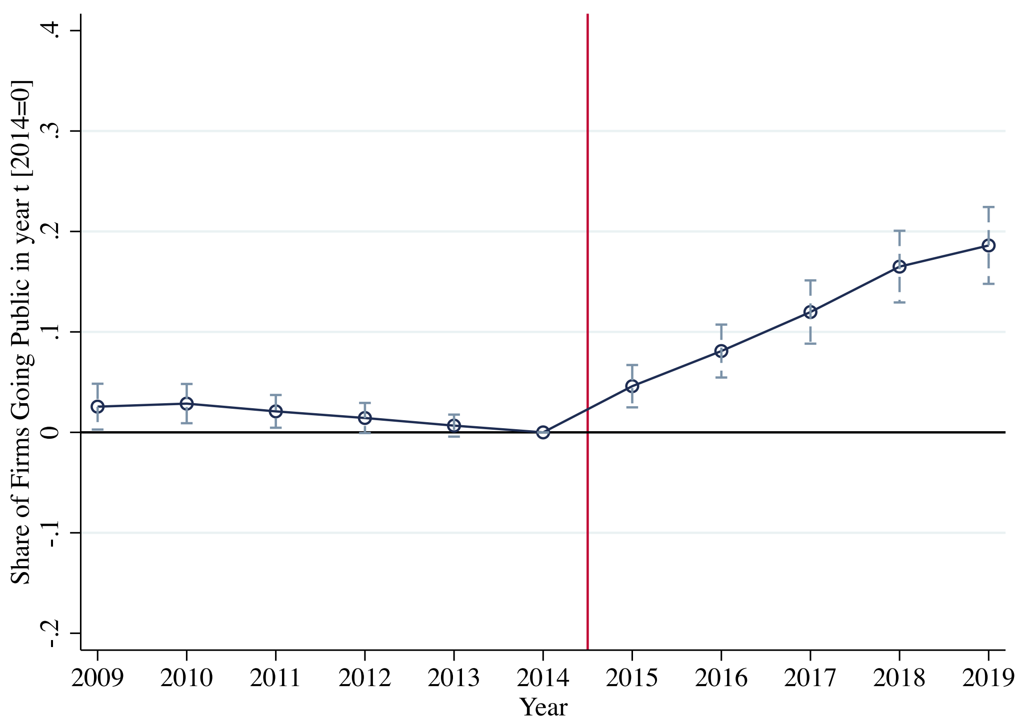
Notes: Panel A in this figure shows the coefficients on $Treated \times Time$ for firms' investment, defined as log(expenditures on physical capital assets), in equation (6). The dashed lines indicate 95% confidence intervals for these coefficient estimates. The solid vertical line indicates the reform year. Panel B shows the coefficients on $Treated \times Time$ for firms' equity issuances, defined as non-negative annual changes in total paid-in capital, scaled by lagged revenue, in equation (6). The sample is restricted to publicly listed firms.

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



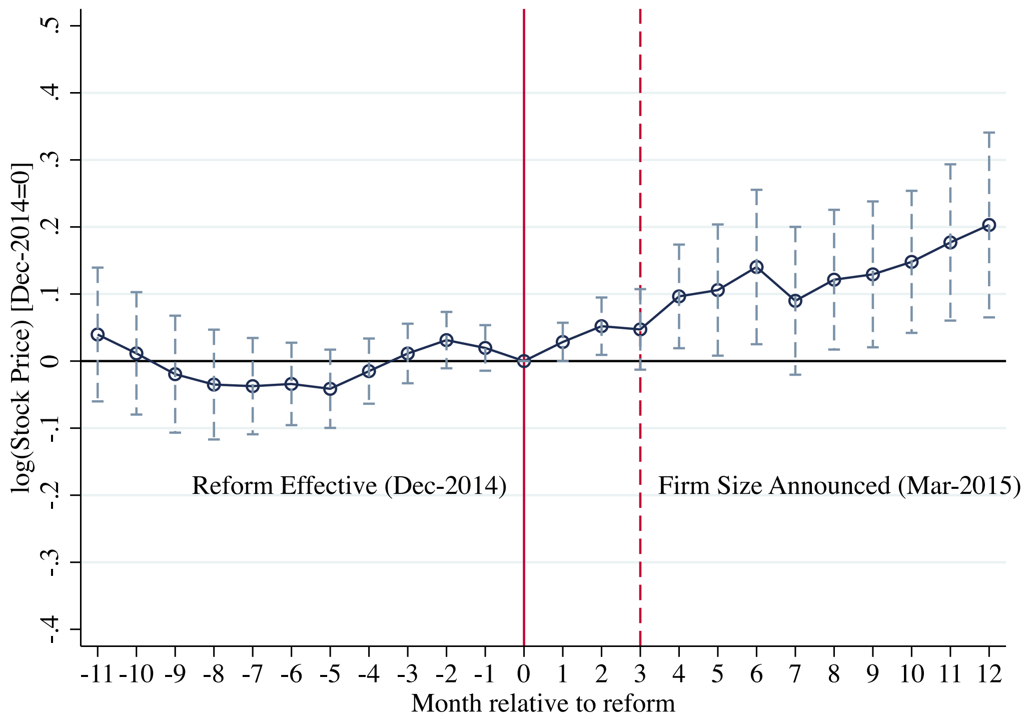
Notes: This figure shows the coefficients on *Treated* × *Time* for firms' investment, defined as *log*(expenditures on physical capital assets), in equation (6) using both publicly listed and private firms. The dashed lines indicate 95% confidence intervals for these coefficient estimates. The solid vertical line indicates the reform year.

Figure 5: Tax Effects on Share of Firms Going Public



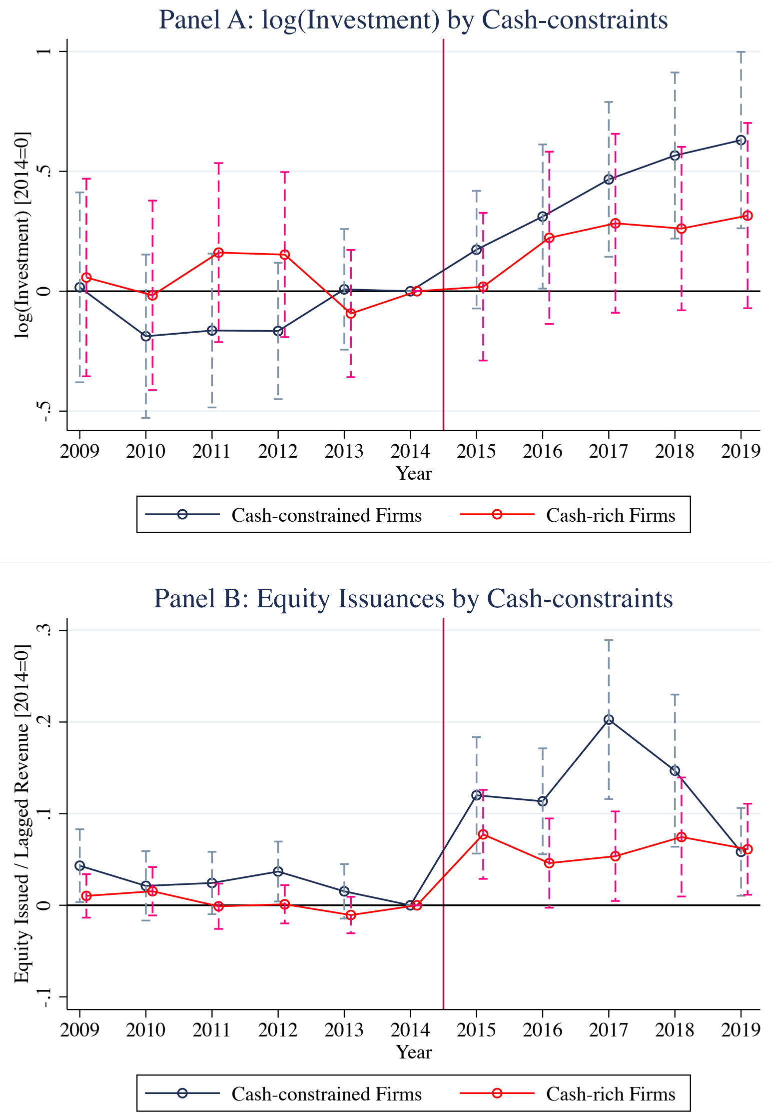
Notes: This figure shows the coefficients on *Treated* × *Time* on the share of firms that went public in a given year, as in equation (6). The dashed lines indicate 95% confidence intervals for these coefficient estimates. The solid vertical line indicates the reform year.

Figure 6: Tax Effects on *log*(Stock Prices): Listed Firms



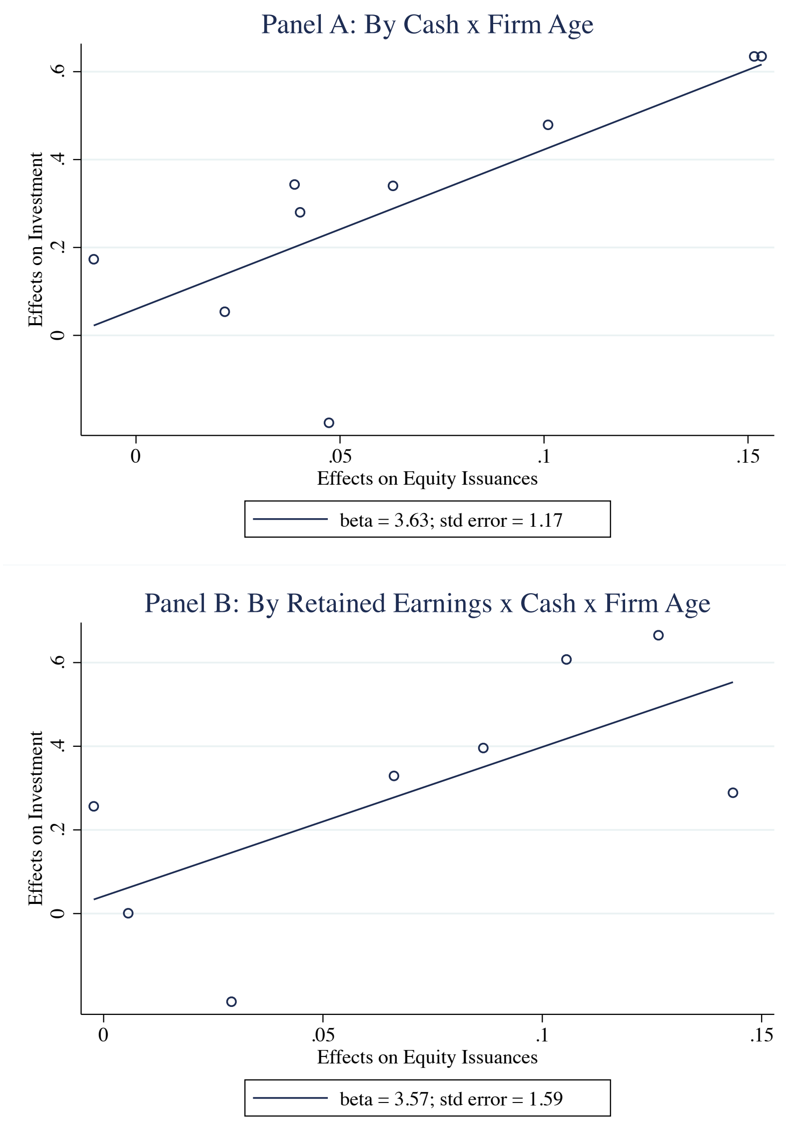
Notes: This figure shows the coefficients on $Treated \times Time$ for each time period (month). The outcome variable is log(stock prices). The dashed lines indicate the 95% confidence intervals for those coefficient estimates. The solid vertical line indicates the month in which the reform was implemented, and the dash vertical line indicates the month in which 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 a balanced panel of publicly listed companies, where I observe their stock prices at the monthly frequency.

Figure 7: Tax Effects on *log*(Investment) and Equity Issuances by Cash Constraints



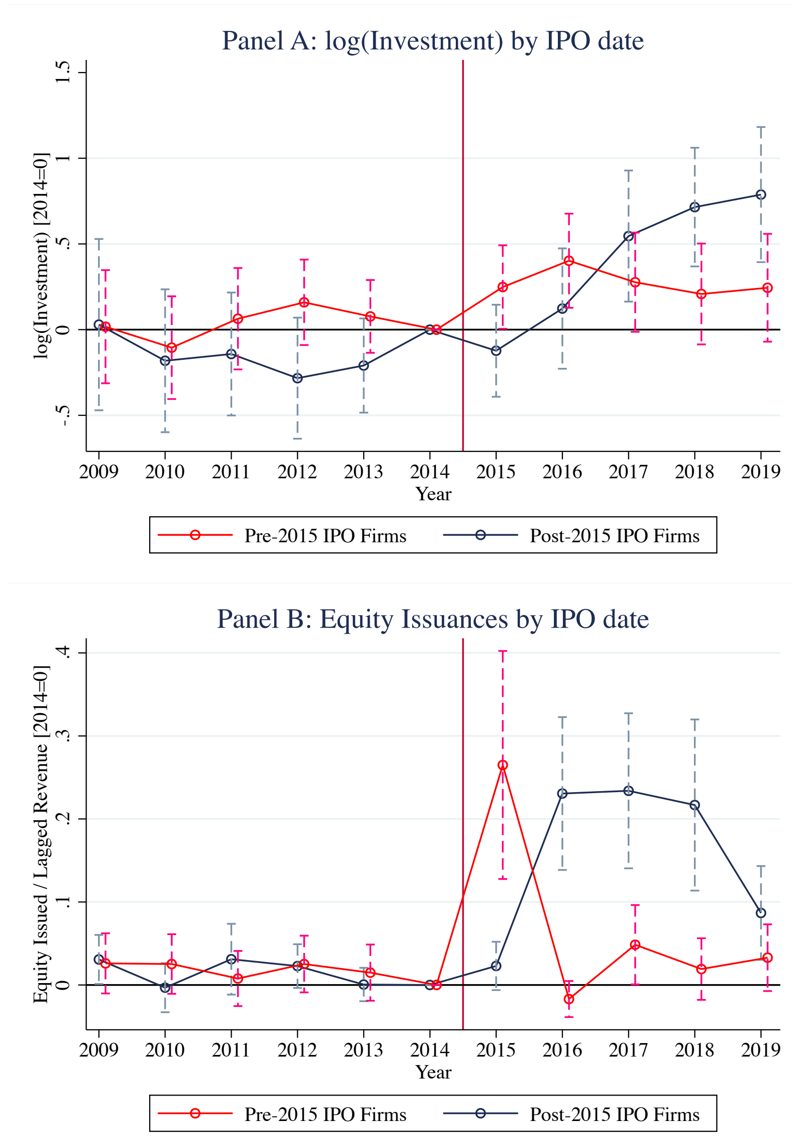
Notes: Panel A in this figure shows the coefficients on $Treated \times Time$ for *log*(Investment), as defined in equation (6), separately for the relevant sample among the cash-constrained firms and for the relevant sample among the cash-rich firms, as defined in Section 5.5. The dark navy line indicates the estimates for cash-constrained firms, and the red line indicates the estimates for cash-rich firms. The dashed lines indicate the 95% confidence intervals for those coefficient estimates. The solid vertical line indicates the reform year. Panel B shows the coefficients on $Treated \times Time$ for equity issuances, separately for the relevant sample among the cash-constrained firms and for the relevant sample among the cash-rich firms. The sample is restricted to publicly listed firms.

Figure 8: Tax Effects on *log*(Investment) and Equity Issuances Across Bins



Notes: Panel A in this figure shows the linear fit of the relationship between the difference-in-differences estimates on investment (y-axis) and on equity issuances (x-axis) across joint bins of (1) pre-reform cash scaled by total assets and (2) firm age. Panel B shows the linear fit across joint bins of (1) pre-reform retained earnings scaled by total assets, (2) pre-reform cash scaled by total assets, and (3) firm age. The sample is restricted to publicly listed firms.

Figure 9: Tax Effects on *log*(Investment) and Equity Issuances by IPO year



Notes: Panel A in this figure shows the coefficients on $Treated \times Time$ for log(Investment), as defined in equation (6), separately for the relevant sample among the treated firms that went public before 2015 and for the relevant sample among the treated firms that went public after 2015, relative to the baseline control group, as defined in Section 5.5. The dark navy line indicates the estimates for firms that went public after 2015, and the red line indicates the estimates for firms that went public before 2015. The dashed lines indicate the 95% confidence intervals for those coefficient estimates. The solid vertical line indicates the reform year. Panel B shows the coefficients on $Treated \times Time$ for equity issuances, separately for the relevant sample among the treated firms that went public after 2015 and for the relevant sample among the treated firms that went public before 2015, relative to the same baseline control group. The sample is restricted to publicly listed firms.

