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Dividend taxes and investment efficiency: Evidence from the 2003 U.S. personal taxation reform☆

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ABSTRACT

We examine the effect of a large dividend tax cut on corporate investment efficiency by exploiting the 2003 personal taxation reform in the U.S. as a quasi-natural experiment. Using a difference-in-differences approach based on the probability that a firm's marginal investor was an individual investor, we show that the 2003 dividend tax cut significantly improved the investment efficiency of U.S. listed firms. However, we find no evidence that the dividend tax cut increased the level of investment of U.S. listed firms. Further, we show that the tax cut increased investment efficiency by mitigating agency problems associated with the excessive free cash flows of overinvesting firms and by relaxing the financial constraints of underinvesting firms.

1. Introduction

Do dividend taxes affect corporate investment behavior? Although there exists a vast body of literature regarding the effects of dividend taxes on the level of corporate investment, the cost of equity, and optimal capital structure (e.g., Dhaliwal et al., 2007a; Chetty and Saez, 2005; Lin and Flannery, 2013), little is known about whether such taxes affect firms' investment efficiency. Since investment efficiency refers to the extent to which firms invest in projects with positive NPVs (net present values), this question is important for corporate managers, policymakers, and academics alike. In this paper, we address this question by testing whether a large dividend tax cut improves the investment efficiency of overinvesting and underinvesting firms. While several recent studies (e.g., Becker et al. (2013) for an international sample of listed firms and Alstadsæter et al. (2017) for Swedish unlisted firms) examine the effect of dividend tax cuts on the allocation of investment across firms (i.e., whether firms with excess cash holdings invest less, while cash-poor firms invest more after a dividend tax cut), no study has separately examined the effect of a dividend tax cut on the *investment efficiency* of over- and underinvesting U.S. public firms.

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To test whether dividend taxes affect firms' investment efficiency, we need to exploit a large change in the dividend tax rate. In this paper, we utilize a large dividend tax cut in the U.S. in 2003 as a natural experiment in our empirical tests. The Jobs Growth Tax Relief Reconciliation Act (JGTRRA) of 2003, or the 2003 tax reform for short, in the U.S. reduced the maximum federal tax rate of individual dividend income from 38.5% to 15%.

The rationale behind the 2003 tax reform was supported by the "traditional view" of neoclassical corporate taxation theory (e.g., Harberger, 1962; Feldstein, 1970; Poterba and Summers, 1984), which predicts that by reducing firms' cost of capital, permanent dividend tax cuts induce firms to issue new investment funds and *increase* investment.[1] The effects of the 2003 U.S. dividend tax cuts on various determinants of corporate investment have been widely studied. Dhaliwal et al. (2007a) report that the 2003 dividend tax cut reduced firms' cost of equity, while Chetty and Saez (2005) find that the tax cut caused firms to pay out more dividends. These results seem consistent with "traditional view" neoclassical models.

However, Yagan (2015) reports that the 2003 U.S. dividend tax cut caused no change in corporate investment and a small change in the cost of capital. He concludes that the dividend tax cut did not have any significant near-term effects on U.S. private firms. Similarly, Alstadsæter et al. (2017) also report that Sweden's 2006 dividend tax cut of 10 percentage points for closely held corporations and 5 percentage points for widely held corporations did not affect aggregate investment. These results seem consistent with the "neutrality view" neoclassical models (e.g., King, 1977; Auerbach, 1979; Bradford, 1981) in which new investments are assumed to be financed by internal funds (i.e., retained earnings) and thus the dividend tax is irrelevant (i.e., *neutral*) for corporate investment decisions.[2]

A recent "agency conflict" model (e.g., Chetty and Saez, 2010), which nests both the "traditional view" and "neutrality view" neoclassical models and captures typical agency problems commonly observed in reality, suggests that a dividend tax has heterogeneous effects on corporate investment and its efficiency across firms. Their model predicts that a dividend tax cut raises the *investment efficiency* of financially unconstrained firms with an agency problem (i.e., an excessive free cash flow problem *à la* Jensen (1986)) as it reduces these firms' incentive to overinvest in negative NPV projects. Thus, financially unconstrained firms invest less following a tax cut. In contrast, financially constrained firms (with minimal agency problems) invest more as the dividend tax cut reduces the cost of new equity issues, increasing the efficiency of investment by relieving underinvestment.

This model is based on the agency theory of the firm laid out by Jensen and Meckling (1976) and its main logic and predictions are clear-cut: Agency conflicts between managers and shareholders stem from managers' pursuit of perks and pet projects for private benefits at the expense of shareholders. Shareholders can motivate managers to invest optimally through costly monitoring and/or pay-for-performance compensation schemes. In this model, when the dividend tax rate is high, managers have a strong preference for keeping funds in the firm to expand the scope for perks and rent extraction. A dividend tax cut, however, increases managers' preference for dividends relative to perks and motivates increased monitoring by large shareholders. These economic forces are stronger when managers own more shares of the firm and when the firm is more closely monitored by large shareholders. The model particularly fits well with cash-rich public firms with excessive free cash flows, which are likely to overinvest in suboptimal projects. When there is a large dividend tax cut, the model predicts that managers of these firms are likely to increase dividends and decrease investments in pet projects. Therefore, a large cut in the dividend tax will mitigate the free cash flow problem, thereby lowering the likelihood of overinvestment and improving investment efficiency.

In this paper, building on the implications from the "agency conflict" model in Chetty and Saez (2010), in the context of firm investments and their efficiency, we focus on heterogeneous corporate responses to a large dividend tax cut applied to taxable individual investors. Our paper is the first to examine the effect of dividend tax cuts on the investment efficiency of over- and underinvesting firms. The closest study to ours is Alstadsæter et al.'s (2017) study based on a sample of Swedish private firms, as both studies contain empirical tests motivated by the agency model of Chetty and Saez (2010). Alstadsæter et al. (2017) find that after a dividend tax cut, cash-constrained firms increase investment, while cash-rich firms decrease investment. Although their results seem consistent with the agency model's predictions, we feel that their sample is not suitable to fit the agency model. For example, if we focus on cash-rich firms' overinvestment as a manifestation of investment in perks, then, after a dividend tax cut, widely held firms will decrease investment more than will closely held firms that are similar to owner-manager firms with minimal agency conflicts, in the spirit of Jensen and Meckling (1976). Unfortunately, however, such a comparison is not possible because Sweden's 2006 tax reform lowered the dividend tax rate for widely held firms by 5 percentage points to 25%, while reducing it by 10 percentage points to 20% for closely held firms. Due to the differential dividend tax cuts, Alstadsæter et al. (2017) find that in contrast to the predictions of the agency model, closely held cash-rich firms decreased investment quite significantly, while widely held cash-rich firms showed only a marginally significant decrease in investment. It appears that the tax cut size differential drives their results. If the same magnitude dividend tax cut was implemented for all private firms in Sweden, we would expect quite the contrary results based on the agency model: widely held cash-rich firms would decrease investment much more than closely held cash-rich firms. Therefore, we still do not know if the agency model's predictions are borne in the data that contain modern corporations characterized by agency conflicts resulting from diffuse ownership.

