Capital Gains Taxes and Real Corporate Investment: Evidence from Korea

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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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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

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[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).

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 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

[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 V.

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 I describes the institutional background. Section II discusses a conceptual framework. Section III defines my empirical design. Section IV presents results, while Section V discusses economic interpretations. Section VI concludes.

I. 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.

A. 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

[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.

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 IV.F.

B. Rules on Firm Size

From 2009 to 2014, the government enforced the following rules for determining firm size: For the main sectors (see Section III.B) 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 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 IV.C). 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 III.A.

II. 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

[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.

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:

(1)

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

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

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 $\alpha < 1$.

A. 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:

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

[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^*.

[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.

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.

B. 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:

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

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

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.

III. 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.

[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.

A. 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.

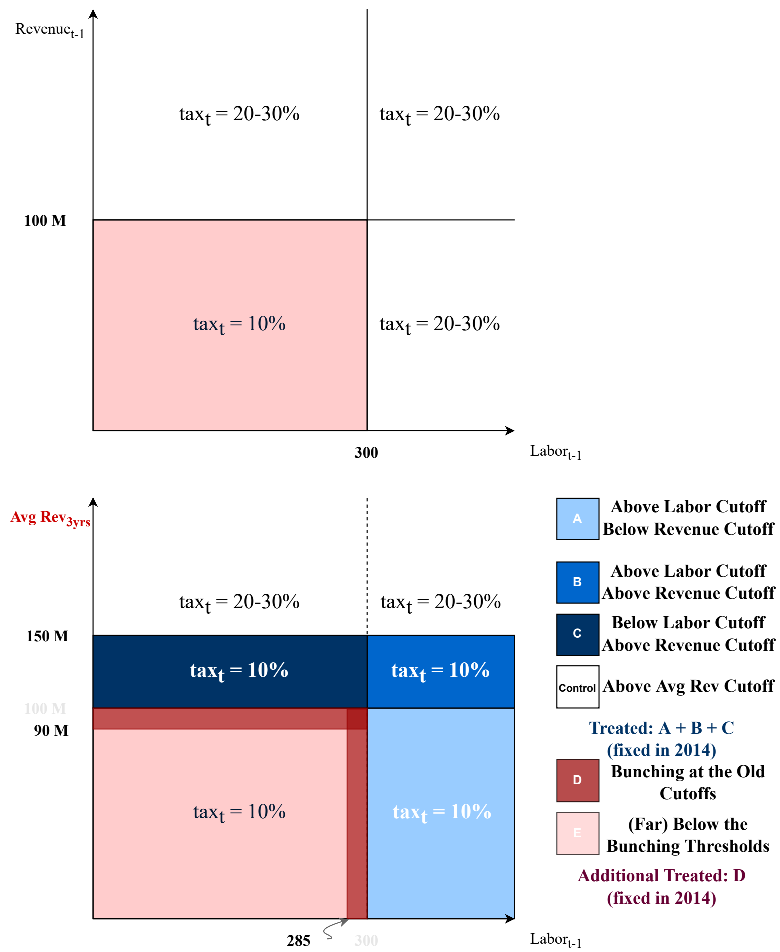
On the other hand, firms above the new threshold, whose size was unaffected by the reform, serve as the control group, given that there was no change in their incentive to invest.[9] Therefore, my main analysis sample consists of the first type of treated firms that experienced a tax cut, while the control 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

[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.

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.

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.

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 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 IV.

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

[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.

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.

B. 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

[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.

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.

C. 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 (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).

D. 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).

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 III.

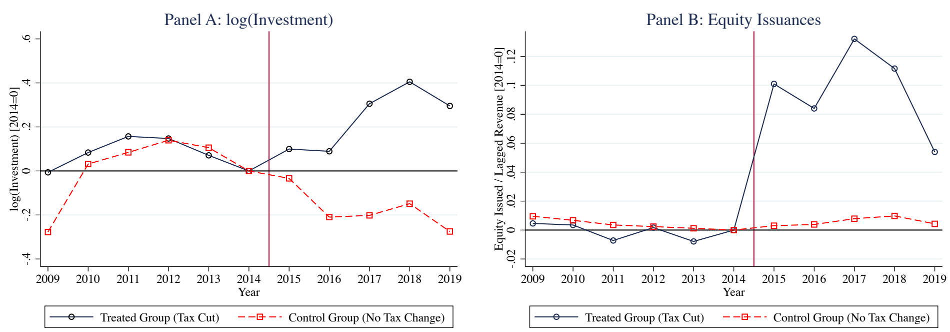
IV. Results

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

A. 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.

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.