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 millions)	65.66	196.1	68.22	233.7	64.06	171.2
	(42.05)	(141.6)	(44.42)	(152.8)	(40.42)	(127.8)
Labor (Average Employee)	238.0	311.7	223.6	408.9	247.0	247.2
	(165.2)	(250.4)	(129.8)	(266.6)	(183.4)	(216.1)
Total Asset (in millions)	66.20	203.5	88.29	264.3	52.36	163.2
	(52.60)	(167.9)	(63.40)	(175.1)	(38.54)	(149.9)
Total Capital (in millions)	35.41	103.9	53.96	148.4	23.80	74.47
	(36.93)	(101.9)	(45.10)	(111.5)	(24.37)	(82.73)
CAPEX (in millions)	2.913	7.104	3.786	9.688	2.367	5.392
	(3.782)	(9.492)	(4.707)	(11.06)	(2.936)	(7.833)
CAPEX / lagged PPE	0.228	0.187	0.239	0.176	0.221	0.194
	(0.259)	(0.214)	(0.265)	(0.183)	(0.256)	(0.233)
Firm Age	22.01	26.31	19.67	31.37	23.48	22.96
	(12.80)	(14.50)	(11.36)	(15.78)	(13.42)	(12.51)
Observations	4966	13835	1912	5515	3054	8320

Notes: Sample years include 2009 – 2019. Labor is the average number of employees used in a given year. CAPEX is expenditures on physical capital assets, such as plant, 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) log(CAPEX)	(2) Equity Issuance	(3) Dividend Payouts	(4) Share Buybacks
Treated x Post	0.342	0.085	0.015	0.019
	(0.082)	(0.011)	(0.021)	(0.013)
Time and Firm FE	Yes	Yes	Yes	Yes
Pre-reform Treated Mean	14.224	0.035	0.092	0.020
Implied Elasticity wrt (1-tau)	1.99	14.21	0.96	5.66
R-squared	0.65	0.32	0.22	0.16
Observations (firm-years)	7105	6776	7125	7125
Clusters (Treated Firms)	187	187	187	187
Cluster (Control Firms)	521	521	521	521

Notes: This table reports the tax effects on investment and capital structure based on specification (7). The dummy for $Treated_i$ equals 1 if a firm i had a tax reduction, 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. Dividend payouts and share repurchases are scaled by lagged current profits. Each time period is a year, and the sample period is from 2009 to 2019. 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.

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

	Listed Firms	Private Firms	Listed and Private Firms	
	(1)	(2)	(3)	(4)
	log(CAPEX)	log(CAPEX)	log(CAPEX)	Pr(Going Public)
Treated x Post	0.342	0.208	0.256	0.101
	(0.082)	(0.067)	(0.052)	(0.013)
Time and Firm FE	Yes	Yes	Yes	Yes
Pre-reform Treated Mean	14.224	13.749	13.907	0.144
Implied Elasticity wrt (1-tau)	1.99	1.21	1.49	4.09
R-squared	0.65	0.70	0.71	0.92
Observations (firm-years)	7105	12252	19357	20164
Clusters (Treated Firms)	187	370	557	557
Clusters (Control Firms)	521	1028	1549	1549

Notes: This table reports the tax effects on investment based on specification (7). The dummy for $Treated_i$ equals 1 if a firm i had a tax reduction, 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. Each time period is a year, and the sample period is from 2009 to 2019. The sample includes both publicly listed 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.

Table 4: Results on Stock Prices (Publicly Listed Firm)

	Stock Prices	
	(1)	(2)
	log(Price)	log(Price)
Treated x Post	0.117	0.134
	(0.050)	(0.050)
Time and Firm FE	Yes	Yes
Pre-reform Treated Mean	1.71	1.71
Implied Elasticity wrt (1-tau)	0.68	0.78
R-squared	0.94	0.95
Observations (firm-months)	13885	13823
Clusters (Treated Firms)	89	89
Clusters (Control Firms)	500	500

Notes: This table reports the tax effects on stock prices based on the variation of specification (7). The dummy for $Treated_i$ equals 1 if a firm i had a tax reduction, 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. The sample is restricted to a balanced panel of publicly listed companies, where I observe their stock prices monthly. All specifications include time and firm fixed effects (FE). Column (2) includes basic controls (quartics in firm age and industry dummies interacted with time dummies) and additional controls (pre-reform operating profit quintile interacted with time dummies). The standard errors are clustered at the firm level and are reported in parentheses.

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

	Investment	Capital Structure			
	(1) log(CAPEX)	(2) Equity Issuance	(3) Dividend Payouts	(4) Share Buybacks	(5) Pr(Going Public)
Treated x Post	0.498	0.107	0.014	0.015	0.296
	(0.116)	(0.018)	(0.025)	(0.013)	(0.041)
Treated x Post x Cash-Rich	-0.316	-0.047	0.005	0.012	-0.003
	(0.164)	(0.022)	(0.042)	(0.026)	(0.061)
Time and Firm FE	Yes	Yes	Yes	Yes	Yes
Pre-reform Treated Mean (CR=0)	14.234	0.048	0.050	0.015	0.415
Implied Elasticity wrt (1-tau) (CR=0)	2.91	13.03	1.65	5.92	4.17
Pre-reform Treated Mean (CR=1)	14.213	0.021	0.139	0.026	0.474
Implied Elasticity wrt (1-tau) (CR=1)	1.06	16.94	0.79	6.03	3.61
R-squared	0.65	0.32	0.22	0.16	0.72
Observations (firm-years)	7105	6776	7125	7125	7125
Clusters (Cash-contrained Treated Firms)	102	102	102	102	102
Clusters (Cash-contrained Control Firms)	251	251	251	251	251
Clusters (Cash-rich Treated Firms)	85	85	85	85	85
Clusters (Cash-rich Control Firms)	270	270	270	270	270

Notes: This table reports the tax effects on investment and capital structure based on specification (11). The dummy for $Treated_i$ equals 1 if a firm i had a tax reduction, 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 5.5. 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. Dividend payouts and share repurchases are scaled by lagged current profits. Each time period is a year, and the sample period is from 2009 to 2019. 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.

Table 6: Results on Investment and Equity Issuances by IPO Year (Listed Firms)

	IPO before 2015		IPO after 2015	
	(1) log(CAPEX)	(2) Equity Issuance	(3) log(CAPEX)	(4) Equity Issuance
Treated x Post	0.241	0.055	0.527	0.145
	(0.099)	(0.014)	(0.116)	(0.020)
Time and Firm FE	Yes	Yes	Yes	Yes
Pre-reform Treated Mean	14.427	0.042	13.887	0.025
Implied Elasticity wrt (1-tau)	1.41	7.64	3.07	34.10
R-squared	0.64	0.28	0.65	0.32
Observations (firm-years)	6375	6086	5999	5728
Clusters (Treated Firms)	104	104	83	83
Clusters (Control Firms)	521	521	521	521

Notes: This table reports the tax effects on investment and capital structure based on specification (7). The dummy for $Treated_i$ equals 1 if a firm i had a tax reduction, 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). Columns (1) and (2) show the results for treated firms that went public before 2015, relative to the baseline control group, as explained in Section 5.5. Columns (3) and (4) show the results for treated firms that went public after 2015, relative to the same baseline control group. 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. Each time period is a year, and the sample period is from 2009 to 2019. 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.

ONLINE APPENDIX:

Capital Gains Taxes and Real Corporate Investment: Evidence from Korea

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A Institutional Details

In Appendix A, I provide further institutional details regarding corporate income tax and payout tax systems and firm size regulations in Korea. In Appendix A.1, I describe historical corporate income tax rates 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 that small firms are eligible to claim and show a set of tests to argue that my main results are not driven by these extraneous benefits. In Appendix A.4, I provide additional details on the capital gains tax system in Korea and compute investment elasticities with respect to the net of capital gains tax rates under alternative assumptions. More details on the historical capital gains tax rates in Korea can be found on this website: www.nts.go.kr/eng.

A.1 Corporate Income and Dividend Tax System in Korea

A.1.1 Corporate Income Tax Rates on Profits

In Korea, the marginal corporate income tax rate was 11% for profits below $200,000, and 22% for profits above $200,000 in 2009. From 2010 to 2011, the marginal tax rate for profits below $200,000 decreased to 10%. From 2012 to 2017, the government added a third profit threshold of $20 million, reduced the marginal tax rate in the middle category to 20%, and kept the top marginal tax rate at 22%. From 2018, the government added a fourth profit threshold of $300 million with the marginal tax rate of 25%. Although there were changes in corporate income tax rates across time in Korea, I find that the change in effective corporate income tax rates for treated firms, relative to control firms, was zero after the reform. This implies that controlling for these small changes in corporate income tax rates does not affect the main results (See Appendix A.3).

A.1.2 Dividend Tax Rates

In Korea, dividends are taxed similarly to individual income. If an investor's dividend income in a given year is less than $20,000, then the investor faces a flat tax rate of 15.4%. If the dividend income is above $20,000, then it becomes part of the investor's personal income, and the marginal tax rate can go up to 42%, depending on the investor's total income in a given year. From 2005 and 2011, the top marginal dividend tax rate was 35%, and increased to 38% in 2012, and to 42% in 2018.

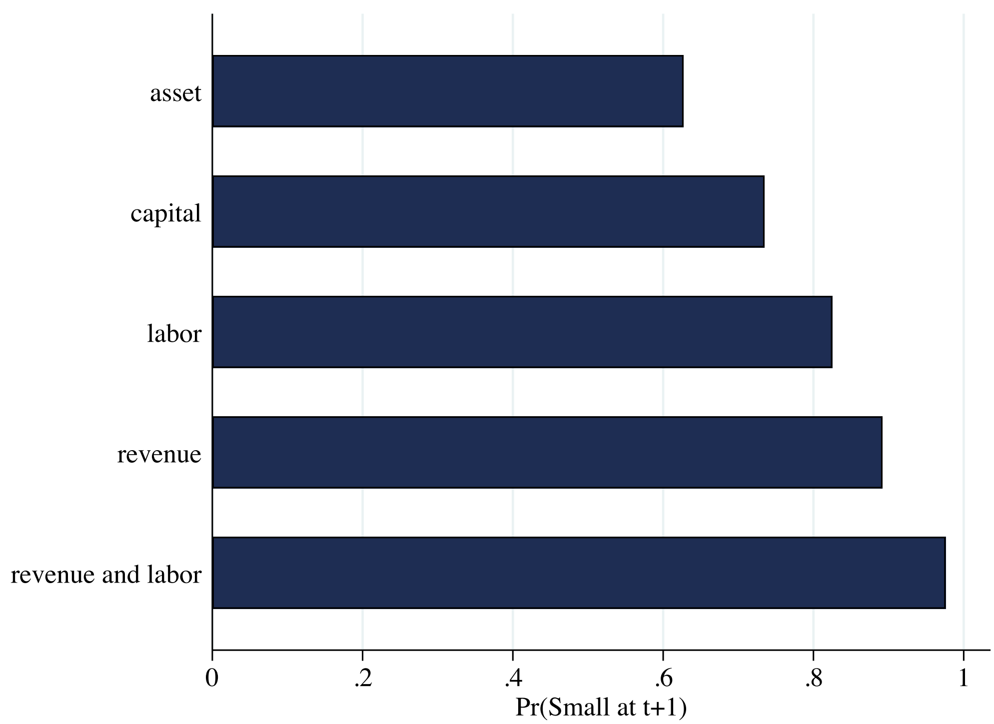
A.2 Determinants of Firm Size, Conditional Density, and the Reform

Table A.1: Conditional Probability Matrix for each running variable (Pre-reform)

	Binding		Less Binding	
	(1)	(2)	(3)	(4)
	Below Revenue Cutoff	Below Labor Cutoff	Below Total Capital Cutoff	Below Asset Cutoff
Below Revenue Cutoff	1	0.845	0.799	0.703
	(0)	(0.361)	(0.401)	(0.457)
Below Labor Cutoff	0.914	1	0.832	0.758
	(0.281)	(0)	(0.374)	(0.428)
Below Total Capital Cutoff	0.969	0.934	1	0.852
	(0.174)	(0.248)	(0)	(0.355)
Below Asset Cutoff	0.999	0.997	0.998	1
	(0.0352)	(0.0570)	(0.0424)	(0)
Observations	10509	11359	12749	14940

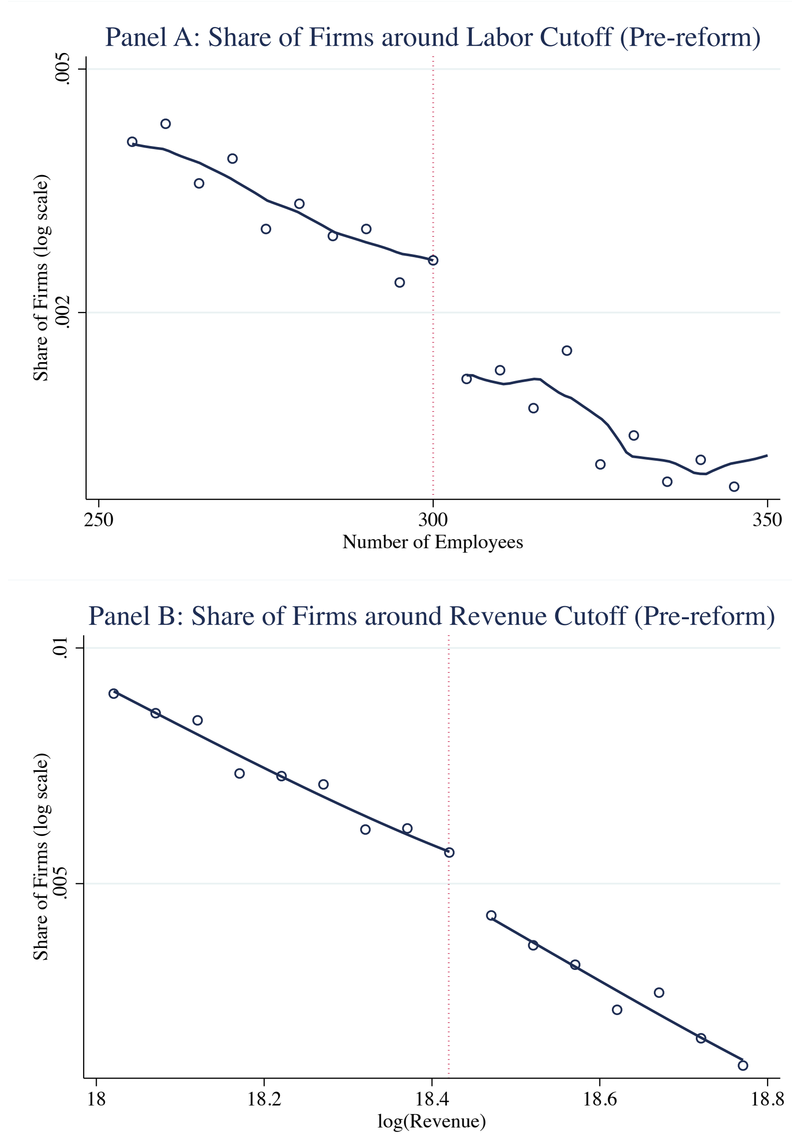
Notes: This table reports the conditional probability matrix for each of the running variables that determined firm size prior to the 2014 reform. Each row represents the probability of a running variable being below its own threshold conditional on the other running variable is either below or above its own threshold. For example, the cell in column 1 and row 2 represents the conditional probability that a firm is below the labor cutoff, given that the firm is already below the revenue threshold. On average, 91.4% 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 revenue and the average number of employees.

Figure A.1: Determinants of Firm Size (Pre-reform)



Notes: This figure shows the probability that a given firm becomes or remains small in the next period conditional that the firm is below each threshold in the current period. The x-axis represents the probability that a firm is small in the next period. The y-axis represents that the firm is below each threshold in the current period.