[1] This view is commonly referred to as the "old view" in the tax literature.

[2] This view is known as a "new view" or "trapped equity view" in the tax literature.

Since the main focus of Chetty and Saez's (2010) agency model is on cash-rich modern public firms with diffuse shareholder ownership, we believe using a sample of U.S. public firms that were subject to the same size large dividend tax cut would be more suitable for testing the agency model's predictions than using a sample of private firms. Our paper's main objective is to fill this gap in the literature: to test the predictions of the agency model by using widely held public firms characterized by agency conflicts. Our strategy is to exploit a large dividend tax cut in the U.S. in 2003 as a natural experiment. To estimate the impact of the 2003 tax reform on firms' investment efficiency, we examine the changes in the investment efficiency of over- and underinvesting firms from before 2002 to after 2004.[3] We identify the effect of the reform on firms' investment behavior using a difference-in-differences (DD) approach, exploiting the fact that the tax reform affects only individual investors but not corporations or nonprofit investors. In a similar way to Lin and Flannery's (2013) approach, we define treatment status based on the percentage of individual ownership measured as one minus the percentage of a firm's shares held by institutional investors, as reflected in information available from 13F filings. Specifically, firms that have individual ownership higher than the sample median in 2002 are classified into the treatment group, while firms that have individual ownership less than the sample median in 2002 are classified into the control group.

We find that the 2003 tax cut led to a significant reduction in the tendency to overinvest and underinvest, although it did not cause a significant increase in the level of corporate investment on average. First, we find that firms whose marginal owner was likely to be an individual (*Treated firms*) increased investment efficiency more than firms whose marginal owner was likely to be an institution (*Control firms*)—average investment inefficiency declined by 1.8% of total assets, which is approximately 17.1% of the sample average, i.e., 10.5% of total assets—in response to the tax reform. We further document that for the sample of overinvesting firms, the 2003 dividend tax cut increased their average investment efficiency by 4.5% of total assets. For the sample of underinvesting firms, the dividend tax cut increased their average investment efficiency by only 0.6% of total assets. However, consistent with Yagan (2015), the 2003 dividend tax cut did not significantly increase the level of corporate investment.

To investigate the underlying economic mechanisms through which the 2003 tax reform affects the investment efficiency of overinvesting and underinvesting firms, we perform a series of tests. First, we examine whether more severe agency conflicts magnify the effects of the 2003 tax reform on investment efficiency. We find that a high degree of agency conflicts, as measured by a lower number of blockholders or low CEO ownership, increases the effect of the 2003 tax cut on investment efficiency only for overinvesting firms. The results are consistent with Chetty and Saez's (2010) prediction that the dividend tax cut will lower the problem of overinvesting free cash flows by reducing shareholder-manager agency conflicts. Second, we perform DD analyses for the effect of the 2003 tax cut on two financial constraint measures—Kaplan and Zingales' (1997) KZ index and Hadlock and Pierce's (2010) SA index—and find that the tax reform relaxed financial constraints, as measured by both indices for underinvesting firms. The results suggest that the dividend tax cut mitigates underinvestment problems by relaxing the financial constraints of underinvesting firms. Then, we examine the relative effects of the dividend tax cut on the costs of debt and equity and find that the tax reform lowered the costs of equity significantly but did not lower the costs of debt significantly. These results are consistent with the findings of Dhaliwal et al. (2007a) and Lin and Flannery (2013).

We contribute to the literature in several ways. First, our results can shed light on the debate between the "traditional view" and the "neutrality view" models. While the results indicating that the 2003 U.S. tax reform did not have a significant effect on firm-level investment or aggregate investment are consistent with the "neutrality view" neoclassical models, the results indicating that the dividend tax cut reduced underinvestment problems by relaxing financing constraints are consistent with the "traditional view" neoclassical models. However, these "neutrality view" and "traditional view" neoclassical models do not explain our results that the dividend tax cut reduces the overinvestment problems by mitigating free cash flow problems. Rather, our results are consistent with Chetty and Saez's (2010) agency conflict model, which nests both traditional and neutrality view neoclassical models. Thus, our results suggest that the agency conflict model of Chetty and Saez (2010) can provide a full picture of the effects that the dividend tax cut has on corporate investment and its efficiency.

Second, given that the 2003 U.S. personal taxation reform affected only individual investors but not institutional investors, we employ a DD approach to identify the effect of the reform on firms' investment behavior by defining treatment status based on the percentage of individual ownership. No prior study has used this identification approach to examine the effect of the 2003 dividend tax cut on corporate investment efficiency.

Third, this study is the first to examine the effects of the 2003 dividend tax cut on the measures of corporate investment efficiency developed in the accounting literature. A few prior studies simply examine whether cash-poor firms increase investment following the dividend tax cut while cash-rich firms decrease investment. Unlike Alstadsæter et al.'s (2017) study based on Swedish data, we directly estimate the effects the dividend tax cut has on not only firms' investment levels but also firms' investment efficiency. In addition, we use the data on U.S. public firms that have significant cross-sectional variation in corporate governance (Gompers and Metrick, 2001; Gompers et al., 2003). Unlike Campbell et al. (2013), who examine the effect of the 2003 tax cut on capital expenditures of U.S. public firms by studying a very short sample period (i.e., one year before and after the tax cut), we focus on examining the effect of the 2003 tax cut on the efficiency of U.S. public firms' investment decisions by investigating longer sample windows (i.e., two to four years before and after the tax cut). Note that it takes a long period for firms' investment decisions and their efficiency to be affected by the tax cut.

[3] We define overinvesting firms and underinvesting firms following a number of accounting studies (e.g., Biddle et al., 2009; Chen et al., 2011; Richardson, 2006) and estimate those effects for over- and underinvesting firms separately.