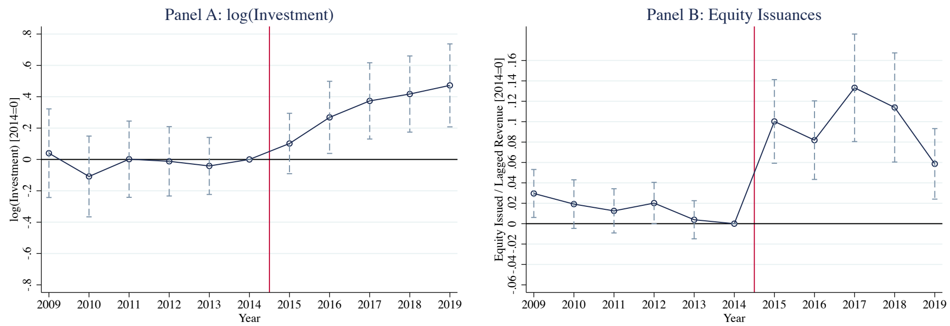
Panel A in Figure 3 plots the coefficients θ_τ, where $\tau \in (2009, ..., 2019)$, for log(investment) as in equation (6). The graph shows a parallel pre-trend on investment between the affected and unaffected firms, as the coefficient estimates are close to zero prior to the reform. Moreover, positive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

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

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.

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

	Investment	Capital Structure		
	(1)	(2)	(3)	(4)
	log(CAPEX)	Equity Issuance	Dividend Payouts	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 III. 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.

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.

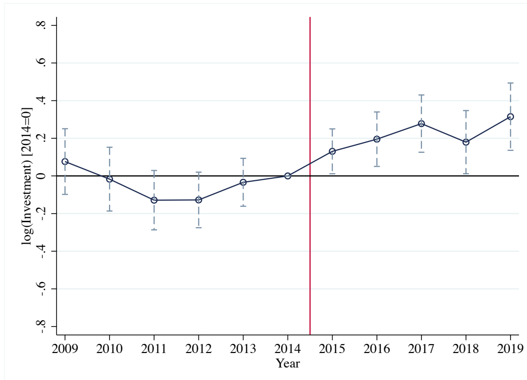
Column (2) of Table 2 shows that the affected firms increased new equity issuances by 8.5 cents per dollar of lagged revenue, which is consistent with the investment response. Panel B in Figure 3 plots the coefficients θ_τ, where $\tau \in (2009, ..., 2019)$, for equity issuances. As the graphs show, 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.

FIGURE 4. TAX EFFECTS ON log(INVESTMENT): LISTED AND PRIVATE FIRMS



Notes: This figure shows the coefficients on $Treated \times Time$ 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.

TABLE 3—RESULTS ON INVESTMENT (PUBLICLY LISTED AND PRIVATE FIRMS)

	Listed Firms	Private Firms	Listed and Private Firms	
	(1) log(CAPEX)	(2) log(CAPEX)	(3) log(CAPEX)	(4) 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 III. 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.

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):

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

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

(10)

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

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.

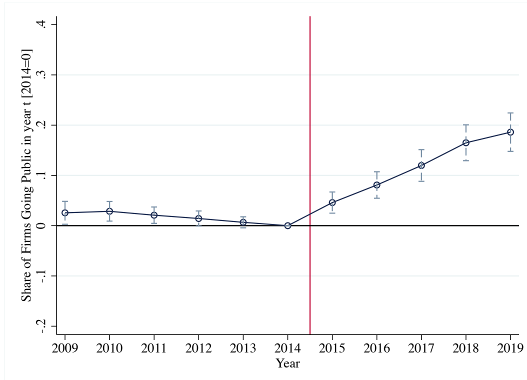
The model assumes that the cost of capital goes down for every firm when capital gains tax rates decrease. In reality, however, certain firms may be less sensitive to lower capital gains taxes depending on their cash-constraints (see Section IV.E). Thus, the difference-in-differences estimates of the 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.

B. *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.

Figure 5. Tax Effects on Share of Firms Going Public



Notes: This figure shows the coefficients on $Treated \times Time$ 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.

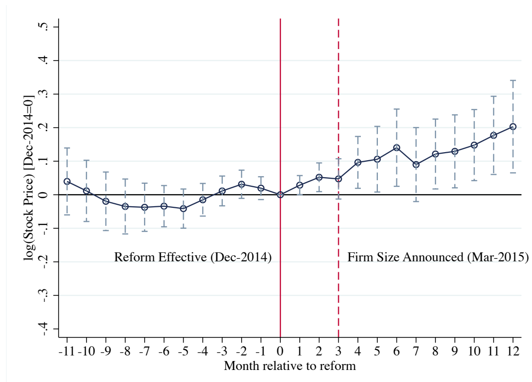
Table 3 reports the difference-in-differences estimates of the tax effects on the share of firms that went public. Column (4) shows that 14.4 percent of treated firms went public before the reform among the treated group, and a tax cut is associated with a 10 percentage points increase in the share of firms that went public after the reform, relative to the control group. In Section IV.E, 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.