Figure A.2: Firm Density around Firm-size Cutoffs (Pre-reform)



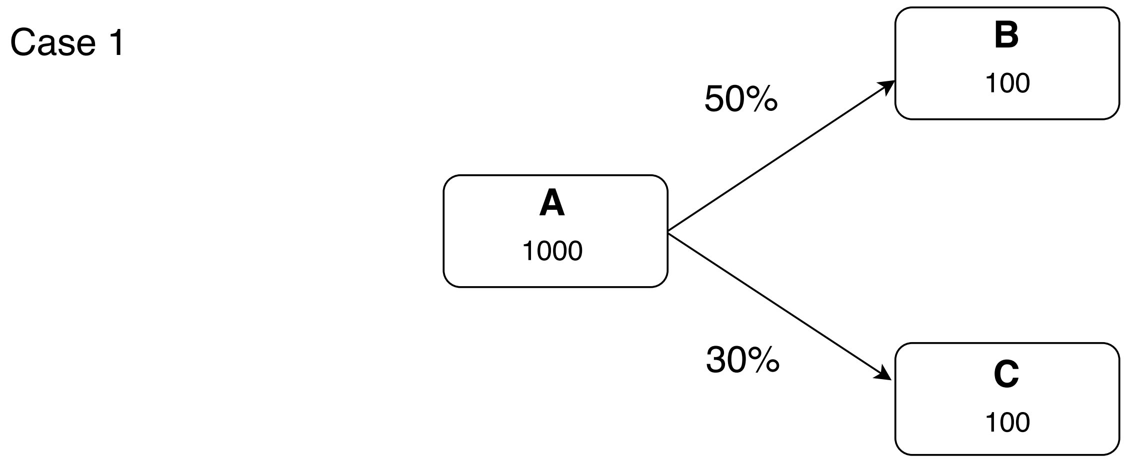
Notes: Panel A in this figure shows the firm density around the labor cutoff, conditional that the firms are jointly below the other thresholds (revenue, total capital, and asset). The cutoff is at the average number of employees of 300, and the bin size is 5 average number of employees. The hollow dots 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 around the revenue cutoff, conditional that the firms are jointly below the other thresholds (labor, total capital, and asset). The cutoff is at the total revenue of 100 million dollars, and the bin size is 5 log points in revenues. The McCrary (2008) test rejects the null that the coefficient at the jump is statistically not different from zero.

Table A.2: Computing Accounting Variables for Tax Purposes

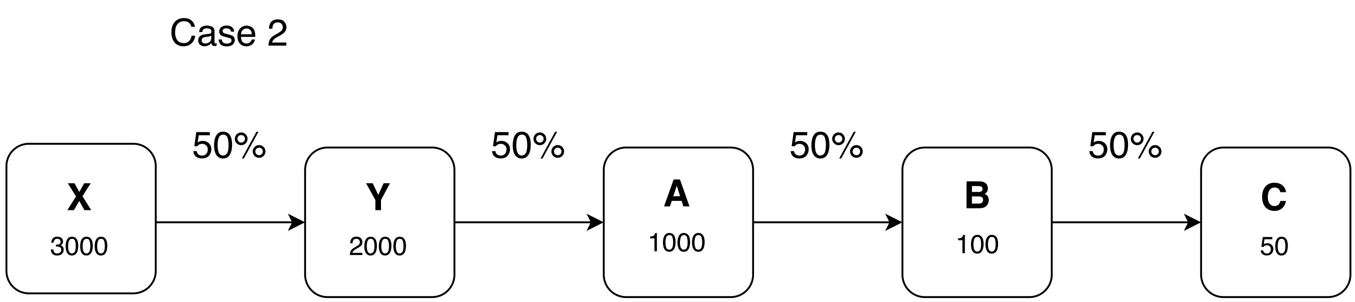
Firm	Relationship	Labor	Ownership	Labor Size for Tax Purposes
Case 1				
A	Parent to B & C	1000	-	$1000 + (1.0) * 100 + (0.3) * 100 = 1130$
B	Subsidiary to A	100	50%	$100 + (1.0) * 1000 = 1100$
C	Subsidiary to A	100	30%	$100 + (0.3) * 1000 = 400$
Case 2				
X	Parent to Y	3000	-	$3000 + (1.0) * 2000 + (0.5) * 1000 = 5500$
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, firm A is the parent company with two subsidiaries, namely B and C. Assume that each of the subsidiary does not own any other subsidiaries (if it does, then it will just become a part of the parent firm's subsidiary). The column, "Labor," denotes the average number of employees in a given year. Given the rules described in Section 2, each firm's labor size 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 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, 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. The same rules apply when computing the values for other accounting variables (total revenue, total capital, and total asset).

Figure A.3: Computing Accounting Values



Notes: This figure shows how to compute accounting values for firms in a case where firm A owns 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 number of employees of 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



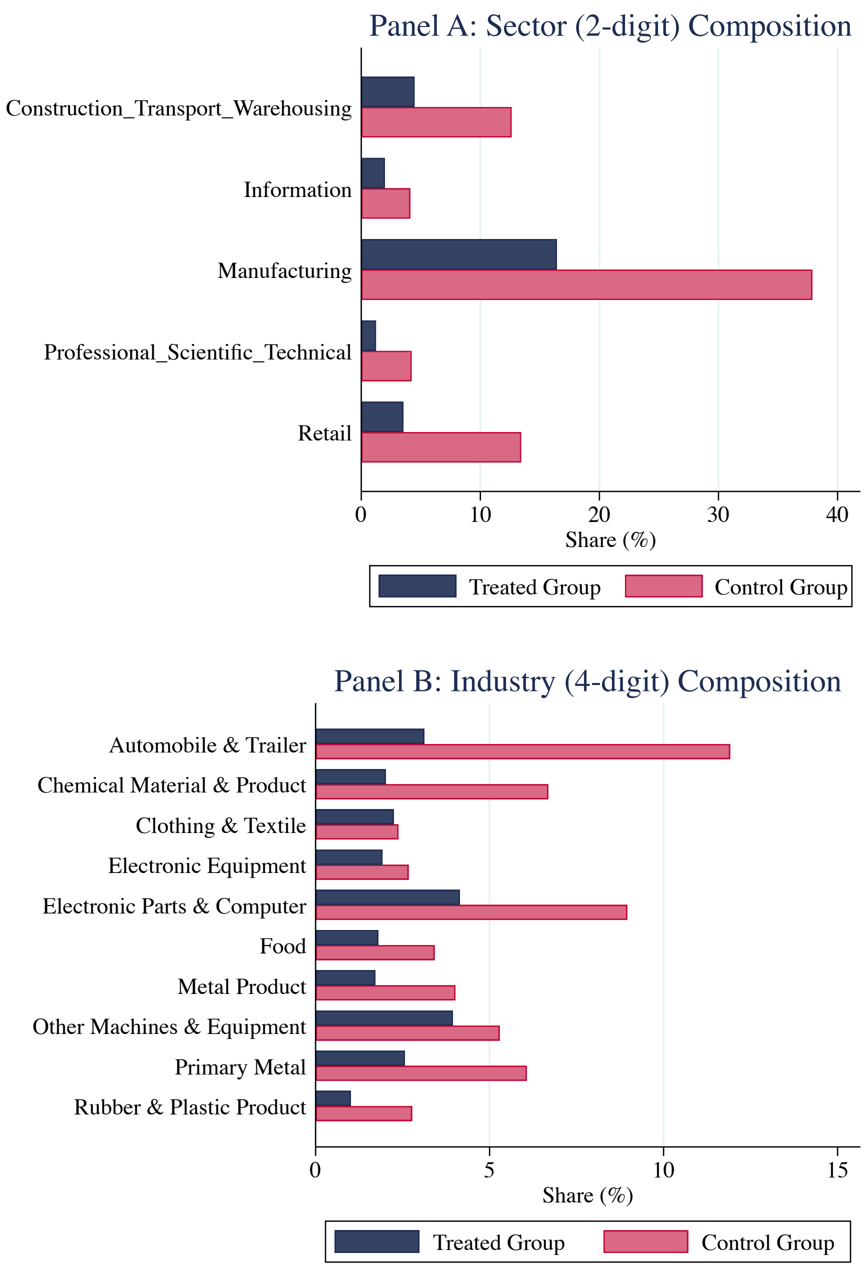
Notes: This figure shows how to compute accounting values for firms in a case where firm X owns 50% of Y, which owns 50% of A, which owns 50% of B, which owns 50% of C. Suppose that there's no other subsidiary. Also, suppose that in a given year, firm X, Y, A, B, and C used 3000, 2000, 1000, 100, and 50 employees on average, respectively. The government computes the average number of employees of 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: $50 + (0.5) * 1000 + (1.0) * 100 = 650$

Table A.3: New Threshold Across Industries

Sector	Industry Name	4-digit SIC	Average 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 Equipment	3290	100 million
Manufacturing	Automobile and Trailer	3300	100 million
Manufacturing	Other Transportation Equipment	3310	100 million

Notes: This table describes how the reform in 2014 affected firms in different industries within the manufacturing sector. Even though firms across all sectors had their labor threshold removed for firm size requirements and changed the revenue threshold into the 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 the 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. Moreover, even though the average revenue threshold did not increase to 150 million dollars for construction or information and production 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.

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



Notes: Panel A in this figure shows the sector composition of firms by treated and control groups. Panel B in this figure shows the industry composition of firms by treated and control groups, for the top 10 industries (by share based on the number of observations) within the manufacturing sector.

A.3 Additional Tax Benefits for Small Firms

This subsection describes a set of tax benefits that a small firm is eligible to claim, other than the lower capital gains tax rate. I first describe each of these additional benefits, and then show related tests to argue that the eligibility to claim these additional benefits was 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 account for these additional tax benefits, I take two approaches. First, I directly control for measures that could have affected firms' cost of capital in my estimation by comparing firms more likely to claim the additional tax benefits with firms ineligible (or less likely) to claim those benefits. Second, I compute the change in effective corporate income tax rates for the affected firms to check whether there was a reduction in the cost of capital from a decrease in corporate income tax rates. I directly observe how much corporate taxes each firm actually paid in the data, as well as each firm's earnings before income taxes (EBIT). I then compute the change in the effective corporate income tax rates based on the information available in the accounting data. Note that small firms can apply only for one of the benefits described below. For example, if a firm can deduct corporate taxes since it's located in a rural area, then it cannot apply for R&D tax credits.

A.3.1 Small Firms in Rural Areas

Small firms in my analysis sample can deduct up to 15% of their corporate taxes if they are physically located in rural areas.[17] For example, if a small firm located in a rural region generates 1 million dollars in profits and faces a marginal corporate income tax rate of 10% (up to $200,000) and 20% (above $200,000), then it can deduct up to 15% of the tax burden (which amounts to $180,000), so the effective tax rate would decrease from 18% to 15.3%, holding everything else constant. Therefore, the amount of reduction in corporate tax burdens depends on profits in a given year for small firms located in rural areas.[18] If this additional corporate tax benefit provides investment incentives for small firms, then I would expect to see an even 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 outcome, cutting the sample by whether or not firms are located in rural areas.

Column (1) of Table A.4 shows the main result, where I interact the dummy for $Treated \times Post$ with the dummy for whether a firm is located in a rural area (which is fixed at the time of the reform and accounts for roughly 36% of the analysis sample). The difference-in-differences coefficient is positive and statistically significant, implying that affected firms in urban areas increase investment following the reform. The triple difference coefficient is statistically insignificant, suggesting that affected firms in rural areas did not increase investment more relative to those in urban areas, even

[17] In Korea, rural areas are defined as cities or provinces other than Seoul, Incheon, and Gyeong-Gi Province. In my sample, roughly 36% of firms are located in rural areas.

[18] A newly established small firm located in a rural area is eligible to deduct up to 50% of its corporate taxes for the first 5 years since its establishment. While this is a substantial corporate tax incentive, there are only a few firms (1% of treated firms and 0.7% of control firms) in my analysis sample that were eligible for this tax benefit. Therefore, I find that whether or not I drop these firms eligible for extra tax credits does not quantitatively affect my main results.

if there were additional tax benefits to do so. Therefore, the fact that there was an additional benefit for small firms in rural areas does not seem to suggest that my main estimates are driven by this additional tax benefit.

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.[19] 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; therefore, its effective tax rate decreases from 18% to 15.5%, holding everything else constant. 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, and 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 the main outcome, cutting the sample by firms' R&D expenditures.

Column (2) of Table A.4 shows the main result in Section 5, where I interact the dummy for $Treated \times Post$ with the dummy for whether a firms is R&D intensive (which is defined as 1 if firms' expenditures on R&D, scaled by revenue, are above the median and is fixed at the time of the reform). 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. The triple difference coefficient is statistically insignificant and negative, suggesting that affected firms that were more R&D intensive did not increase investment more, even if there was an additional tax benefit 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 driven by this additional tax benefit.[20]

A.3.3 Investment Tax Credits for Small Firms

Small firms are eligible for the following investment tax credits: (1) Small firms are eligible to deduct 3% of expenditures on physical capital, such as machines, equipment, and operating systems, from corporate taxes (except for used or leased items), (2) Small (large) firms are eligible to deduct 7% (5%) of expenditures on productivity-enhancing facilities (i.e., automated systems, new technologies, software, etc.) from corporate taxes, (3) Small (large) firms are eligible to deduct 6% (3%) of expenditures on energy-saving facilities (i.e., energy efficient system, water saving technology, renewable energy, etc.) from corporate taxes, and (4) Small (large) firms are eligible to

[19] 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.

[20] 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, despite that the additional corporate tax incentives to do so. R&D intensive firms, however, may find the tax difference small or their R&D spending as a partial substitute for capital investment. For example, this result is consistent with the increase in R&D expenditures among treated firms after the reform (see Appendix E). Therefore, R&D intensive firms may not increase investment in physical capital more compared to less R&D intensive firms after a capital gains tax cut even though there is an additional corporate tax benefit.

deduct 10% (5%) of expenditures on eco-friendly facilities (i.e., less pollution or using alternative resources, etc.) from corporate taxes. Note that a small firm is eligible to benefit from only one of the investment tax credits – for example, a firm cannot claim tax deductions from both investment on physical capital and investment on eco-friendly facilities. Furthermore, large firms (control group) were also eligible to claim these additional tax benefits (at a smaller rate), so the change in effective corporate income tax rates after the reform might be small relative to the control group based on these investment tax credits.

A.3.4 Changes in Effective Corporate Income Tax Rates

I compute the changes in effective corporate income tax rates accounting for these various tax incentives eligible for small firms by using accounting variables observed in the data. Table A.5 shows the difference-in-differences estimate of the reform's effects on effective corporate income tax rates. The coefficient estimate is zero, which implies that the treated firms did not experience a change in effective corporate income tax rates relative to the control group after the reform on average. This result implies that although there were additional tax benefits that affected firms were eligible to claim after the reform, those extra incentives were not the main driver of the investment response following a capital gains tax cut.[21]

[21] There can be a number of reasons why there was no change in effective corporate income tax rates for treated firms after the reform, relative to the control group, despite there being several corporate tax incentives for small firms. First, treated firms might have experienced a large increase in earnings after the reform, which would increase their corporate tax burdens more relative to the deductions that they can claim. Second, there might be unobserved firm specific ways that treated firms could deduct taxes on corporate income, other than through being reclassified as small firms. Third, given that treated firms can claim only one out of several eligible benefits, and large firms (control group) can also claim some of those benefits at a lower rate, the actual change in effective corporate income tax rates for treated firms after the reform, relative to control firms, might be small.

Table A.4: Main Results by Firms' Location and R&D Intensity

	By Firm Location	By RnD Intensity
	(1)	(2)
	log(CAPEX)	log(CAPEX)
Treated x Post	0.326	0.362
	(0.101)	(0.103)
Treated x Post x Benefit	0.045	-0.101
	(0.171)	(0.170)
Time and Firm FE	Yes	Yes
Pre-reform Treated Mean (Benefit=0)	14.097	14.027
Implied Elasticity wrt (1-tau) (Benefit=0)	1.90	2.11
Pre-reform Treated Mean (Benefit=1)	14.53	14.64
Implied Elasticity wrt (1-tau) (Benefit=1)	2.17	1.52
R-squared	0.65	0.65
Observations (firm-years)	7105	7105
Clusters (Less Benefit Treated firms)	136	140
Clusters (Less Benefit Control Firms)	317	301
Clusters (More Benefit Treated Firms)	51	47
Clusters (More Benefit Control Firms)	204	220

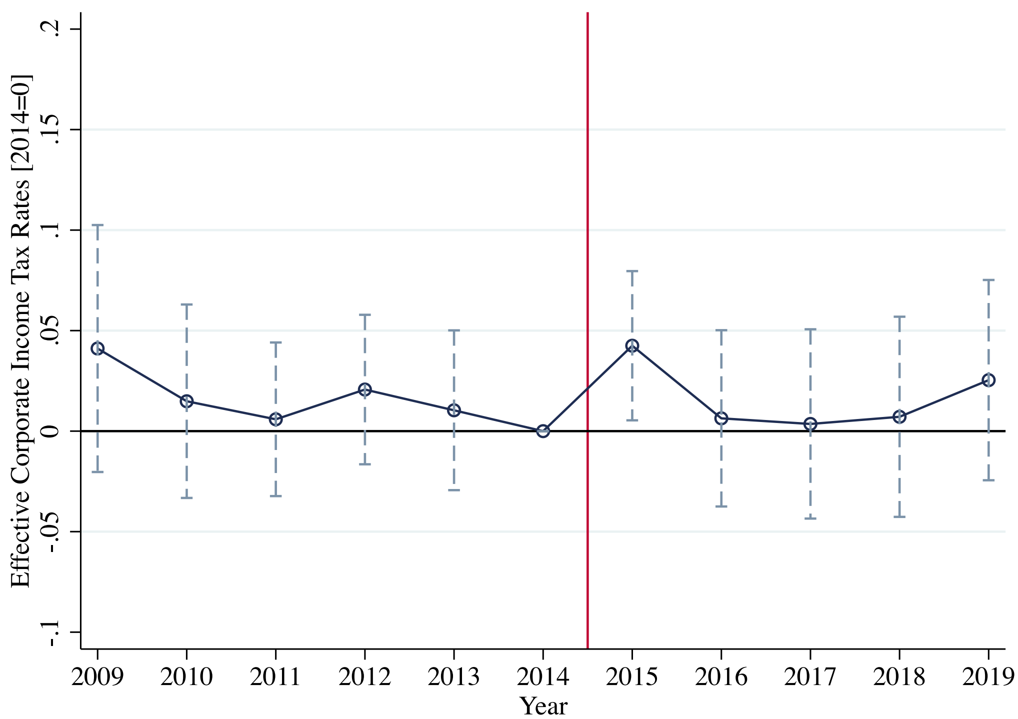
Notes: This table reports the tax effects on the main corporate outcome based on the triple difference estimation. The dummy for $Treated_i$ equals 1 if a firm i had a tax reduction, as explained in Section 4. The dummy for $post_t$ equals 1 if the time period is after the end of the reform year (2014). In Column (1), the dummy for "Benefit" is defined as 1 if firms are located in rural areas (defined in Section A.3). In Column (2), the dummy for "Benefit" is defined as 1 if firms' research and development expenditures are above the median (as defined in Appendix A.3). Investment is defined as log of expenditures on physical capital assets. Each time period is a year, and the sample period is from 2009 to 2019. 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.