In sum, we believe that by employing an improved empirical framework, including an accounting measure of investment efficiency, a longer sample period, data from U.S. public firms with sufficient variation in corporate governance, and subsamples based on whether firms overinvest or underinvest, this study enhances our understanding of the mechanisms through which a large dividend tax cut helps improve corporate investment efficiency and thus increases corporate valuation. We argue that the sample of U.S. *public* firms with diffuse ownership is more suited to test the implications of the "agency conflict" model (e.g., Chetty and Saez, 2010) than the sample of Swedish *private* firms used by Alstadsæter et al. (2017). Compared to the samples used in previous studies, the sample of U.S. public firms with diffuse ownership and substantial agency problems allowed us to examine the main implications of the "agency conflict" model (e.g., Chetty and Saez, 2010) in a cleaner setting.

This paper proceeds as follows. In Section 2, we discuss the key literature on the effect of tax reform on corporate investment and its determinants and derive hypotheses on the effects of the dividend tax cut on corporate investment and investment efficiency. Section 3 explains our research design. We then describe the sample selection and variable definitions and present descriptive statistics. In Section 4, we report our main empirical findings and robustness tests. In Section 5, we present the results of analyses designed to test economic mechanisms. In Section 6, we present the results of the analyses designed to examine cross-sectional differences in the effect of the 2003 tax cut and discuss the results of the robustness tests based on alternative U.S. dividend tax rate changes. We conclude in Section 7.

2. Development of hypotheses

The theoretical predictions on the effect of dividend taxation on corporate investment and its efficiency are not uniform. Two types of neoclassical models, i.e., the "traditional view" (e.g., Harberger, 1962; Feldstein, 1970; Poterba and Summers, 1984) and the "neutrality view" (e.g., King, 1977; Auerbach, 1979; Bradford, 1981), and agency-theoretic models (e.g., Chetty and Saez, 2010) are found in the literature. The agency-theoretic models based on Jensen and Meckling's (1976) intuition and the neutrality view neoclassical models provide predictions mainly for firms that are financially unconstrained (i.e., those with excess cash holdings), while traditional view neoclassical models provide predictions pertinent to firms that are financially constrained. In this section, we discuss the related literature and derive testable hypotheses on the effects of dividend taxation on corporate investment and investment inefficiency, i.e., overinvestment and underinvestment. In this paper, investment efficiency refers to the extent to which firms undertake only projects with positive NPVs. We directly measure investment efficiency, which is well developed in the accounting literature (e.g., Biddle et al., 2009; Richardson, 2006; Chen et al., 2011).

2.1. Dividend tax cut and corporate investment

The effects of the 2003 U.S. dividend tax cut on various determinants of corporate investment have been widely studied. Dhaliwal et al. (2007a) report that the 2003 dividend tax cut reduced firms' cost of equity and Chetty and Saez (2005) find evidence that the tax cut caused firms to pay out more in equity income. These results seem consistent with "traditional view" neoclassical models (e.g., Harberger, 1962; Feldstein, 1970; Poterba and Summers, 1984), suggesting that by reducing firms' cost of capital, permanent dividend tax cuts induce firms to raise new investment funds and increase investment. However, Yagan (2015) reports that the 2003 U.S. dividend tax cut caused no change in corporate investment and a small change in the cost of capital and argues that the dividend tax cut did not have any significant near-term effects. The results seem consistent with the "neutrality view" neoclassical models (e.g., King, 1977; Auerbach, 1979; Bradford, 1981).

Given that our sample and empirical framework are different from Yagan's (2015) sample and empirical framework, we revisit the question of whether the dividend tax cut increased the level of firm-level investment. While we use a sample of U.S. firms listed on major stock exchanges, Yagan selects a sample of U.S. private firms that filed corporate income tax returns to the Internal Revenue Service. Thus, we revisit the question of whether the tax reform affected the level of firm-level investment by using the DD framework. To identify the firms that are affected by the tax reform and the firms that are not affected, we use an approach similar to Lin and Flannery's (2013) approach that relies on the assumption that the probability that a firm's marginal investor is an individual is equal to the percentage of ownership by individual investors. Notably, the 2003 dividend tax cut is applicable only to taxable individual investors, not to tax-exempt individual investors or institutional investors. Previous studies, such as Blouin et al. (2011), Brown et al. (2007), Dhaliwal et al. (2007a), and Dhaliwal et al. (2007b), make similar assumptions.

Based on these discussions, we formulate the following *null* hypothesis for the effect of the 2003 dividend tax cut on corporate investment:

Hypothesis 1. The 2003 dividend tax cut did not increase corporate investment.

2.2. Dividend tax cut and underinvestment

In the first instance, consider financially constrained firms that require financing investments by raising external equity or risky debt. The "traditional view" neoclassical models (e.g., Harberger, 1962; Feldstein, 1970; Poterba and Summers, 1984) predict that permanent dividend tax cuts reduce underinvestment for those firms. In their model, a dividend tax cut reduces

the cost of external capital and thus enables financially constrained firms to finance investments by issuing equity or risky debt at lower costs, resulting in a reduction in underinvestment.

Similarly, agency conflict models (e.g., Chetty and Saez, 2010) predict that a dividend tax distorts the behavior of financially constrained firms. In their model, an increase in the dividend tax rate reduces equity issues and investment for financially constrained firms. Intuitively, a dividend tax cut raises the marginal product of investment but does not affect the price of investment for financially constrained firms. Therefore, in response to a cut in the dividend tax rate, firms increase investment, issue more equity, and pay more dividends, consistent with "traditional view" predictions (e.g., Poterba and Summers, 1984).

Based on these discussions, we formulate the following hypotheses for the effect of the 2003 dividend tax cut on the investment efficiency of underinvesting firms:

Hypothesis 2A. The investment efficiency of underinvesting firms improved after the 2003 dividend tax cut.

Hypothesis 2B. The 2003 dividend tax cut relaxed financial constraints and lowered the costs of equity and debt.

2.3. Dividend tax cut and overinvestment

Now, consider financially unconstrained firms (i.e., firms with excess cash holdings or abundant free cash flows) that fund new investments by retaining earnings and issuing riskless debt rather than by issuing equity or risky debt. The "neutrality view" neoclassical models (e.g., King, 1977; Auerbach, 1979; Bradford, 1981) predict that permanent dividend tax cuts lead to totally different outcomes for those firms. In these models, dividend taxes do not affect corporate investment and thus no change in corporate investment or no reduction in overinvestment is predicted after a large cut in dividend taxes.