C. 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.

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.

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.

D. 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 III.A) 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.

TABLE 4—RESULTS ON STOCK PRICES (PUBLICLY LISTED FIRM)

	Stock Prices	
	(1) log(Price)	(2) 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 III. 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.

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).

E. 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.

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:

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

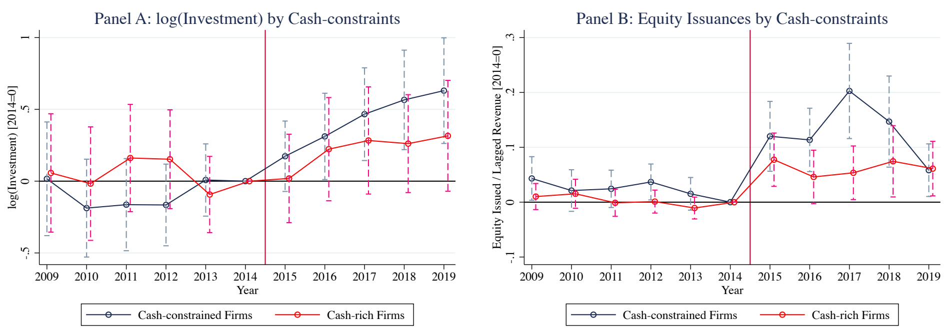
where $CR_i = 1$ if a firm i is above the median (cash-rich) in the measure of cash-constraint (fixed at the reform year of 2014), and the rest of variables are as defined in equation (7). θ_1 captures the tax effects for cash-constrained firms ($CR_i = 0$) and θ_2 captures the difference in the tax 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

[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.

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 IV.E. 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.

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 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

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

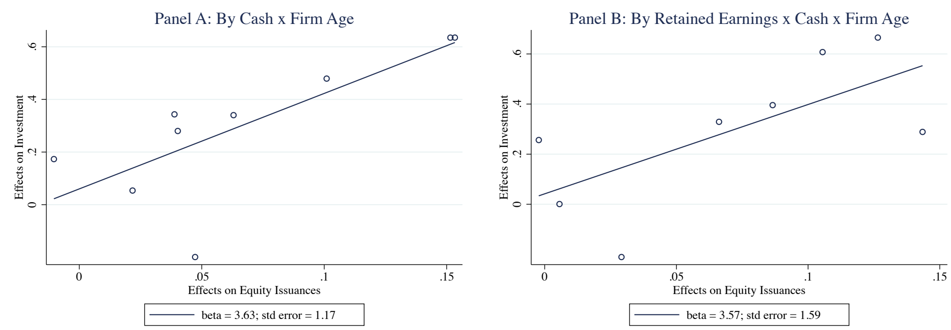
Notes: This table reports the tax effects on investment and capital structure based on specification (11). The dummy for $Treated_i$ equals 1 if a firm i had a tax reduction, as explained in Section III. The dummy for $post_t$ equals 1 if the time period is after the end of the reform year (2014). The dummy for CR_i is 1 if the firm is cash-rich firm, as defined in Section IV.E. 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.

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 IV.A, 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.

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.

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.

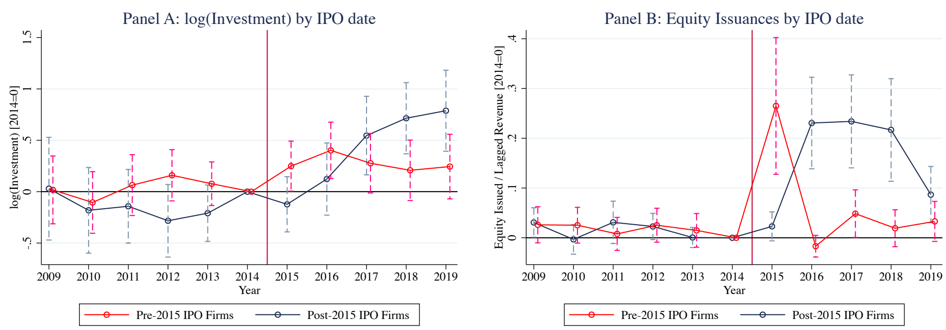
The reform provided tax incentives for privately held treated firms to go public. By going public, treated private firms can further reduce their effective capital gains tax rates; these incentives might be stronger for private firms that had a larger share of small shareholders. As Section III.B shows,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

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 IV.E. 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.

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.

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.

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 III. 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 IV.E. 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.

F. 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.

V. 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 I, 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

[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.

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 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 IV.C). 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.

VI. 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.

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[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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