Table A.5: Changes in Effective Corporate Income Tax Rates

	Listed and Private Firms	Listed Firms	Private Firms
	(1)	(2)	(3)
	CIT	CIT	CIT
Treated x Post	0.003	-0.001	0.006
	(0.011)	(0.020)	(0.012)
Time and Firm FE	Yes	Yes	Yes
Pre-reform Treated Mean	0.116	0.079	0.134
R-squared	0.18	0.16	0.21
Observations (firm-years)	20164	7125	13039
Clusters (Treated Firms)	557	187	370
Clusters (Control Firms)	1549	521	1028

Notes: This table reports the reform's effects on effective corporate income tax rates based on the difference-in-differences estimation. The dummy for $post_t$ equals 1 if the time period is after the end of the reform year (2014). An effective corporate income tax rate (CIT) is defined as the total corporate income taxes paid divided by earnings before income taxes (EBIT) in each period. Each time period is a year, and the sample period is from 2009 to 2019. The sample includes both publicly listed and private companies. All specifications include time and firm fixed effects (FE). The standard errors are clustered at the firm level and are reported in parentheses.

Figure A.5: Reform Effects on Effective Corporate Income Tax Rates



Notes: The dark solid line in this figure indicates the coefficients on *Treated* $\times$ *Time*, as in equation (6), for firms' effective corporate income tax rates. The vertical dashed lines indicate 95% confidence intervals. The solid vertical line indicates the reform year.

A.4 Additional Details on Capital Gains Tax Rates

This subsection describes additional details on capital gains tax rates for different types of investors in Korea during my sample period. The rules depend on (1) whether the investor is a "large" (as defined below) shareholder, (2) firm size, and (3) whether the firm is publicly listed or private.

A.4.1 For Listed Firms

In Korea, small shareholders are exempt from paying taxes on realized gains from selling stocks of publicly listed companies (except those sold at private (over-the-counter) markets). By contrast, large shareholders have to pay taxes on realized gains from selling stock of listed firms.

The definition of large shareholders changed more than once during the sample period. From 2009 to 2012, a large shareholder was defined as anyone who owned at least 3% (or worth at least 10 million dollars in market value) of the firm's stock listed under the Korea Composite Stock Price Index (KOSPI). For shareholders whose firms were listed under the Korean Securities Dealers Automated Quotations (KOSDAQ), the threshold was 5% (or 5 million dollars in market value). From 2013 to 2015, the threshold for KOSPI firms was 2% (or 5 million dollars in market value) and the threshold for KOSDAQ firms was 4% (or 4 million dollars in market value). From 2016 to 2019, the threshold for KOSPI firms was 1% (or 2.5 million dollars in market value from 2016 to 2017, and 1.5 million dollars from 2018 to 2019) and the threshold for KOSDAQ firms was 2% (or 2 million dollars in market value from 2016 to 2017 and 1.5 million dollars from 2018 to 2019). Therefore, the definition of large shareholders changed in a way intended to include more shareholders that have to pay capital gains taxes.[22]

From 2009 to the end of 2015, large shareholders in small firms had to pay 10% on their realized gains when selling their stock, while large shareholders in large firms had to pay 30% on their (short-term) realized gains when they sold their stocks within a year, and 20% otherwise. From 2016 to 2017, large shareholders in small firms had to pay 20% on their realized gains, while the tax rates for large shareholders in large firms did not change. From 2018, large shareholders in large firms face a marginal capital gains tax rate of 25% on long-term realized gains worth at least 300,000 dollars, while the short-term capital gains tax rate still remains at 30%.

In the data, I observe the ownership rates (aggregated at the firm-level, rather than at the investor-level) for the largest shareholders, board of directors, managers, auditors, and shareholders with at least 5% ownership. Among the listed firms within my treated group, the combined ownership rate for these large shareholders was about 65% before the reform and 66% after the reform on average. Among the listed firms within my control group, the combined ownership rate for the large shareholders was about 67% before the reform, and 65% after the reform on average. This implies that large shareholders account for at least more than a half of the ownership rate among listed firms in both treated and control groups after the reform on average. Since the definition of large shareholders became more inclusive throughout my sample period, more shareholders likely became large shareholders who were not included in the data.

[22] I account for these changes in the share of large shareholders for the treated and control firms when I estimate the change in effective capital gains tax rates, and compute the investment elasticity with respect to the net of capital gains tax rates.

Based on the observable ownership information, I derive an upper bound on the change in effective capital gains tax rates for the treated firms by assuming that all large shareholders are short-term investors who sell their stocks within a year. Column (1) of Table A.6 shows the change in the effective capital gains tax rate based on this assumption, which indicates that the tax rate decreased by roughly 10 percentage points after the reform on average from the base rate of 23.24 percent. Therefore, this gives us a lower bound on the investment elasticity with respect to the net of tax rate of 2.6.

Additionally, I derive a lower bound on the change in effective capital gains tax rates for the treated firms by assuming that all large shareholders are long-term investors who sell their stocks after one year. Column (2) of Table A.6 shows the change in the effective capital gains tax rate based on this assumption, which indicates that the tax rate decreased by roughly 4.8 percentage points after the reform on average, from the base rate of 16.77 percent. Therefore, this gives us an upper bound on the investment elasticity with respect to the net of tax rate of 5.9.

Importantly, the share of large shareholders among the listed firms is likely underestimated in my data set, given that there are potentially many more large shareholders who are not in the data because either they are not important personnel of the firms or own less than 5% (but above the threshold). As the share of large shareholders increases, the change in short-term effective capital gains tax rates would be higher, which would give us a lower investment elasticity with respect to the net of tax rates. For example, if I assume that all investors within the listed firms are large shareholders who sell their stocks within a year, the change in effective capital gains tax rates for treated firms is 12 percentage points on average, from the base rate of 30 percent, which yields an investment elasticity with respect to the net of tax rate of 1.99.[23] To summarize, if the share of large shareholders was higher, then the estimate on the investment elasticity with respect to the net of (short-term) capital gains tax rate would be lower. Since the government lowered the threshold for defining large shareholders after the reform, it is likely that the share of large shareholders increased, which might yield an estimate of the investment elasticity closer to the lower bound.

A.4.2 For Private Firms

All investors (except foreigners) have to pay taxes on realized gains from selling stock of private firms. The tax rates depend on firm size and whether the investor is a large shareholder. Throughout my sample period, small shareholders in small firms have to pay 10% on their realized gains when selling their stock, while small shareholders in large firms face a 20% tax rate.

From 2009 to the end of 2015, large shareholders in small firms had to pay 10% on their realized gains when selling their stock, while large shareholders in large firms had to pay 30% on their (short-term) realized gains when they sold their stocks within a year, and 20% otherwise. From 2016 to 2017, large shareholders in small firms had to pay 20% on their realized gains, while the tax rates for large shareholders in large firms did not change. From 2018, large shareholders in large firms face a marginal capital gains tax rate of 25% on long-term realized gains worth at least 300,000 dollars, while the short-term capital gains tax rate still remains at 30%.

[23] By contrast, if I assume that all investors within the listed firms are large shareholders who sell their stocks after a year, then the change in effective capital gains tax rates for treated firms is 4 percentage points, from the base rate of 22 percent, which yields an investment elasticity of 6.67.

The definition of large shareholders changed more than once during the sample period. From 2009 to 2012, a large shareholder was defined as anyone who owns at least 3% (or worth at least 10 million dollars in market value) of the firm's stock. From 2013 to 2015, the threshold was 2% (or 5 million dollars in market value). From 2016 to 2019, the threshold was 4% (or 2.5 million dollars in market value from 2016 to 2017 and 1.5 million dollars from 2018 to 2019).

While I do not have data on ownership rates among private firms, I can make the following assumption to derive lower and upper bounds on the change in capital gains tax rates for the treated firms. I assume that the total ownership rate of large shareholders in private firms is the same as the listed firms (65% before the reform and 66% after the reform for the treated group, and 67% before the reform and 65% after the reform for the control group on average) and assuming that private firms' ownership structure is at least as concentrated.

Then I derive an upper bound on the change in effective capital gains tax rates for the treated firms by assuming that all large shareholders are short-term investors who sell their stocks within a year. Column (3) of Table A.6 shows the change in the effective capital gains tax rate based on this assumption, which indicates that the tax rate decreased by roughly 9.8 percentage points after the reform on average from the base rate of 26.5 percent. Therefore, this gives us a lower bound on the investment elasticity with respect to the net of tax rate of 1.56.

Additionally, I derive a lower bound on the change in effective capital gains tax rates for the treated firms by assuming that all large shareholders are long-term investors who sell their stocks after one year. Column (4) of Table A.6 shows the change in the effective capital gains tax rate based on this assumption, which indicates that the tax rate decreased by roughly 6 percentage points after the reform on average, from the base rate of 20 percent. Therefore, this gives us an upper bound on the investment elasticity with respect to the net of tax rate of 2.77.

Importantly, the portion of large shareholders among private firms was likely larger. As the share of large shareholders increases, the change in short-term effective capital gains tax rates would be higher, which would give us a lower investment elasticity with respect to the net of tax rates. For example, if I assume that all investors within the private firms are large shareholders who sell their stocks within a year, then the change in effective capital gains tax rates for treated firms is 12 percentage points on average, from the base rate of 30 percent, which yields an investment elasticity with respect to the net of tax rate of 1.2.[24] To summarize, if the share of large shareholders was higher, then the estimate on the investment elasticity with respect to the net of (short-term) capital gains tax rate would be lower. Since the government lowered the threshold for defining large shareholders after the reform, it is likely that the share of large shareholders increased, which might yield an estimate of the investment elasticity closer to the lower bound.

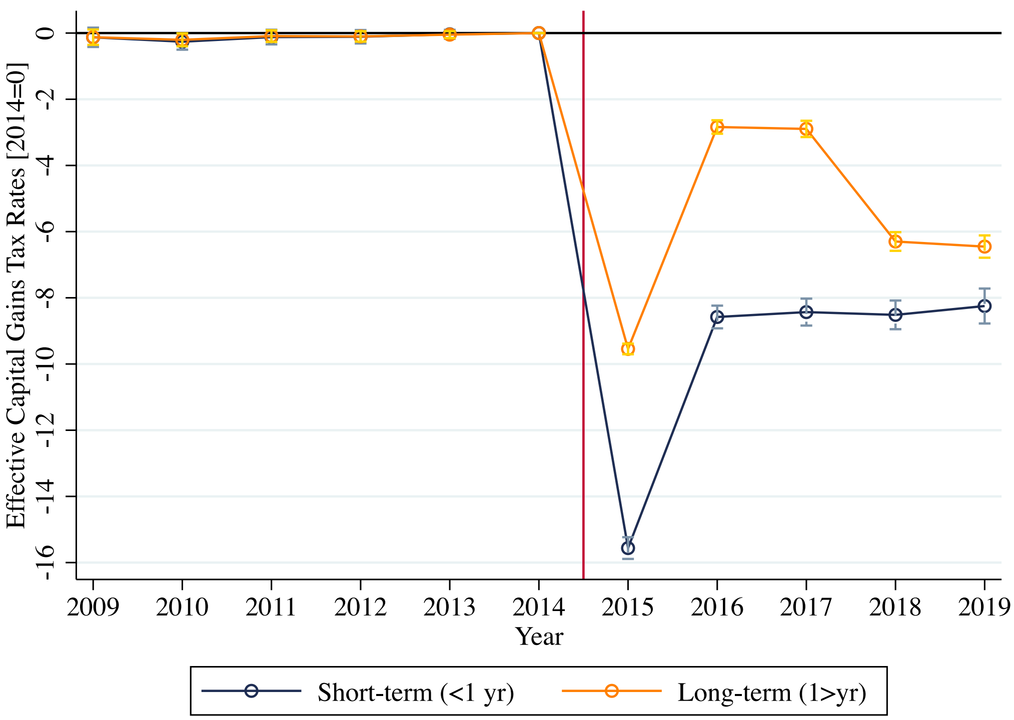
[24] By contrast, if I assume that all investors within the private firms are large shareholders who sell their stocks after a year, then the change in effective capital gains tax rates for treated firms is 4 percentage points, from the base rate of 22 percent, which yields an investment elasticity of 4.06.

Table A.6: Changes in Effective Capital Gains Tax Rates

	Listed Firms		Private Firms		Listed and Private Firms	
	(1) Short-term	(2) Long-term	(3) Short-term	(4) Long-term	(5) Short-term	(6) Long-term
Treated x Post	-9.965	-4.766	-9.786	-6.015	-9.886	-5.558
	(0.328)	(0.310)	(0.000)	(0.020)	(0.188)	(0.110)
Time and Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Pre-reform Treated Mean	23.24	16.77	26.45	20.00	25.41	18.95
R-squared	0.84	0.77	1.00	0.88	0.89	0.87
Observations (firm-years)	7125	7125	8727	13039	15852	20164
Clusters (Treated Firms)	187	187	370	370	557	557
Clusters (Control Firms)	521	521	1028	1028	1549	1549

Notes: This table reports the reform's effects on effective capital gains tax rates based on the difference-in-differences estimation. The dummy for $post_t$ equals 1 if the time period is after the end of the reform year (2014). Each time period is a year, and the sample period is from 2009 to 2019. The sample includes both publicly listed and private companies. All specifications include time and firm fixed effects (FE). The standard errors are clustered at the firm level and are reported in parentheses.

Figure A.6: Reform Effects on Effective Capital Gains Tax Rates



Notes: The dark solid line in this figure indicates the coefficients on $Treated \times Time$, as in equation (6), for firms' effective short-term capital gains tax rates. The orange solid line indicates the coefficients on $Treated \times Time$ for firms' effective long-term capital gains tax rates. The solid vertical line indicates the reform year. The sample includes both listed and private firms.

B Model Predictions

B.1 Model-based Predictions on Elasticities

In this subsection, I describe a static investment model to derive the investment elasticity with respect to the net of capital gains tax rate. Here, I assume that the marginal after-tax return on investment is greater than the interest rate, which is a key assumption behind the traditional view model.

Setup I start with a standard production function framework:

1. Output is given by $y = g(L, K) = AL^{\alpha_L}K^{\alpha_K}$, where $0 < \alpha_L + \alpha_K < 1$.
2. Investment at time t, I_t, evolves as follows: $I_t = K_t - (1-\delta)K_{t-1}$, where δ is the depreciation rate for capital. Thus, at the steady-state, $I = \delta K$.
3. Wage rate, w, for the cost of labor is exogenously given.
4. The cost of capital is $\frac{r}{(1-\tau_g)(1-\tau_c)}$, where r, τ_g, τ_c are the expected rate of return, capital gains tax rate, and corporate tax rate, as described in equation (12) in Section 6, with $\rho = 0$.
5. Investment is financed by either issuing new equity (E) or by using its existing cash (C) or both: $I = E + C$.