However, "agency conflict" models (e.g., Chetty and Saez, 2010) predict that permanent dividend tax cuts reduce the overinvestment of free cash flows. Chetty and Saez (2010) propose a model of dividend and corporate income taxation based on the agency theory of the firm (Jensen and Meckling, 1976). The critical feature of the model is a divergence between the preferences of managers and shareholders that arises from perks and pet projects. Shareholders use costly monitoring and performance-sensitive compensation to incentivize managers to invest and pay out dividends. Consistent with Shleifer and Vishny (1986, 1997), only large shareholders choose to monitor the firm in equilibrium.

Their "agency conflict" model of the firm shows that dividend taxation induces managers to undertake unproductive investments (i.e., overinvest) by retaining earnings. In their model, dividend taxes create a deadweight cost by distorting the tradeoff between pet project investments (i.e., unproductive investments, empire building, and perks) and dividend payouts, even if the marginal source of investment is retained earnings, i.e., firms have abundant free cash flows. A dividend tax cut increases the manager's after-tax return on profitable projects, while leaving the return on pet projects unaffected. Thus, in firms with abundant free cash flows, the manager substitutes from investing in perks to the profitable project, resulting in an increase in profitable corporate investment, while the investment in pet projects falls. In addition, they also show that a dividend tax cut increases the amount of monitoring by large shareholders. Therefore, the dividend tax cut alleviates the problem of overinvesting free cash flows.

Based on these discussions, we formulate the following hypotheses for the effect of the 2003 dividend tax cut on the investment efficiency of overinvesting firms:

Hypothesis 3A. The investment efficiency of overinvesting firms improved after the 2003 dividend tax cut.

Hypothesis 3B. More severe shareholder−manager agency conflicts magnified the effect of the 2003 dividend tax cut on the investment efficiency of overinvesting firms.

2.4. Substitution between dividends and share repurchases

One could argue that our discussion is not complete because the Chetty and Saez's (2010) model on which our discussion is based does not explicitly consider the role of share repurchases, which give a firm a means to return money to shareholders without entailing a tax penalty. Chetty and Saez (2010, appendix) extended their model to allow costly share repurchases and show that agency costs depend on the elasticity of total payouts (i.e., the sum of share repurchases and dividends) with respect to dividend taxes. In the extended model, pet project investment is determined by total payouts, not just dividend payments.[4] Thus, the key insights from the basic theoretical model we use in this paper can be generalized to the cases including share repurchases. The main predictions carry through as long as total payouts increase after the 2003 tax cut. If the substitution between dividends and share repurchases occurred after the 2003 dividend tax cut, we would not expect improvements in the investment efficiency of overinvesting firms. Brown et al. (2007) present suggestive evidence that the 2003 dividend tax cut might have led to the substitution of dividends for share repurchases by showing that the firms that initiated dividends in 2003 tended to reduce share repurchases. However, Chetty and Saez (2006) raise some concerns with their sample selection and methods and provide evidence that both dividends and repurchases increased and that thus total

[4] See the appendix of Chetty and Saez (2010) for the model incorporating costly share repurchases.

payouts increased substantially in response to the 2003 dividend tax cut.[5] Yagan (2015) also finds that the 2003 dividend tax cut increased total payouts. Given the weight of the existing evidence leaning toward increased total payouts in the wake of the 2003 tax cut, we claim that our results are consistent with the extended agency theoretic model that includes costly share repurchases.

3. Empirical framework

3.1. Research design

3.1.1. History of U.S. dividend tax reforms and evaluation of test periods

The United States has experienced frequent tax rate changes regarding dividends and other forms of personal income, as summarized in Panel A of Table 1. The top marginal tax rate on dividends has changed eight times from 70% in 1980 to 20% in 2013. Not all of the changes, however, are appropriate to test the impact of dividend taxation on corporate investment efficiency. We use the following criteria to choose the best study setting that allows us to avoid any possible confounding effects of other tax rate changes. First, in terms of the percentage of the previous tax rate, a reform should affect the top marginal tax rate on dividends (t_{div}) more significantly than it affects the top marginal tax rate on ordinary income (t_{oi}) and the top marginal tax rate on capital gains (t_{cg}) (Criterion 1). Second, relative tax rates (t_{div}/t_{cg} or t_{div}/t_{oi}) should be affected by a reform (Criterion 2). Third, we require that corporate tax rates do not change during the period (Criterion 3). Fourth, there should be at least two years of data before and after a tax reform since it will take years for corporate investment to be affected by a tax reform (Criterion 4). In addition, we require that the magnitude of the tax rate change be substantial (i.e., medium or large). The results of the evaluation of each test period for legitimacy for the DD analyses are provided in Panel B of Table 1. In setting the test period for each tax reform, we attempt to retain as many years of data as possible around a tax reform, provided that we have minimum two years of data before and after the tax reform.

First, the 1981 reform, i.e., the Economic Recovery Act of 1981 (ERTA), involved a substantial decline in t_{div} from 70% to 50%, but this reform violates Criterion 2. The reform decreased t_{div}, t_{cg}, and t_{oi} in equal proportions and thus did not change t_{div}/t_{cg} or t_{div}/t_{oi} at all. In addition, firms faced a significant level of tax policy uncertainty around the reform. Exactly 3 months after the ETRA reform was signed into law, the 1982 reform, i.e., Tax Equity and Fiscal Responsibility Act of 1982 (TEFRA), that reversed some elements of the 1981 reform was introduced to the House and then eventually enacted in 1982. Thus, it is unlikely that the ETRA tax cut had a positive influence on corporate investment efficiency. Nevertheless, we examine the combined effect of the two reforms in 1981 and 1982 using the test period of 1978−1985. Although we do not expect that there was any significant effect on corporate investment efficiency generated by the two reforms, this analysis can shed light on the effect of tax policy uncertainty on corporate investment decisions.

Second, the 1986 reform, i.e., the Tax Reform Act of 1986 (TRA), involves a significant decline in t_{div} (and t_{oi}) from 50% to 28% and significant changes in t_{div}/t_{cg}. However, the reform violates Criterion 3, as corporate tax rates also declined during this period (i.e., 1988). In addition, capital gains tax increased during this period (i.e., in 1987). To minimize the effect of the change in the capital gains tax rate, we exclude 1987 from the test period. The reform raised the bottom tax rate from 11% to 15%. This was the first time in U.S. income tax history in which the top tax rate was lowered and the bottom rate was increased at the same time. These complications make it difficult to identify the effect of dividend tax rate reduction using the test sample. However, given that the dividend tax rate decrease was substantial, we examine the test period surrounding the 1986 reform (i.e., 1983−1989) for a robustness test.