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

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

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

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

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

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

$MR(y;\epsilon) = \left(\frac{1}{\epsilon}+1\right) D y^{\frac{1}{\epsilon}}$. A profit-maximizing firm sets $MR(y;\epsilon) = MC(y; w, r, \tau_g, \tau_c)$:

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

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

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

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

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

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

A large decrease in τ_g from τ_g^0 to τ_g^* (or increase in the keep rate from $1-\tau_g^0$ to $1-\tau_g^*$) increases capital from K^0 to K^* as follows:

$$dK = \left[\left(\frac{1}{\epsilon}+1\right)^{\alpha_L+\alpha_K} AD^{\alpha_L+\alpha_K}\left(\frac{\alpha_L}{w}\right)^{\alpha_L}\left(\frac{\alpha_K(1-\tau_c)}{r}\right)^{\left(1-(\alpha_L+\alpha_K)(\frac{1}{\epsilon})-\alpha_L\right)}\right]^{\frac{1}{1-(\alpha_L+\alpha_K)\left(\frac{1}{\epsilon}+1\right)}}$$
$$\left(\left(1-\tau_g^*\right)^{\frac{\left(1-(\alpha_L+\alpha_K)(\frac{1}{\epsilon})-\alpha_L\right)}{1-(\alpha_L+\alpha_K)\left(\frac{1}{\epsilon}+1\right)}} - \left(1-\tau_g^0\right)^{\frac{\left(1-(\alpha_L+\alpha_K)(\frac{1}{\epsilon})-\alpha_L\right)}{1-(\alpha_L+\alpha_K)\left(\frac{1}{\epsilon}+1\right)}}\right)$$

$$\frac{dK/K^0}{(\tau_g^0-\tau_g^*)/(1-\tau_g^0)} = \left(\left(\frac{1-\tau_g^*}{1-\tau_g^0}\right)^{\frac{\left(1-(\alpha_L+\alpha_K)(\frac{1}{\epsilon})-\alpha_L\right)}{1-(\alpha_L+\alpha_K)\left(\frac{1}{\epsilon}+1\right)}} - 1\right) * \frac{1-\tau_g^0}{\tau_g^0-\tau_g^*}$$

Since $I = \delta K$ at the steady-state, we can also express the change in investment as follows:

$$\frac{dI/I^0}{(\tau_g^0 - \tau_g^*)/(1-\tau_g^0)} = \frac{1}{\delta}\frac{dK/K^0}{(\tau_g^0 - \tau_g^*)/(1-\tau_g^0)} = \frac{1}{\delta}\left(\left(\frac{1-\tau_g^*}{1-\tau_g^0}\right)^{\frac{\left(1-(\alpha_L+\alpha_K)(\frac{1}{\epsilon})-\alpha_L\right)}{1-(\alpha_L+\alpha_K)\left(\frac{1}{\epsilon}+1\right)}} - 1\right) * \frac{1-\tau_g^0}{\tau_g^0 - \tau_g^*}$$

Moreover, since investment is financed either by issuing new equity (E) or by using its retained earnings (C) (or by both), $\Delta I = \Delta E + \Delta C$. If we assume that investment is entirely financed by issuing new equity, then

$$\frac{dE/E^0}{(\tau_g^0 - \tau_g^*)/(1-\tau_g^0)} = \frac{dI/I^0}{(\tau_g^0 - \tau_g^*)/(1-\tau_g^0)} = \frac{1}{\delta}\left(\left(\frac{1-\tau_g^*}{1-\tau_g^0}\right)^{\frac{\left(1-(\alpha_L+\alpha_K)(\frac{1}{\epsilon})-\alpha_L\right)}{1-(\alpha_L+\alpha_K)\left(\frac{1}{\epsilon}+1\right)}} - 1\right) * \frac{1-\tau_g^0}{\tau_g^0 - \tau_g^*}$$

In practice, $\frac{dE/E^0}{(\tau_g^0 - \tau_g^*)/(1-\tau_g^0)} = \frac{dI/I^0}{(\tau_g^0 - \tau_g^*)/(1-\tau_g^0)}$ may not hold either because investment and equity issuance may have different bases or because firms may additionally use some of their cash or raise some debts to finance their marginal investment.

B.2 Extended Measure for the User Cost of Capital

In this subsection, I consider an extended measure for the user cost of capital closely following Barro and Furman (2018).[25] The user cost measure is derived from a neoclassical framework, as used in Hall and Jorgenson (1967) and King and Fullerton (1984). A profit-maximizing firm faces the following steady-state user cost of capital:

$$MPK = C_K = \frac{(1-\tau_c\lambda)}{(1-\tau_c)(1-\tau_g)}(r+\delta) \qquad (B.2)$$

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

Assuming that $\tau_c = 0.22$, $r = 0.082$ (Barro and Furman, 2018), $\delta = 0.1$, and $\lambda = 0.55$, a decrease in capital gains tax rate from 0.3 to 0.18 would reduce the cost of capital from 0.29 to 0.25, which is roughly a 15 percent reduction.[26] The change in the user cost is stable around 15 percent across different depreciation rates for a given required rate of return.

As in Barro and Furman (2018), I also incorporate debt financing in the measure for the user cost. I extend the measure in (B.2) as follows:

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

where θ is the elasticity of default-associated costs with respect to the debt-asset ratio, Δ is the debt-asset ratio, and i is the nominal interest rate on corporate bonds.[27]

Assuming that $\theta = 2$ (Barro and Furman, 2018), $\Delta = 0.5$, $i = 0.03$, $r = 0.082$, and $\delta = 0.1$, a decrease in capital gains tax rate from 0.3 to 0.18 would reduce the cost of capital by roughly 15 percent across different depreciation rates. Based on this framework, accounting for debt-financing does not seem to significantly affect the estimated change in the user cost.

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

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

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

C Additional Heterogeneity Results

C.1 Additional Heterogeneity Results by Firms' Cash-Constraints

In Appendix C.1, I show additional heterogeneity analysis results based on different measures of firms' cash constraints. In Section 5.5, I use retained earnings scaled by assets (averaged over the current and past two years at the time of the reform) as a proxy for firms' cash-constraint, and define that firms are cash-constrained if their measure is below the median. There are other popular measures of cash constraints, such as firm age, dividend payouts (Whited-Wu index), leverage ratio, and cash. I repeat the same heterogeneity analysis using each of these different measures of cash 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.[28] I run the triple difference model as in Section 5.5, substituting the dummy variable for Cash-Rich with this new indicator and including baseline control variables (see Section 4). Column (1) in Table C.1 shows the result based on this triple difference estimation. The difference-in-differences coefficient for investment is positive and significant, implying that cash-constrained firms increase investment after the reform. Moreover, the triple-difference coefficient is negative and statistically significant, suggesting that less cash-constrained firms increase investment less.

I construct Whited-Wu (WW) Index following Whited and Wu (2006) and Hennessy and Whited (2007).[29] 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 5.5, substituting the dummy variable for Cash-Rich with this new indicator. Column (2) in Table C.1 shows the result based on this triple difference estimation. The difference-in-differences coefficient for investment is positive and significant, implying that cash-constrained firms increase investment after the reform. Moreover, the triple-difference coefficient is negative, suggesting that less cash-constrained firms increase investment less, although the difference is not statistically different from zero.

I define leverage as firms' current debts divided by total assets and define that the firm is cash-rich if its leverage ratio is below the median. I run the triple difference model as in Section 5.5, substituting the dummy variable for Cash-Rich with this new indicator. Column (3) in Table C.1 shows the result based on this triple difference estimation. The difference-in-differences coefficient is positive and significant, implying that cash-constrained firms increase investment after the reform. Moreover, the triple-difference coefficient is negative, implying that less cash-constrained firms increase investment less, although the difference is not statistically different from zero.

[28] In Korea, the mean and median firm age among the sample of treated firms is 22 and 19, respectively, while the mean and median firm age among the sample of control firms is 26 and 24, respectively.

[29] WW index is constructed as $-0.091[(ib + dp)/at] - 0.062[\mathbb{1}(dvc + dvp > 0)] + 0.021[dltt/at] - 0.044[log(at)] + 0.102[ARG] - 0.035[RG]$. The variables in italics are Compustat-equivalent data items: *ib* denotes income before extraordinary items, *dp* denotes depreciation and amortization, *at* denotes total assets, *dvc* denotes dividends from common stock, *dvp* denotes dividends from preferred stock, and *dltt* denotes long-term debts. 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).

Finally, I define cash as firms' liquid assets divided by total assets and define that the firm is cash-rich if its cash is above the median. I run the triple difference model as in Section 5.5, substituting the dummy variable for Cash-Rich with this new indicator. Column (4) in Table C.1 shows the result based on this triple difference estimation. The difference-in-differences coefficient for investment is positive and significant, implying that cash-constrained firms increase investment after the reform. Moreover, the triple-difference coefficient is negative and statistically significant, suggesting that less cash-constrained firms increase investment less.

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

Table C.1: Results on Investment by Different Measures of Cash Constraints

	log(Investment)			
	(1)	(2)	(3)	(4)
	Age	Whited-Wu	Leverage	Cash
Treated x Post	0.547	0.671	0.441	0.516
	(0.119)	(0.229)	(0.118)	(0.126)
Treated x Post x Cash-Rich	-0.412	-0.416	-0.185	-0.333
	(0.189)	(0.266)	(0.169)	(0.168)
Time and Firm FE	Yes	Yes	Yes	Yes
Pre-reform Treated Mean (CR=0)	14.141	13.864	14.242	14.602
Implied Elasticity wrt (1-tau) (CR=0)	3.19	3.91	2.57	3.01
Pre-reform Treated Mean (CR=1)	14.364	14.720	14.192	13.855
Implied Elasticity wrt (1-tau) (CR=1)	0.79	1.49	1.50	1.07
R-squared	0.67	0.67	0.67	0.67
Observations (firm-years)	7079	7079	7079	7079
Clusters (Cash-constrained Treated Firms)	130	118	98	91
Clusters (Cash-constrained Control Firms)	174	64	309	292
Clusters (Cash-rich Treated Firms)	57	69	89	96
Clusters (Cash-rich Control Firms)	347	457	212	229

Notes: This table reports the tax effects on investment based on the triple difference estimation. The dummy for $Treated_i$ equals 1 if a firm i had a tax reduction. 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 is cash-constrained, as defined in Appendix C.1. Investment is defined as log of expenditures on physical capital assets. Each time period is a year, and the sample period is from 2009 to 2019. All specifications include time and firm fixed effects (FE). The sample is restricted to publicly listed companies. The standard errors are clustered at the firm level and are reported in parentheses.

C.2 Managerial Incentive Channel

C.2.1 Extended Model with Agency Conflicts

I extend the model in Section 3 by incorporating agency conflicts (Shleifer and Vishny 1986; Chetty and Saez 2010).[30] The main source of departure from the model is that the firm's manager can also invest in pet projects, J, to maximize his or her private utility. In period 1, the firm's manager chooses $\{I, J, R, E\}$ to maximize his or her utility such that $I + J + R = C + E$. In period 2, net-of-tax profits are distributed to shareholders. Therefore, the manager's problem is:

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

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

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

I assume that (1) $f(I)$ and $g(J)$ are strictly concave, (2) firms can raise additional funds only through issuing new equity, (3) firms pay investors only through repurchasing shares, and (4) there is no other corporate governance mechanism to incentivize the manager to maximize firm profits.[31]

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 the manager. Therefore, the manager can also use C to invest in J that gives private benefits of $g(J)$. Assume that $g'(0) > \omega f'(C)$, which ensures an interior optimum in investment response. Then I and R are determined by the following 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 manager 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.

[30] 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 this paper.

[31] Examples of corporate governance mechanisms in the 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 putting monitoring costs or other measures of corporate governance on the weight (Chetty and Saez 2010). In this way, strengthening corporate governance would have similar effects as giving more shares to the manager, since higher levels of governance would make it more costly for the manager to deviate from profit-maximization.

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 $\omega r = g'(C-I^*-R)$.[32] Intuitively, this means that depending on the weight that the manager puts on the firm's profit maximization, the firm's initial level of share repurchases and investment would be set differently. This also implies that when the weight changes, either through 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 manager has a weak incentive towards the firm's profit maximization, then the manager retains as much cash as possible for pet projects and does not buy back shares. By contrast, for a manager 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 manager does not buy back shares and splits cash between I and J, where the manager chooses I to equate his or her 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 manager has $\omega > \bar{\omega}$, then the manager has enough cash to repurchase shares, and sets $I = I^*$. Any increase in ω (i.e., a lower tax rate), increases the marginal return on profitable investment, as much as it increases the opportunity cost of investment, which equals the amount of share repurchases to pay himself or herself. Therefore, an empirical prediction based on this theoretical framework is that after a tax cut, firms whose managers own a lower fraction of firms' stock would increase investment more relative to firms whose managers own a larger share.

C.2.2 Empirical Test for Managerial Incentive Channel

The managerial incentive channel predicts that low-ownership managers will increase investment more after a payout tax cut than will high-ownership managers. When a firm is cash-rich and its managers own 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 manager owns a low share in a cash-rich firm, lowering the tax rate only shifts incentives away from his or her 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 managers, so investment will increase regardless of managers' ownership rates. Therefore, the model predicts that after a tax cut, low-ownership managers increase investment

[32] See Chetty and Saez (2010) for the detailed proof behind this.

more than do high-ownership managers on average.

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

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

where $CEO_i = 1$ if managers (i.e., CEOs and board of directors) of the firm i has a stock share above the median at the reform year of 2014 (fixed at the reform year), and the rest of variables are as defined in equation (6).[33] θ_1 captures the tax effects for low-ownership managers ($CEO_i = 0$) and θ_2 captures the difference in the tax effects between the two manager types.

Table C.2 presents the results for the triple-difference estimation. Column (1) shows that the investment response is positive and significant for firms with managers who own a lower fraction of firms' stock, with an implied elasticity of 2. The coefficient on the triple interaction term is positive, but not statistically significant, implying that the investment elasticity is not smaller for firms with high ownership managers. The triple difference coefficient, along with the difference-in-differences coefficient, is neither consistent nor inconsistent with predictions of the model by (Chetty and Saez 2010).

An important caveat is that these results may be sensitive to the assumption on the pre-reform managerial ownership of listed firms that went public after the reform. Because I do not have data on managerial ownership of private firms, I use the average managerial ownership rates of post-IPO (listed) firms to replace missing values of their pre-IPO managerial ownership rates. In other words, I assume that managerial ownership rates remain constant on average after firms go public. This assumption may not be true if firms change their managers' ownership structure after initial public offerings.[34] Future studies that use more detailed managerial ownership data for private firms may help identify this channel more accurately and precisely.

[33] In my analysis sample, low-ownership managers have around 4 percent of their firms' stock on average, while high-ownership managers have around 27 percent on average.

[34] Even if I assume that private firms' managerial ownership is high before going public, the difference in investment responses between firms with low-ownership managers and firms with high-ownership managers is still not statistically different from zero.

Table C.2: Results on Investment and Capital Structure by Managerial Ownership (Listed Firms)

	Investment	Capital Structure		
	(1)	(2)	(3)	(4)
	log(CAPEX)	Equity Issuance	Dividend Payout	Share Buyback
Treated x Post	0.344	0.088	0.036	0.008
	(0.128)	(0.019)	(0.031)	(0.019)
Treated x Post x CEO	0.019	-0.006	-0.040	0.019
	(0.167)	(0.023)	(0.042)	(0.025)
Time and Firm FE	Yes	Yes	Yes	Yes
Pre-reform Treated Mean (CEO=0)	14.163	0.044	0.080	0.011
Implied Elasticity wrt (1-tau) (CEO=0)	2.01	11.62	2.65	4.18
Pre-reform Treated Mean (CEO=1)	14.272	0.027	0.101	0.027
Implied Elasticity wrt (1-tau) (CEO=1)	2.12	17.52	-0.24	5.78
R-squared	0.65	0.32	0.22	0.16
Observations (firm-years)	7097	6768	7117	7117
Clusters (Low-ownership CEO Treated Firms)	80	80	80	80
Clusters (Low-ownership CEO Control Firms)	295	295	295	295
Clusters (High-ownership CEO Treated Firms)	107	107	107	107
Clusters (High-ownership CEO Control Firms)	225	225	225	225

Notes: This table reports the tax effects on investment and capital structure based on specification (7). The dummy for $Treated_i$ equals 1 if a firm i had a tax reduction, 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 managers have stock shares above the median, as defined in Appendix C.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. Dividend payouts and share repurchases are scaled by lagged current profits. Each time period is a year, and the sample period is from 2009 to 2019. 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.

D Robustness Checks

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

D.1 With Control Variables

I repeat the main analysis in equation (7), with basic and additional controls, and with only basic controls. Column (1) of Table D.1 shows the main result with only basic controls, and Column (2) of Table D.1 shows the result with both basic and additional controls. Basic controls are quartics in firm age and industry dummies interacted with year dummies, and additional controls are dummies for each pre-reform (2014) operating profit quintile interacted with dummies for each year. I include quartics in age to control for baseline financial constraints of firms among treated and control groups. Furthermore, industry composition is different between treated and control groups, 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 (gross profit minus operating expenses) quintiles interacted with dummies for each year. In other words, I allow each quintile of the operating profits to have its own non-parametric time trends unrelated to the firm-size reform in 2014. In Column (3), I also non-parametrically control for price-to-book value ratio in a similar way to absorb any non-tax trends driven by baseline differences in firm value across groups.