Third, the 1990 reform, i.e., Omnibus Budget Reconciliation Act of 1990 (OBRA-90), involves a small increase in the top statutory tax rate on dividends and ordinary income from 28% to 31%. Although the period satisfies all four criteria, the change in the tax rate is quite small. Thus, we do not use the test period surrounding the 1990 reform (i.e., 1988−1992) for a robustness test.

Fourth, the 1993 reform, i.e., Omnibus Budget Reconciliation Act of 1993 (OBRA-93) also involves a moderate increase in the top tax rate on dividends and ordinary income from 31% to 39.6%, a small increase in the corporate income tax rate from 34% to 35%, and an increase in other taxes, such as fuel taxes. Thus, the test period violates Criterion 3. Given that the change in corporate tax rate was very small, we include the test period surrounding the 1993 reform (i.e., 1991−1996) as part of our robustness tests.

Fifth, the 2003 reform, i.e., Jobs and Growth Tax Relief Reconciliation Act (JGTRRA), involves a large drop in the top tax rate on dividends from 38.6% to 15%, a decline in the tax rate on capital gains from 20% to 15%, and a decline in the tax rate on ordinary income from 38.6% to 35%.[6] The reform satisfies all four criteria. Both absolute and relative changes in tax rates were the largest, and the period did not involve any changes in corporate tax rates. In addition, a sufficiently long period was subject to this tax regime. Thus, we use this test period surrounding the 2003 reform (i.e., 1999−2007) for our main analyses.

[5] Due to high volatility in aggregate share repurchases over time, however, Chetty and Saez (2006) note that existing data and methods cannot definitively answer the substitution question.

[6] Before the JGTRRA was enacted, the Economic Growth and Tax Relief Reconciliation Act (EGTRRA) lowered the top tax rate on both ordinary income and dividends from 39.6% to 38.6%.

Table 1
History of U.S. personal tax rates and evaluation of test periods.

Panel A. History of U.S. personal tax rates (1980–2020)

Period	t_{oi}	% chg.	t_{cg}	% chg.	t_{div}	% chg.	t_{div}/t_{oi}	Chg.	t_{div}/t_{cg}	Chg.
Pre-1981	0.700		0.280		0.700		1.00		2.50	
1982–1986	0.500	−28.6%	0.200	−28.6%	0.500	−28.6%	1.00	0.00	2.50	0.00
1987	0.385	−23.0%	0.280	40.0%	0.385	−23.0%	1.00	0.00	1.38	−1.13
1988–1990	0.280	−27.3%	0.280	0.0%	0.280	−27.3%	1.00	0.00	1.00	−0.38
1991–1992	0.310	10.7%	0.280	0.0%	0.310	10.7%	1.00	0.00	1.11	0.11
1993–1996	0.396	27.7%	0.280	0.0%	0.396	27.7%	1.00	0.00	1.41	0.31
1997–2000	0.396	0.0%	0.200	−28.6%	0.396	0.0%	1.00	0.00	1.98	0.57
2001–2002	0.386	−2.5%	0.200	0.0%	0.386	−2.5%	1.00	0.00	1.93	−0.05
2003–2012	0.350	−9.3%	0.150	−25.0%	0.150	−61.1%	0.43	−0.57	1.00	−0.93
2013–2017	0.396	13.1%	0.200	33.3%	0.200	33.3%	0.51	0.08	1.00	0.00
2018–2020	0.370	−6.6%	0.200	0.0%	0.200	0.0%	0.54	0.04	1.00	0.00

Panel B. Evaluation of test periods for legitimacy for difference-in-differences analyses

Reform year	Legislation	t_{div}	Size	Test period	Criterion 1	Criterion 2	Criterion 3	Criterion 4
1981	ETRA	Down	Medium	1978–1985	Y	N	Y	Y
1986	TRA	Down	Large	1983–1989 (ex. 1987)	Y	Y	N	Y
1990	OBRA-90	Up	Small	1988–1992	Y	Y	Y	Y
1993	OBRA-93	Up	Medium	1991–1996	Y	Y	N	Y
2003	JGTRRA	Down	Large	1999–2007	Y	Y	Y	Y
2012	ATRA	Up	Medium	2007–2017	Y	Y	Y	Y

Notes. Panel A summarizes the changes in the top tax rates on different kinds of personal income: t_{oi} denotes top tax rate on ordinary income; t_{cg} denotes top tax rate on capital gains; t_{div} denotes top tax rate on dividends (the same as that on ordinary income until 2003). Panel B summarizes the evaluation results of test periods for legitimacy for difference-in-differences analyses. We use the following criteria to choose the best study setting. First, a reform should affect the tax rate on dividends (t_{div}) more significantly than it affects the tax rate on ordinary income (t_{oi}) and the tax rate on capital gains (t_{cg}) in terms of the percentage of the previous tax rate (Criterion 1). Second, relative tax rates (t_{div}/t_{cg} or t_{div}/t_{oi}) should be affected by a reform (Criterion 2). Third, we require that corporate tax rates do not change during the period (Criterion 3). Fourth, there should be at least two years of data before and after a tax reform since it will take years for corporate investment to be affected by a tax reform (Criterion 4). "Y" indicates that the criterion is satisfied, while "N" indicates that the criterion is not satisfied. Full names of legislation are provided in Section 3.1.1.

Finally, the 2012 reform, i.e., American Taxpayer Relief Act of 2012 (ATRA), involves a modest increase in the top marginal tax rate on dividends and capital gains from 15% to 20% and an increase in the top marginal tax rate on ordinary income from 35% to 39.6%. The reform satisfies all four criteria. The reform did not lead to a change in t_{div}/t_{cg}, but a change in t_{div}/t_{oi}. Thus, we use the test period surrounding the 2012 reform (i.e., 2007–2017) for a robustness test. However, we expect the effect of the tax reform to be quite weak due to unchanged relative tax rates between dividends and capital gains.

Therefore, this study examines the test period surrounding the 2003 reform for the main DD analyses and uses the test periods that include those surrounding the 1981 reform, the 1986 reform, the 1993 reform, and the 2012 reform for robustness tests. As discussed above, the 2003 reform comprised the largest change in the top tax rate on dividends, large relative tax rate changes, a period that did not involve any changes in corporate tax rates, and a sufficiently long period subject to this tax regime.

3.1.2. Baseline DD design

The adjustment of corporate investment decisions and their efficiency may require significantly more time than the adjustments of leverage, cash holdings, or payouts. Thus, it would be better to use a longer sample period to test the influence of the tax cut on investment efficiency. However, during the longer time period, corporate investment may be affected by other unobserved factors such as time trends (Yagan, 2015) and business cycles (Poterba and Summers, 1984). Considering these problems, we use the DD method to estimate the effect of the 2003 tax reform on investment efficiency. An advantage of using the DD method is that we can mitigate the confounding effects arising from omitted trends (e.g., those arising from macroeconomic conditions) and unobserved cross-sectional differences (e.g., proportion of tax-paying individual investors).[7]

To study the impact of the 2003 tax reform on firm-level investment inefficiency, one needs to identify the firms that are affected by the reform and the firms that are not affected. The 2003 tax cut is applicable only to the equity income distributed to individual investors, so many shareholders, such as corporations, universities, and individual investors who held their equities in a tax-sheltered retirement account, were not affected. Theoretical models (e.g., Bell and Jenkinson, 2002; Lin and Flannery, 2013) show that a firm's marginal investors influence the firm's valuation, cost of equity, and optimal capital structure. Therefore, if a firm's marginal investor was an unaffected institutional investor when the 2003 tax cut was passed,

[7] The DD estimator combines a single cross-sectional difference estimator and a single time-series difference estimator to take advantage of both estimators' strengths. The cross-sectional comparison avoids the problem of omitted trends by comparing two groups over the same time period. The time series comparison avoids the problem of unobserved differences between two different groups of firms by looking at the same firms before and after the change. See Roberts and Whited (2013) for details.

there should be no effects on corporate investment and its efficiency. However, if a firm's marginal investor was an individual investor, the firm might have changed its investment decisions.

Our identification relies on the assumption that the reform impacted the tax burden of qualified individual investors without impacting institutional investors (e.g., investment advisors). Particularly, in line with Lin and Flannery (2013), we assume that the probability that a firm's marginal investor is an individual is equal to the percentage of ownership by individual investors.[8] Similar assumptions are made in previous studies such as Blouin et al. (2011), Dhaliwal et al. (2007a), and Dhaliwal et al. (2007b). As such, we utilize the level of individual ownership in 2002 to construct a proxy for the degree to which firms are affected by the 2003 tax reform.

To determine treatment status, we use the individual ownership proportion in 2002, where the individual ownership proportion is measured as one minus the proportion of a firm's shares held by institutional investors, as reported in the 13F filings. In line with the standard DD literature (e.g., Card and Krueger, 1994; Fang et al., 2014; Blouin et al., 2011; Yagan, 2015; Alstadsæter et al., 2017), we define treated firms and control firms using a time-invariant indicator variable. Specifically, the firms that have individual ownership higher than the sample median in 2002 are classified into the treatment group, while the firms that have individual ownership less than the sample median in 2002 are classified into the control group. We define a time-invariant indicator variable, $TREAT_i$, which equals one if firm i is in the treatment group and zero if firm i is in the control group.[9] We define an indicator variable, $POST_t$, which equals one if the firm-year observations are after 2003 (i.e., 2004–2007) and zero if firm observations are before 2003 (i.e., 1999–2002).

To test whether the 2003 tax reform had an impact on investment inefficiency, we estimate the following multivariate DD model:

$$INVEFF_{i,t} = \alpha + \beta TREAT_i \times POST_t + \mathbf{X}\Gamma + \eta_i + \theta_{j,t} + \xi_{i,t}, \tag{1}$$

where $INVEFF_{i,t}$ is firm i's investment efficiency in year t, as defined in Section 3.3.1, $TREAT_i$ is a variable indicating the treatment status, $POST_t$ is the posttreatment indicator, $\mathbf{X} = (x_1, x_2, \ldots, x_k)$ is a vector of the covariates, as described in Section 3.3.2, $\Gamma = (\gamma_1, \gamma_2, \ldots, \gamma_k)'$ is a vector of coefficients on the covariates, and β is the DD estimator. To address industry-year fixed effects ($\theta_{j,t}$) or unobserved time-varying differences that are heterogeneous across industries, we include industry-year dummies, where industries are defined following Fama and French's (1997) 48-industry classification. To address unobserved firm heterogeneity (η_i), we employ fixed effects regressions. We do not include $TREAT_i$ and $POST_t$ separately in the regression models since their effects are subsumed by firm fixed effects and industry-year fixed effects.

Our main focus is the coefficient, β, of the interaction term, $TREAT_i \times POST_t$. If β is positive and significant, it lends support to our hypothesis that the 2003 tax reform increases investment efficiency. Following Biddle et al. (2009), based on the sign of the residual in Eq. (2), we further divide firm-year observations into subsamples of overinvesting and underinvesting firms. We estimate Eq. (1) separately for each subsample to examine whether the 2003 tax reform impacted overinvesting and underinvesting firms differently.

3.2. Sample selection

Our sample consists of U.S. firms traded on the New York Stock Exchange, American Stock Exchange and Nasdaq. The sample period is from 1999 to 2007. Our sample period begins four years before the 2003 tax reform and ends four years after the reform. We obtain financial statement data from the Compustat North America database, stock market data from the CRSP database, and institutional ownership data from the Thomson-Reuters Institutional Holdings (13F) database, formerly known as the CDA-Spectrum database.

Following the literature, we exclude firms in the financial services sector (SIC codes between 6000 and 6999) since investment policies are quite different in the financial services sector and the sector is restricted by policies and regulations. If a firm in Compustat does not have institutional ownership data in the 13F database, we set the share of institutional ownership to zero. We exclude firm-year observations in 2003 from the main analyses since the 2003 tax reform was enacted on May 28th, 2003. To minimize the influence of outliers on the results, we winsorize all control variables at the 1st and 99th percentiles, using their distributions for each year. Our sample consists of 18,179 firm-year observations from 3051 listed firms, similarly to Lin and Flannery's (2013) sample. The number of firms ranges from a minimum of 1988 in 1999 to a maximum of 2605 in 2002.[10]

[8] See page 553 in Lin and Flannery (2013) for more details about all three assumptions made by them.