The coefficient estimates are larger when I include only basic or both basic and additional control variables, but the results are qualitatively similar to the ones from the main specification without any controls in equation (7). While the coefficient estimate in Column (3) is smaller than the main estimate, this result is consistent with the key result that a reduction in capital gains tax rates would lead to an increase in investment.

The difference-in-differences estimate with the control variables is slightly larger than the one without the control variables in part because adding those controls, which are essentially fixed effects, may reduce the variance of the residual of the treatment. This argument depends on the idea that there exists treatment effect heterogeneity based on firm characteristics, such as by cash-constraints (See Section 5.5), and in the presence of such heterogeneity, the OLS with fixed effects (control variables in this case) may yield an inconsistent estimate of the sample-weighted average treatment effect (Gibbons, Serrato and Urbancic 2018).

D.2 With Different Levels of Winsorization

I repeat the main analysis using the same specification as in equation (7), winsorizing (bottom- and top-coding) the main outcome variable at the 1% and 99% levels, instead of at the 5% and 95% levels. Column (4) of Table D.1 shows the result based on these different levels of winsorizing. The coefficient estimate for *log*(investment) is larger, but qualitatively similar.

Moreover, I repeat the main analysis without any winsorization and by including the excluded firms that have expenditures on total investment more than twice of their lagged tangible assets. Column (5) of Table D.1 shows the tax effects on investment based on including these excluded firms without any winsorization and shows that the result is qualitatively similar.

D.3 Including Firms in Other Sectors

I repeat the main analysis using the same specification as in equation (7), including firms in other sectors. Column (6) of Table D.1 shows the result based on using firms in other sectors in addition to the firms in the main analysis sample. The coefficient estimate for log(investment) is larger, but qualitatively similar when I include firms in other sectors.

D.4 Forward-looking Firms (Partially Treated by the Reform)

I repeat the main analysis on investment using firms that were growing below the old thresholds. Even though these forward-looking firms did not experience a tax cut or bunch to avoid higher taxes, their investment might still be affected by the reform in a dynamic sense. For example, suppose a small firm with 150 average number of employees and 50 million in total revenue 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. Since the old cutoffs became no longer binding, this firm may grow even more or faster after the reform in 2014. Therefore, this type of forward-looking firm could be partially affected by the reform. As an additional robustness check, I use the firms below the bunching thresholds as an additional control group and estimate a separate difference-in-differences model by comparing the main treated firms with a tax cut with the firms above the new threshold and this additional control group. Column (7) of Table D.1 shows the estimate based on this specification. The difference-in-differences coefficient for the tax effects on investment is larger, but qualitatively similar, suggesting that the additional control group might not have been partially treated by the reform and increased investment.

D.5 Dollar-weighting by Revenue

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 (larger firms) will carry higher weights in their investment estimates. The key issue with weighting by revenue (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. Column (8) of Table D.1 shows the result based on weighting by lagged revenue. The coefficient estimate is smaller than the one from without weighting, but both estimates are comparable. Therefore, the main result is robust to dollar-weighting.

D.6 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 the 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 deflates 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 20 (= 2 groups $\times$ 10 years 2009-2019) 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 profit (gross profit minus operating expenses) 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 Appendix D.1. Column (9) of Table D.1 reports the estimate with DFL-reweighting. The coefficient estimate is larger with DFL-reweighting, but comparable to the main estimate.

D.7 Placebo Tests for Aggregate Shock and Potential Mean-reversion

It is possible that other time-varying shocks, such as different policy reforms or financial crisis, may coincide with the main reform in 2014 and may differentially affect firms of different sizes. Furthermore, it is possible that the difference-in-differences estimate of the investment response could have been driven by a mean-reversion because the treated firms were smaller relative to the control group on average. To address such potential concerns, I conduct placebo tests using pre-reform time periods (from 2002 to 2014) and setting earlier years as the placebo reform dates (i.e., 2006, ..., 2009). For example, there was a global financial crisis in 2007, which might have differentially affected firms of different sizes. I use the year 2007 as the placebo date and set *Post* equal to 1 if it is after 2007. Panel A in Figure D.2 show the results based on this placebo test. The coefficient estimates after 2007 are not statistically different from zero, suggesting that the main result is unlikely driven by other policy changes or mean-reversion. Panel B of Figure D.2 finds similar results using 2009 as the reform year. Moreover, if a mean-reversion was the main driver of the investment response after the reform, then a similar mean-reversion would have likely occurred in earlier periods, which would violate the parallel pre-trends. Finally, I control for pre-reform firm size and profitability to make the treatment group more comparable to the control group, and find qualitatively similar results.

D.8 Measuring Cash-constraints Using a Different Cut (Tercile)

In Section 5.5, I use retained earnings scaled by assets (averaged over the current and past two 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. Column (1) of Table D.2 shows the result based on defining the cash-constrained (cash-rich) firms as the ones below the thirty third percentile (above the sixty sixth percentile) of the average retained earnings in 2014. I find that the result is qualitatively similar to the main estimate.

D.9 Different Measures of Investment

For additional measures of investment, I scale expenditures on physical capital assets by the average tangible asset over the pre-reform period. Moreover, I estimate the effects of the tax cut on the capital stock, defined as the log of tangible assets. Table D.3 and Figure D.4 show that the results based on these different measures of investment are qualitatively comparable to the main estimate using log(investment).

D.10 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 the 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.4 shows the result just using firms that were bunching as treated and unaffected firms as control. Column (1) shows that their investment response is lower than the one from the firms with a tax cut, consistent with the idea that firms that were bunching did so because they did not have investment opportunities to justify crossing the thresholds.

D.11 Dropping Firms Around the New Cutoff

Including treated firms right below the new threshold may attenuate the main estimate of the tax effects on investment if these firms had an incentive to decrease investment and suppress sales to stay below the new cutoff. By contrast, including the control firms right above the new threshold may overstate the result if these firms had an incentive to decrease investment to go below the new cutoff. To account for this potential source of bias, I incrementally drop firms around the new threshold in the range between two to ten percent. Figure D.3 shows the results, where the first dot indicates the estimate where I drop firms 2 percent around the new threshold. The dashed horizontal line indicates the main estimate. The estimates across different bins are close to the original estimate and thus, my results are robust to accounting for these potential sources of bias.

D.12 Firms Going Above or Below the New Cutoff after the Reform

As discussed in Section 4, the dummy, $Treated_i$, is fixed at the reform year, which could attenuate or overstate my estimates. Roughly 19% of the treated firms crossed the new threshold by 2019 on average, while about 11% of the control firms went below the new cutoff by 2019 on average. Note that there is a 3-year grace period for firms that crossed the new threshold by the end of 2015 (or after), so all of these firms that went over the new threshold by 2018 still remained as small firms. Furthermore, to address this potential bias, one can re-scale my estimates using the dummy for $Treated_{it}$ in 2015 as an instrumental variable for dummies for $Treated_{it}$ in 2016, 2017, 2018, and 2019. The intuition is that even though $Treated_{it}$ for $t \in (2016, 2017, 2018, 2019)$ may be endogenous, $Treated_{i2015}$ is exogenously determined by the reform and highly correlated with treatment dummies in later years. Given that a relatively small portion of firms went below the new threshold and due to the grace period, my intent-to-treat estimates from the difference-in-differences estimation are close to the treatment effects on the treated. Another way to deal with this potential issue is by dropping firms around the new cutoff (see Section D.11).

D.13 Jackknife Coefficient Estimates

To address a potential concern arising from a relatively small number of treated firms, I estimate jackknife coefficients by leaving out one treated firm at a time and check whether there are a few firms having large effects on the coefficient of interest. Panels A and B in Figure D.5 show that the jackknife coefficient estimates across 187 trials are very similar to the main estimate. Therefore, these results are consistent with my results being not driven by very few outlier firms.

D.14 Block Permutation Tests

To address potential concerns that a few outliers are driving the main results and that the difference-in-differences estimation strategy may over-reject the null hypothesis when error terms are serially correlated, I conduct a set of block permutation tests on the main outcomes similar to those used in Chetty, Looney and Kroft (2009). Each block permutation is performed by randomly assigning firms into a placebo group of 187 firms (without replacement) and the rest into the control group. Note that I do a set of placebo tests using earlier years as placebo years in Appendix D.7, which I do not repeat here. In this way, the reform year remained unchanged, but the treated units are block permuted. Then, I estimate the main equation (7) using these groups and repeat this procedure 49,999 times to produce the test results. Panels A and B in Figure D.6 show an empirical cumulative distribution function of 50,000 placebo coefficients. The main estimates (in red vertical lines) are in the tail of the distribution: for investment, only 11 out of 50,000 placebo coefficients are larger than the estimated effect, suggesting a p-value close to zero, and for equity issuances, no placebo coefficient is larger than the estimated effect, suggesting a p-value of zero. The non-parametric p-values are similar to those in the main results, suggesting that clustering at the firm-level addresses the serial correlation concern, and random differences between the treated and control group, potentially driven by a few outliers, are unlikely to generate the main results.

D.15 Results in Dollar Amounts

To show the effects of the tax cut on investment and equity issuances in dollar amounts (using 1,000 Korean won to 1 U.S. dollars currency conversion), I estimate the same difference-in-differences model for the main result, as well as for the heterogeneity results by cash-constraints and by IPO dates, using the outcome variables in dollar amounts, rather than in logs or scaled by lagged revenue. Table D.6 shows these results in millions of dollars. Columns (1) and (2) indicate that treated firms increased investment by 2.7 million dollars and raised more equity by 2.8 million dollars on average after the reform. Columns (3) and (4) indicate that treated cash-rich firms increased investment by 1.6 million dollars and raised more equity by 1.9 million dollars, while Columns (5) and (6) show that treated cash-constrained firms increased investment and equity issuances by 3.9 million dollars and 3.5 million dollars, respectively, on average after the reform. Columns (7) and (8) indicate that treated firms that went IPO before 2015 increased investment by 2.2 million dollars and raised more equity by 1.2 million dollars, while treated firms that went IPO after 2015 increased investment by 4 million dollars and raised new equity by 5 million dollars on average after the reform.

Table D.1: Tax Effects on Investment Across Different Robustness Tests

	Control Variables			Sample Selection				Weights	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Basic Controls	Full Controls	Firm Value	Winsor	Trim	Other Sectors	Add Control Group	Dollar Weight	DFL
Treated x Post	0.360	0.370	0.250	0.377	0.429	0.421	0.393	0.332	0.363
	(0.090)	(0.092)	(0.124)	(0.089)	(0.089)	(0.080)	(0.075)	(0.110)	(0.089)
Time and Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-reform Treated Mean	14.220	14.220	14.621	14.204	14.170	14.227	14.221	14.256	14.224
Implied Elasticity wrt (1-tau)	2.10	2.16	1.46	2.20	2.50	2.46	2.29	1.94	2.12
R-squared	0.67	0.68	0.66	0.64	0.61	0.65	0.60	0.69	0.63
Observations (firm-years)	7079	7079	5651	7105	7595	8089	17320	6110	7105
Clusters (Treated Firms)	187	187	187	187	201	199	187	187	187
Clusters (Control Firms)	521	521	521	521	547	612	1015	521	521

Notes: This table reports the tax effects on investment. The dummy for $Treated_i$ equals 1 if a firm i had a tax reduction, 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. Column (1) includes only basic control variables. Column (2) includes basic and additional controls. Column (3) non-parametrically controls for firm value in addition. In Column (4), the main outcome is winsorized at the first and the ninety-ninth percentiles. Column (5) includes the excluded firms that had investment exceeding twice their lagged tangible assets. Column (6) includes firms in other sectors. Column (7) includes growing firms below the bunching thresholds as an additional control group. Column (8) uses lagged revenue as weights. Column (9) uses DFL re-weighting. Each time period is a year, and the sample period is from 2009 to 2019. 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.

Table D.2: Heterogeneity Results by Tercile Cut

	By Cash-constraints
	(1)
	log(CAPEX)
Treated x Post	0.605
	(0.159)
Treated x Post x Cash-Rich	-0.525
	(0.219)
Time and Firm FE	Yes
Pre-reform Treated Mean (CR=0)	14.015
Implied Elasticity wrt (1-tau) (CR=0)	3.53
Pre-reform Treated Mean (CR=1)	14.3
Implied Elasticity wrt (1-tau) (CR=1)	0.47
R-squared	0.63
Observations (firm-years)	4457
Clusters (Cash-contrained Treated Firms)	57
Clusters (Cash-constrained Control Firms)	154
Clusters (Cash-rich Treated Firms)	49
Clusters (Cash-rich Control Firms)	180

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, 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). In Column (1), the dummy for $Cash\ Rich_i$ is 1 if the firm is cash-rich firm, as defined in Section D.10. Investment is defined as log of expenditures on physical capital assets. Each time period is a year, and the sample period is from 2009 to 2019. 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.

Table D.3: Main Results by Different Measures of Investment

	Investment/Tangible Assets	Tangible Assets
	(1)	(2)
	CAPEX/AvgPPE	ln(PPE)
Treated x Post	0.107	0.185
	(0.021)	(0.051)
Time and Firm FE	Yes	Yes
Pre-reform Treated Mean	0.205	16.292
Implied Elasticity wrt (1-tau)	3.06	1.08
R-squared	0.43	0.89
Observations (firm-years)	7125	7125
Clusters (Treated Firms)	187	187
Cluster (Control Firms)	521	521

Notes: This table reports the tax effects on different measures of investment. The dummy for $Treated_i$ equals 1 if a firm i had a tax reduction, 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 average tangible asset, where the pre-reform tangible assets are used to compute the average. In Column (2), PPE is defined as the book value of tangible assets. Each time period is a year, and the sample period is from 2009 to 2019. 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.

Table D.4: Main Results by Firms Bunching at Old Cutoffs

	Bunching Firms	Tax Cut Firms
	(1)	(2)
	ln(CAPEX)	ln(CAPEX)
Treated x Post	0.181	0.256
	(0.106)	(0.052)
Time and Firm FE	Yes	Yes
Pre-reform Treated Mean	13.989	13.907
Implied Elasticity wrt (1-tau)	1.05	1.49
R-squared	0.73	0.71
Observations (firm-years)	15363	19357
Clusters (Treated Firms)	137	557
Clusters (Control Firms)	1549	1549

Notes: This table reports the tax effects on investment based on the difference-in-differences estimation, where I define treated firms in Column (1) as those that were bunching at the old cutoffs prior to 2014. 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. Each time period is a year, and the sample period is from 2009 to 2019. The sample includes both publicly listed and private companies. All specifications include time and firm fixed effects (FE). The standard errors are clustered at the firm level and are reported in parentheses.