[9] We have two reasons for preferring the DD models with a treatment indicator rather than the DD models with a continuous treatment variable. First, the DD models with a treatment dummy provide an easier interpretation of the results. In particular, it is more convenient to evaluate the economic significance of the results. Second, the models with a treatment indicator based on the median would be a more conservative approach than the models with a continuous treatment variable (e.g., Campbell et al., 2013) or the models with other quantiles, such as quintiles (e.g., Alstadsæter et al., 2017). Nevertheless, as a robustness test, we estimate the DD models with the share of individual ownership in 2002, $IndOwn_{i,2002}$, as a treatment variable instead of a treatment dummy, $TREAT_i$.

[10] We use a relatively longer sample period of 1999–2007 rather than the period of 2002–2004 used in Lin and Flannery (2013) because it may take longer for a firm to adjust the level or efficiency of investment than the leverage ratio. Our sample period begins in 1999 because of a concern of confounding effects arising from the 1997 Tax Relief Act Reform that lowered the top marginal long-term capital gains tax rate from 28% to 20%.

3.3. Construction of variables

3.3.1. Measuring investment and investment efficiency

Following Biddle et al. (2009), we measure investment inefficiency using the deviation of a firm's actual investment from its optimal investment, which is a function of growth opportunities proxied by a firm's sales growth. The rationale behind the model is that in perfect markets without frictions, a firm's optimal investment should be positively related to its growth opportunities.[11] To define investment efficiency, we first estimate the following regression model for each industry-year:

$$INV_{i,t} = a + bSG_{i,t-1} + \epsilon_{i,t}, \tag{2}$$

where $INV_{i,t}$ is the sum of capital expenditures, acquisitions, research and development (R&D) expenses less the sale of property, plant and equipment (PPE) divided by lagged book total assets, $SG_{i,t-1}$ is firm i's sales growth rate in year $t-1$, and the regressions are run for each industry-year for all industries (based on Fama and French's 48-industry classification) with at least 20 observations in a given year.

Firm-year observations with positive residuals are classified as overinvesting firms, and firm-year observations with negative residuals are classified as underinvesting firms. The optimal or expected level of investment is computed as the predicted values from the regressions of firm-level investment on lagged firm-level sales growth. Thus, our investment efficiency measure, $INVEFF_{i,t}$, is defined as -1 times the absolute value of residual ($|\widehat{\epsilon}_{i,t}|$) where the residual is obtained from the investment regression model stated in Eq. (2). We multiply the absolute value of the residual by -1 so that a higher value indicates a higher degree of investment efficiency; that is, a higher level of deviation from the optimal level means a lower investment efficiency, and vice versa.

3.3.2. Measuring control variables

We include a set of variables to control for possible factors that influence investment or investment efficiency, as in Biddle et al. (2009): firm size, Tobin's q, cash flow volatility, sales volatility, investment volatility, Altman's (1968) Z score, asset tangibility, market leverage, industry-average market leverage, cash flows from operations as a proportion of sales, financial slack (defined as the ratio of cash to PPE), a dividend payer indicator, firm age, operating cycle, a financial loss indicator, and the cash ratio (defined as the sum of cash and short-term investments to total assets). We also include analyst coverage and the inverse of Gompers et al.'s (2003) G index in an investment model, while we do not include them in investment efficiency models.[12] The detailed definitions of all the control variables are provided in Appendix A. In implementing the DD analyses, we employ fixed effects regressions, allowing us to address the unobserved heterogeneity across firms. We also include industry-year dummies.

3.4. Descriptive statistics

Panel A of Table 2 reports summary statistics of the main regression variables used in this study. Our sample has 18,279 firm-year observations from 1999 to 2007, with firm-year observations from 2003 excluded. Thus, we include firm-year observations from a four-year period before the 2003 tax reform and a four-year period after the 2003 tax reform. The mean investment efficiency is -11%, suggesting that the mean deviation from optimal investment across all firm-years equals 11% of the prior year's total assets. The mean investment efficiency for overinvesting firms and underinvesting firms is -15% and -8%, respectively, which suggests that the magnitude of overinvestment problems is greater than the magnitude of underinvestment problems for U.S. public firms in the sample period. The proportion of overinvesting firm-years is approximately 34%. Overall, our key variables display moderate variations, consistent with the literature (Lin and Flannery, 2013; Yagan, 2015).

Panel B of Table 2 reports summary statistics of individual and institutional ownership measured prior to the passage of the 2003 tax cut (i.e., in 2002). The mean (median) of individual ownership in 2002 was approximately 62.6% (65.0%), while the mean (median) of institutional ownership in 2002 was approximately 37.4% (35.0%). The institutional ownership in 2002 ranged from 0.0% to 98.8%. Notably, the 10th percentile is zero. Assuming that firms not covered by the 13F database are fully owned by individual investors, we replaced missing values with zero. As this might be a strong assumption, we examine whether our results are robust to dropping firms that lack 13F data. We find that the results do not change much.

[11] When we use Tobin's q as a measure of growth opportunities, we obtain very similar results.

[12] We do not include those measures in investment efficiency models because we consider the reduction in agency problems as a main economic mechanism.

Table 2
Summary statistics.

Panel A. Main regression variables in the full sample and the subsamples of overinvesting and underinvesting firms