Table D.5: Results on Investment and Capital Structure by Cash Constraints and IPO date

	IPO before 2015		IPO after 2015	
	(1)	(2)	(3)	(4)
	log(CAPEX)	Equity Issuance	log(CAPEX)	Equity Issuance
Treated x Post	0.404	0.080	0.635	0.151
	(0.142)	(0.024)	(0.152)	(0.030)
Treated x Post x Cash-Rich	-0.318	-0.049	-0.223	-0.015
	(0.197)	(0.029)	(0.238)	(0.039)
Time and Firm FE	Yes	Yes	Yes	Yes
Pre-reform Treated Mean (CR=0)	14.413	0.059	14.002	0.035
Implied Elasticity wrt (1-tau) (CR=0)	2.36	7.94	3.70	24.89
Pre-reform Treated Mean (CR=1)	14.441	0.027	13.709	0.009
Implied Elasticity wrt (1-tau) (CR=1)	0.51	6.68	2.40	84.80
R-squared	0.64	0.28	0.65	0.32
Observations (firm-years)	6375	6086	5999	5728
Clusters (Cash-constrained Treated Firms)	51	51	51	51
Clusters (Cash-constrained Control Firms)	251	251	251	251
Clusters (Cash-rich Treated Firms)	53	53	32	32
Clusters (Cash-rich Control Firms)	270	270	270	270

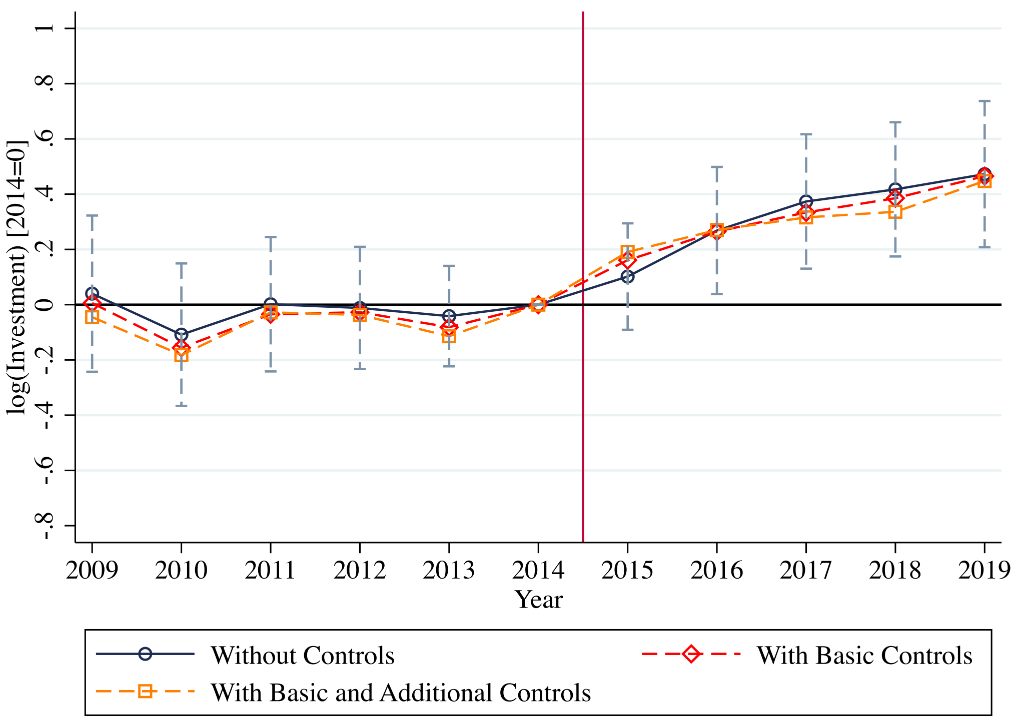
Notes: This table reports the tax effects on investment and capital structure based on specification (12). The dummy for $Treated_i$ equals 1 if a firm i had a tax reduction, as explained in Section 4. The dummy for $post_t$ equals 1 if the time period is after the end of the reform year (2014). The dummy for CR_i is 1 if the firm is cash-rich firm, as defined in Section 5.5. Columns (1) and (2) show the results for treated firms that went public before 2015, relative to the baseline control group, as explained in Section 5.5. Columns (3) and (4) show the results for treated firms that went public after 2015, relative to the same baseline control group. 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. Each time period is a year, and the sample period is from 2009 to 2019. 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.

Table D.6: Results on Investment and Equity Issuances in Dollar Amounts

	All Firms		Cash-Rich Firms		Cash-Constrained Firms		Pre-2015 IPO Firms		Post-2015 IPO Firms	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	CAPEX	Equity Issuance	CAPEX	Equity Issuance	CAPEX	Equity Issuance	CAPEX	Equity Issuance	CAPEX	Equity Issuance
Treated x Post	2.685	2.827	1.604	1.901	3.872	3.486	2.226	1.229	3.970	5.201
	(0.485)	(0.616)	(0.634)	(0.590)	(0.745)	(1.034)	(0.527)	(0.730)	(0.820)	(0.868)
Time and Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.58	0.23	0.58	0.20	0.59	0.22	0.58	0.24	0.57	0.23
Observations (firm-years)	6110	5829	3176	3073	2934	2756	5484	5237	5181	4952
Clusters (Treated Firms)	187	187	85	85	102	102	104	104	83	83
Cluster (Control Firms)	521	521	270	270	251	251	521	521	521	521

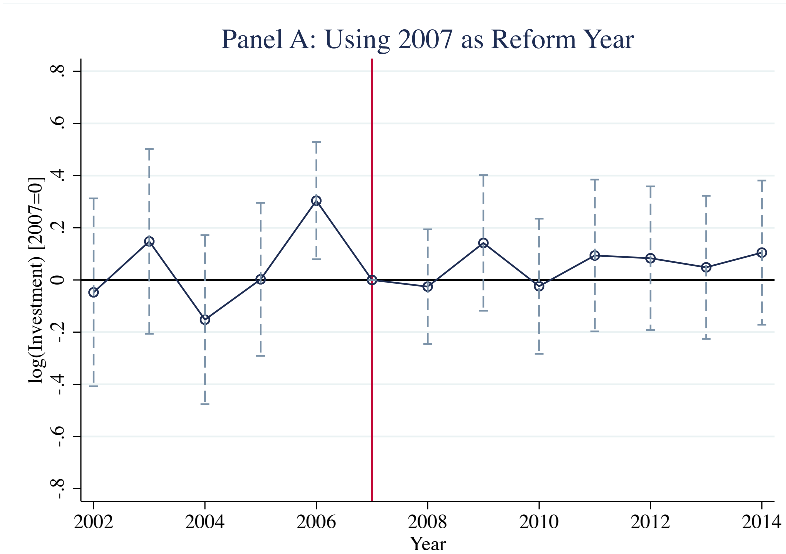
Notes: This table reports the tax effects on investment and capital structure based on specification (7). The dummy for $Treated_i$ equals 1 if a firm i had a tax reduction, as explained in Section 4. The dummy for $post_t$ equals 1 if the time period is after the end of the reform year (2014). Investment is defined as expenditures on physical capital assets (in millions U.S. dollars). Newly issued equity is measured as non-negative changes in total paid-in capital (in millions U.S. dollars). The currency conversion is based on 1,000 Korean won to 1 U.S. dollars. Each time period is a year, and the sample period is from 2009 to 2019. 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.

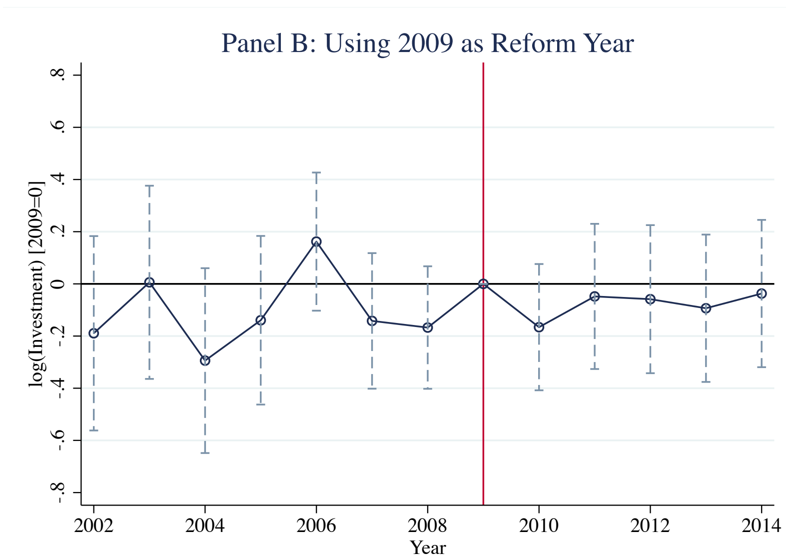
Figure D.1: Tax Effects on Investment



Notes: The dark solid line in this figure indicates the coefficients on $Treated \times Time$ for firms' investment, defined as log(expenditures on physical capital assets), in equation (6). The dashed lines indicate 95% confidence intervals for these coefficient estimates. The solid vertical line indicates the reform year. The red dashed line indicates the coefficients in equation (6) with basic controls and the orange dashed line indicates those with both basic and additional controls.

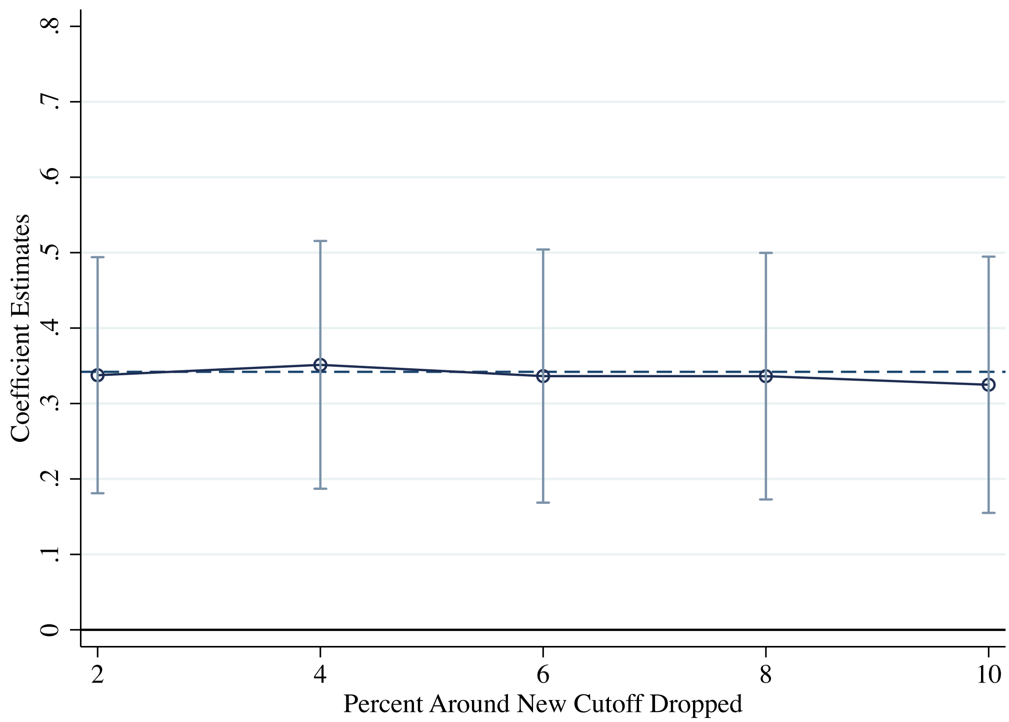
Figure D.2: Placebo Tests using Prior Year (2007) as the Reform Date





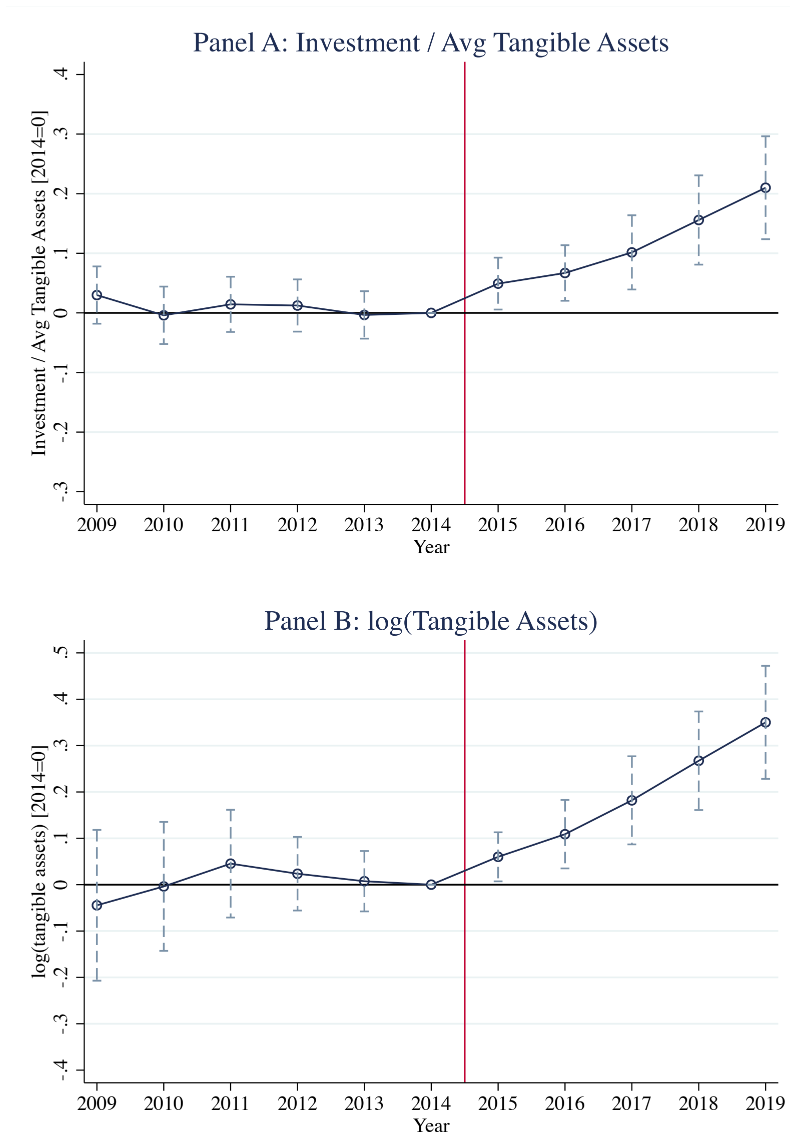
Notes: Panel A of this figure shows the difference-in-differences coefficient estimates on investment, as in equation (6), using 2007 as the reform year (instead of 2014). The dashed lines indicate 95% confidence intervals for these coefficient estimates. The solid vertical line indicates the (placebo) reform year. Panel B of this figure shows the coefficient estimates using 2009 as the reform year.

Figure D.3: Dropping Firms Right Around the New Cutoff



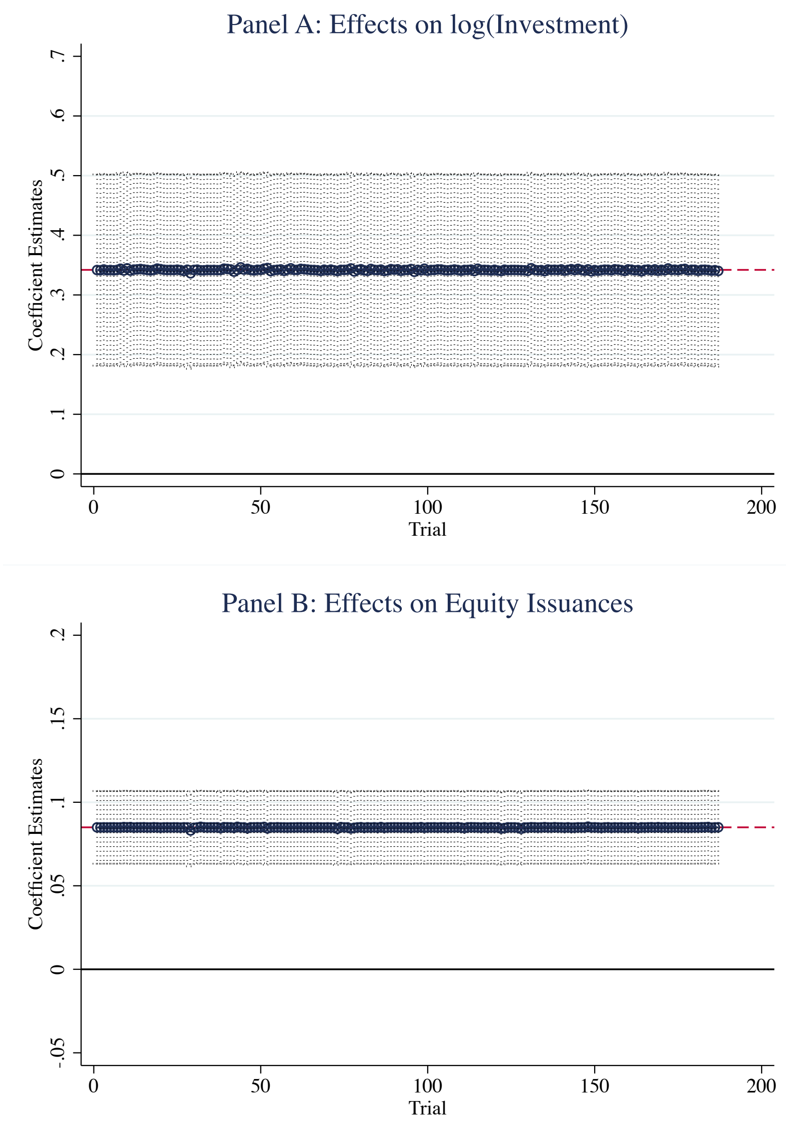
Notes: This figure shows the difference-in-differences coefficients on investment across different specifications, where I incrementally drop firms around the new cutoff in the range between two to ten percent (as discussed in Appendix D.11). The dashed horizontal line indicates the main estimate. The outcome variable is investment, as defined by *log*(expenditures on physical capital assets). The solid vertical lines indicate the 95% confidence intervals for those coefficient estimates.

Figure D.4: Tax Effects on Investment Rate and Tangible Assets



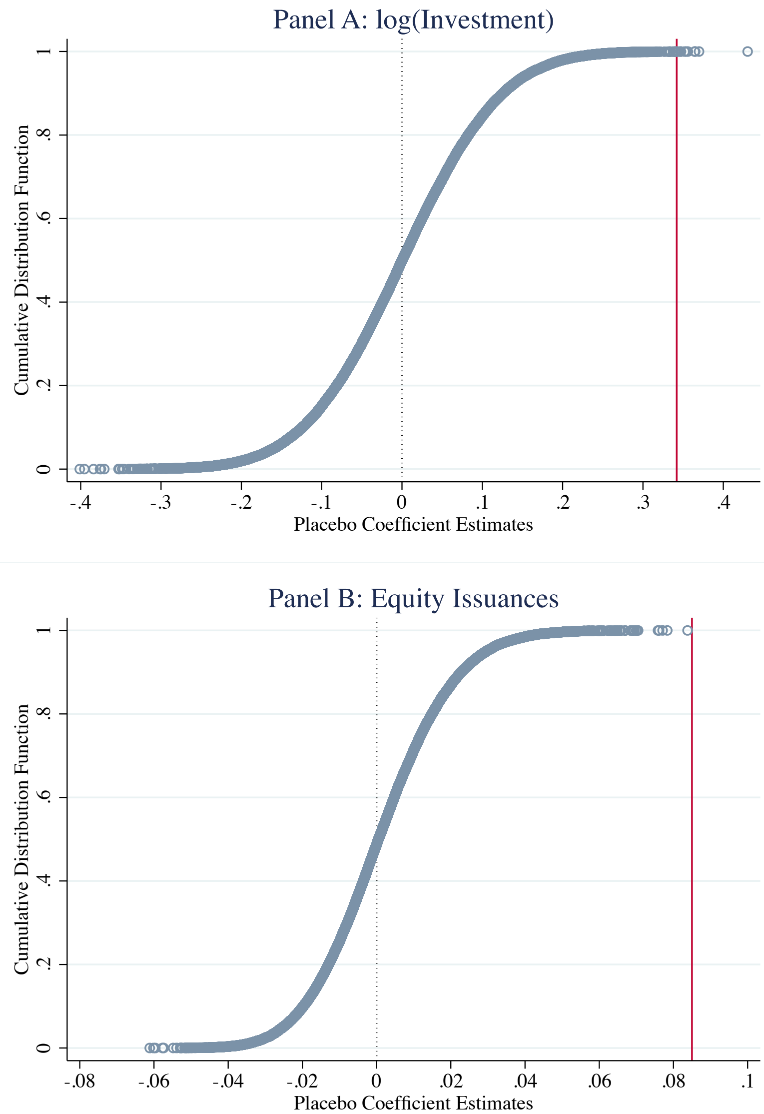
Notes: Panel A of this figure indicates the coefficients on $Treated \times Time$, as in equation (6), for firms' investment, defined as expenditures on physical capital assets scaled by the pre-reform average of tangible assets. The dashed lines indicate 95% confidence intervals for these coefficient estimates. Panel B of this figure indicates the coefficients on $Treated \times Time$, as in equation (6), for firms' capital stock, defined as log(tangible assets).

Figure D.5: Jackknife Coefficient Estimates on Investment and Equity Issuances



Notes: Panel A of this figure shows the jackknife coefficients for *log*(investment), where each coefficient is estimated from the difference-in-differences model, as in equation (7), while leaving out one treated firm at a time (repeated 186 times). The red horizontal line indicates the main estimate. Panel B of this figure shows the jackknife coefficient estimates for equity issuances.

Figure D.6: Block Permutation Tests on Investment and Equity Issuances



Notes: Panel A plots the empirical distributions of placebo effects for *log*(investment). Each cumulative distribution function is constructed by regressing the outcome variable on 50,000 randomly assigned treated firms and controls as explained in Appendix D.14. The cumulative distribution function appears smooth (without parametric smoothing) because of the large number of points used to construct it. The vertical lines indicate the main estimate. Panel B plots the empirical distributions of placebo effects for equity issuances. In Panel A, 11 out of the 50,000 placebo coefficients are larger than the main coefficient estimate (with p-value close to zero). In Panel B, no placebo coefficient is larger than the main coefficient estimate (with p-value of zero).

E Other Outcomes

In Appendix E, I provide a set of estimates of the tax effects on other outcomes: (1) average number of employees, (2) total revenue, (3) capital structure, (4) return on assets, and (5) expenditures on R&D.

Column (1) of Table E shows the effects of reducing capital gains tax rates on the average number of employees. The coefficient estimate is 0.07, implying that affected firms increased employees by 7 log points on average after the reform, compared to unaffected firms, based on the sample of publicly listed firms. Note that the elasticity of labor with respect to the net of tax rate, 0.42, is smaller but comparable to the capital stock elasticity of 1 with respect to the net of tax rate.

Column (2) of Table E shows the effects of reducing capital gains tax rates on firms' total revenue. The coefficient estimate is 0.315, implying that affected firms increased revenues by 32 log points on average after the reform, compared to unaffected firms, based on the sample of publicly listed firms. Overall, the increase in total revenue is consistent with the results that affected firms increased investment and the average number of employees.

Column (3) shows that the effects of lowering capital gains tax rates on firms' debts are both economically and statistically significant. Column (4) shows that the affected firms decreased debt to equity ratio, implying that the book value of total equity relative to total debts increased for these firms. Column (5) shows that the effects of lower capital gains taxes on firms' return on assets are positive. Finally, Column (6) shows that lowering capital gains tax rates increased firms' expenditures on research and development by 1.8 cents per dollar of revenue. Therefore, most of these other outcomes were affected in a way that was consistent with the increases in investment and equity issuances of treated firms.

Table E: Results on Other Corporate Outcomes (Publicly Listed Firms)

	Employment and output		Capital Structure		Profits	Other Investment
	(1)	(2)	(3)	(4)	(5)	(6)
	ln(Employee)	ln(Revenue)	Debts	Debt-Equity	Return on Assets	RnD Expenditures
Treated x Post	0.072	0.315	0.180	-0.202	0.117	0.018
	(0.040)	(0.043)	(0.051)	(0.058)	(0.067)	(0.003)
Time and Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Pre-reform Treated Mean	5.163	17.579	16.820	1.142	0.453	0.015
Implied Elasticity wrt (1-tau)	0.42	1.84	1.05	-1.03	1.51	7.13
R-squared	0.79	0.85	0.86	0.70	0.58	0.71
Observations (firm-years)	7056	7125	7125	7125	7125	7123
Clusters (Treated Firms)	187	187	187	187	187	187
Cluster (Control Firms)	521	521	521	521	521	521

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, 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). Employee is defined as the average number of workers employed over the total operating period in a given year. Revenue is defined as the total revenue. Debts are defined as the log of the book value of total liabilities. Debt to Equity ratio is the book value of total liabilities scaled by the book value of total equity (total assets minus total liabilities). Return on assets is defined as current profits scaled by lagged tangible assets. R&D Expenditures are firms' expenses on research and development activities (i.e., hiring researchers and buying lab equipment), scaled by total revenue. Each time period is a year, and the sample period is from 2009 to 2019. The sample is restricted to publicly listed companies. All specifications include time and firm fixed effects (fixed effects). The standard errors are clustered at the firm level and are reported in parentheses.

F External Validity

In Appendix F, I provide a set of descriptive statistics to show that treated firms in my analysis sample are comparable to the general firms in the Korean economy. Table F.1 provides descriptive statistics on listed and private companies in Korea during the sample period. As Column (1) shows, the average firm in the overall sample that includes small firms below the old thresholds is smaller, but comparable to the average treated firm in my analysis sample (see Table 1). Therefore, the fact that the affected firms are smaller than the unaffected firms in my sample does not mean that they are small compared to the average (representative) firm in Korea.

Additionally, I use firm financial data from the Amadeus Database on 18 Organization for Economic Co-operating and Development (OECD) countries in Europe to examine their capital structure, such as cash and leverage ratio, and to check how comparable these firms are to the Korean firms in terms of their cash constraints. The thought experiment is that if firms in the other countries had the same capital gains tax system and experienced the same reform, then we could extrapolate how much their investment response would have been, based on their characteristics related to measures of cash constraints.

Table F.2 describes summary statistics for firms during a similar sample period (from 2010 to 2018) for each country. I focus on firms operating under manufacturing, construction, transportation, warehousing and information services sectors (similar to firms in my main analysis sample). For firms of similar sizes, in terms of the number of employees and revenue, which would have been treated firms had they been Korean firms around this time, the cash-constraint characteristics based on firm age, leverage ratio, and cash equivalent assets are similar between Korea and countries like Denmark, France, Finland, Greece, and Poland (see Figure F). Overall, listed firms (of similar sizes and operating under similar industries) in Finland and Poland are most comparable to listed firms in Korea, based on characteristics related to cash-constraints during a similar sample period.

Table F.1: Descriptive Statistics for Listed and Private Companies in Korea

	Overall Sample
	(1) Listed and Private
Total Revenue (in millions)	134.9
	(80.30)
Labor (Average Employee)	266.6
	(192)
Total Asset (in millions)	164.1
	(104.7)
Total Capital (in millions)	95.55
	(61.49)
CAPEX (in millions)	5.656
	(2.109)
CAPEX / lagged PPE	0.194
	(0.104)
Firm Age	25.59
	(21)
Leverage Ratio	0.300
	(0.279)
Cash (Cash Asset / Total Asset)	0.0860
	(0.0555)
Observations	13944

Notes: Sample years include 2009 – 2019. The medians are in parentheses. Labor is the average number of employees used in a given year. CAPEX is expenditures on physical capital assets, such as plant, property, and equipment (PPE). Leverage ratio is defined as total debts scaled by total assets. The sample includes treated firms, control firms, as well as small firms below the old thresholds.

Table F.2: Summary Statistics (OECD Countries)

Panel A: Countries AT – GR

	(1) AT	(2) BE	(3) DE	(4) DK	(5) ES	(6) FI	(7) FR	(8) GB	(9) GR
Total Revenue	99.10	105.8	94.95	106.8	92.98	93.29	94.03	88.51	79.77
	(104.6)	(110.5)	(100.3)	(109.7)	(102.0)	(101.4)	(100.3)	(92.10)	(77.50)
Number of Employees	368.1	303.7	397.6	353.2	386.4	385.2	381.4	441.8	475.1
	(350)	(310.5)	(370)	(329.5)	(350)	(360)	(363)	(382)	(400)
Total Assets	78.02	97.19	65.40	85.17	97.84	73.81	70.43	76.58	143.6
	(61.20)	(66.62)	(54.56)	(61.00)	(74.92)	(54.83)	(56.61)	(54.87)	(95.64)
Firm Age	35.04	34.19	43.37	30.54	28.94	27.38	30.18	32.22	32.92
	(26)	(29)	(30)	(26)	(26)	(21)	(27)	(25)	(29)
Leverage Ratio	0.145	0.274	0.152	0.202	0.302	0.165	0.302	0.217	0.236
	(0.128)	(0.242)	(0.133)	(0.143)	(0.277)	(0.139)	(0.279)	(0.196)	(0.211)
Cash / Total Assets	0.0732	0.118	0.0978	0.0664	0.0498	0.0821	0.0745	0.110	0.0785
	(0.0285)	(0.0535)	(0.0488)	(0.0265)	(0.0235)	(0.0460)	(0.0288)	(0.0637)	(0.0444)
Observations	1326	2176	9351	678	5215	1087	6330	12084	770

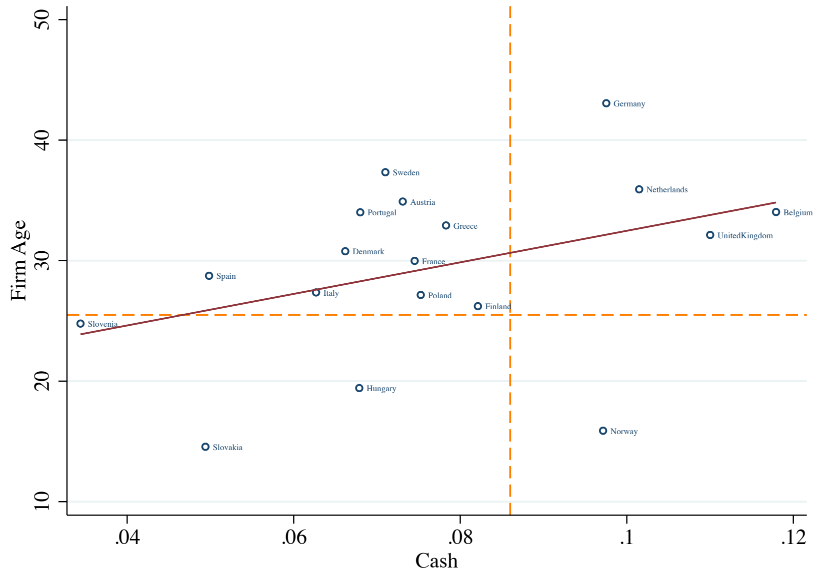
Panel B: Countries HU – SK

	(1) HU	(2) IT	(3) NL	(4) NO	(5) PL	(6) PT	(7) SE	(8) SI	(9) SK
Total Revenue	57.42	95.87	106.4	94.27	58.35	72.17	105.9	78.80	79.14
	(46.45)	(105.7)	(112.1)	(106.3)	(49.30)	(64.62)	(110.9)	(74.55)	(84.60)
Number of Employees	565.4	346.7	322.5	350.2	533.9	457.6	324.0	499.0	714.9
	(457)	(332)	(305)	(320)	(449)	(411)	(325)	(423)	(750)
Total Assets	45.75	104.1	83.52	86.93	47.62	76.05	72.91	76.07	59.45
	(30.70)	(93.05)	(59.52)	(48.34)	(35.12)	(54.64)	(58.15)	(62.94)	(49.82)
Firm Age	19.48	27.42	36.32	15.88	27.17	34.02	37.72	24.79	14.55
	(19)	(26)	(27)	(18)	(19)	(29)	(29)	(23)	(15)
Leverage Ratio	0.124	0.273	0.397	0.252	0.248	0.242	0.195	0.216	0.224
	(0.0921)	(0.234)	(0.365)	(0.219)	(0.219)	(0.207)	(0.153)	(0.206)	(0.193)
Cash / Total Assets	0.0668	0.0627	0.101	0.0975	0.0753	0.0680	0.0708	0.0344	0.0494
	(0.0313)	(0.0320)	(0.0442)	(0.0405)	(0.0369)	(0.0239)	(0.0239)	(0.0114)	(0.0199)
Observations	2795	8830	1573	511	5223	2179	1504	592	1045

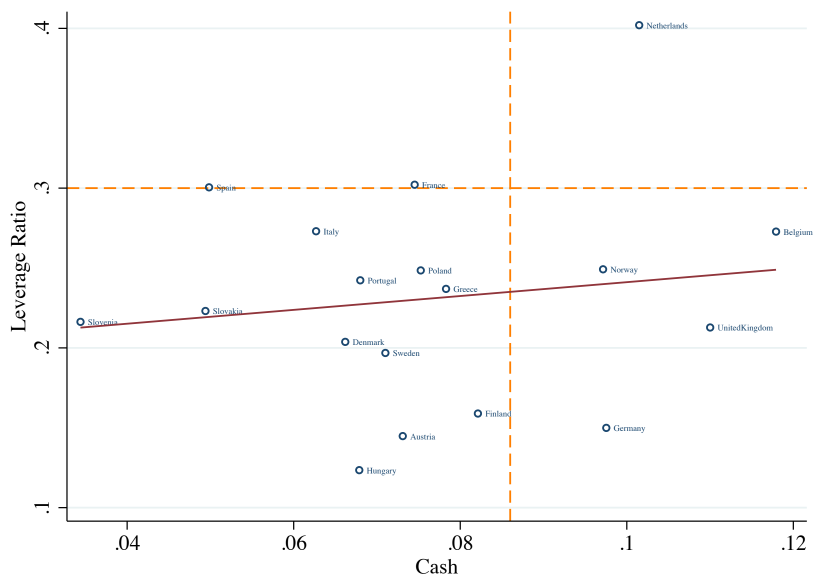
Notes: Sample years include 2010-2018. The medians are in parentheses. The sample is restricted to comparable firms used in my main analysis (see Appendix F). Leverage ratio is defined as current liabilities divided by total assets. Cash is defined as cash equivalent assets scaled by total assets. The 18 OECD countries include: Austria (AT), Belgium (BE), Denmark (DK), Finland (FI), France (FR), Germany (DE), Greece (GR), Hungary (HU), Italy (IT), the Netherlands (NL), Norway (NO), Poland (PL), Portugal (PT), Slovakia (SK), Slovenia (SI), Spain (ES), Sweden (SE), and the United Kingdom (GB).

Figure F: Cash Constraints (OECD countries)

Panel A: Firm Age vs. Cash



Panel B: Leverage vs. Cash



Notes: Panel A of this figure shows the relationship between firms' age and cash (cash equivalent assets scaled by total assets) across 18 OECD countries in Europe. Panel B of this figure shows the relationship between firms' leverage (current liabilities scaled by total assets) and cash across 18 OECD countries in Europe. The sample restriction is described in Appendix F. The orange dashed lines indicate the average firm age, leverage ratio, and cash for the comparable sample in Korea.