Variable	Full sample ($N = 18,279$)					Overinvesting firms: A ($N = 6162$)		Underinvesting firms: B ($N = 12,117$)		Test of difference in means: $A - B$
	Mean	S.D.	Q1	Median	Q3	Mean	S.D.	Mean	S.D.	Difference
$INVEFF_{i,t}$	−0.105	0.148	−0.125	−0.070	−0.035	−0.150	0.232	−0.083	0.063	−0.067***
$INV_{i,t}$	0.157	0.195	0.051	0.103	0.192	0.311	0.266	0.079	0.060	0.232***
$LOGASSET_{i,t-1}$	5.797	1.932	4.411	5.677	7.057	5.557	1.849	5.919	1.962	−0.362***
$Q_{i,t-1}$	2.221	2.082	1.145	1.581	2.486	2.791	2.639	1.930	1.657	0.861***
$SD\_CFO_{i,t-1}$	0.083	0.089	0.032	0.056	0.099	0.096	0.102	0.076	0.081	0.019***
$SD\_SALES_{i,t-1}$	0.209	0.209	0.077	0.145	0.263	0.204	0.193	0.211	0.216	−0.007**
$SD\_INV_{i,t-1}$	0.087	0.090	0.029	0.057	0.111	0.097	0.095	0.082	0.087	0.015***
$Z_{i,t-1}$	1.098	3.078	0.640	1.662	2.581	0.613	3.851	1.344	2.564	−0.731***
$TANG_{i,t-1}$	0.269	0.227	0.089	0.193	0.389	0.285	0.241	0.260	0.220	0.025***
$LEV_{i,t-1}$	0.156	0.195	0.000	0.074	0.253	0.109	0.154	0.180	0.209	−0.071***
$IND\_LEV_{i,t-1}$	0.160	0.103	0.068	0.174	0.186	0.151	0.097	0.164	0.105	−0.013***
$CFO\_SALES_{i,t-1}$	−0.105	1.604	0.019	0.079	0.148	−0.258	2.194	−0.027	1.189	−0.231***
$SLACK_{i,t-1}$	3.415	9.169	0.090	0.479	2.644	3.838	9.017	3.200	9.238	0.638***
$DIVIDEND_{i,t-1}$	0.355	0.479	0.000	0.000	1.000	0.306	0.461	0.381	0.486	−0.075***
$AGE_{i,t-1}$	16.360	15.790	6.000	11.000	23.000	13.980	13.790	17.570	16.590	−3.597***
$OPERATINGCYCLE_{i,t-1}$	4.635	0.750	4.244	4.707	5.115	4.576	0.772	4.665	0.737	−0.089***
$LOSS_{i,t-1}$	0.282	0.450	0.000	0.000	1.000	0.293	0.455	0.276	0.447	0.017**
$CASH_{i,t-1}$	0.196	0.217	0.026	0.106	0.305	0.243	0.237	0.173	0.202	0.070***
$ANALYSTS_{i,t-1}$	7.913	8.972	1.000	5.000	11.000	8.372	8.860	7.680	9.020	0.691***
$INVG_{i,t-1}$	−9.124	2.647	−11.000	−9.000	−7.000	−9.024	2.627	−9.170	2.655	0.146**

Panel B. Individual and institutional ownership levels prior to the passage of the 2003 tax cut

Variable	Mean	S.D.	Min	P5	P10	Q1	Median	Q3	P90	P95	Max	Obs.
$IndOwn_{i,2002}$	0.626	0.317	0.012	0.121	0.185	0.339	0.650	0.973	1.000	1.000	1.000	3734
$InsOwn_{i,2002}$	0.374	0.317	0.000	0.000	0.000	0.027	0.350	0.661	0.815	0.879	0.988	3734

Panel A reports summary statistics of the main regression variables constructed using a sample of U.S. public firms. The sample consists of nonfinancial firms for a 9-year period surrounding the 2003 capital taxation reform, i.e., the period from 1999 to 2007. We exclude firm-year observations in 2003 since the 2003 tax reform was enacted on May 28, 2003. The sample is divided into two groups: overinvesting firms and underinvesting firms, as defined in Section 3.3.1. The full sample, the subsample of overinvesting firms, and the subsample of underinvesting firms contain 18,279, 6162, and 12,117 firm-year observations, respectively. However, $INVG_{i,t-1}$ is only available for 7470, 2342, and 5128 firm-year observations in the full sample, the subsample of overinvesting firms, and the subsample of underinvesting firms, respectively. In the last column, the results of the t-tests of the differences in means between the two subgroups are reported. The superscripts ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. Panel B reports summary statistics of individual ($IndOwn_{i,2002}$) and institutional ownership ($InsOwn_{i,2002}$) measured prior to the passage of the 2003 tax cut (i.e., in 2002).

4. Effects of the 2003 personal taxation reform on corporate investment and investment efficiency

4.1. Baseline DD results

4.1.1. DD analysis of the effect of the tax reform on corporate investment

Before we investigate the effect of the 2003 tax reform on investment efficiency, using the DD framework, we revisit the question of whether the tax reform influenced the level of corporate investment. Dhaliwal et al. (2007a) report that the 2003 tax reform reduced firms' cost of equity, and Chetty and Saez (2005) report that the reform caused firms to pay out more in equity income. These results seem consistent with "traditional view" neoclassical models (e.g., Harberger, 1962; Feldstein, 1970; Poterba and Summers, 1984), suggesting that by reducing firms' cost of capital, permanent dividend tax cuts induce firms to raise new investment funds and increase investment. However, Yagan (2015) reports that the tax cut caused a zero change in corporate investment and a small change in the cost of capital and argues that the dividend tax cut did not have any significant near-term effects on U.S. private firms. Given that our sample and empirical framework are different from those in Yagan's (2015) DD analysis, we revisit the question of whether the dividend tax cut increased the level of corporate investment.

The first two columns in Panel A of Table 3 report the results of the DD analyses in which the dependent variable is the level of corporate investment. The model is the same as Eq. (1) except for the dependent variable. Column (1) of Panel A, Table 3 shows that $TREAT_i \times POST_t$ has negative and insignificant coefficients, suggesting that taking into account other factors and policy changes surrounding the tax reform, the 2003 tax reform did not significantly increase or decrease corporate investment. Column (2) shows that the results are unchanged when we include two variables measuring corporate governance, i.e., analyst coverage and the inverse of Gompers et al.'s (2003) G index. Panel B of Table 3 also shows that the 2003 tax reform

Table 3
Effect of the tax reform on investment and investment efficiency.

Panel A. Using a variable indicating high individual ownership prior to the passage of the 2003 tax cut as a treatment status variable

Model	Full sample			Overinvesting firms		Underinvesting firms	
	$INV_{i,t}$	$INV_{i,t}$	$INVEFF_{i,t}$	$INV_{i,t}$	$INVEFF_{i,t}$	$INV_{i,t}$	$INVEFF_{i,t}$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$TREAT_i \times POST_t$	−0.004	−0.003	0.018***	−0.045***	0.045***	0.009***	0.006***
	(0.007)	(0.011)	(0.005)	(0.017)	(0.017)	(0.002)	(0.002)
$LOGASSET_{i,t-1}$	−0.083***	−0.084***	0.044***	−0.111***	0.114***	−0.011***	−0.017***
	(0.007)	(0.010)	(0.005)	(0.014)	(0.013)	(0.002)	(0.002)
$Q_{i,t-1}$	0.015***	0.010***	−0.008***	0.017***	−0.014***	0.004***	0.001*
	(0.002)	(0.003)	(0.002)	(0.003)	(0.003)	(0.001)	(0.001)
$SD\_CFO_{i,t-1}$	0.057	0.192**	−0.039	0.012	0.010	−0.011	−0.006
	(0.036)	(0.079)	(0.029)	(0.082)	(0.080)	(0.011)	(0.012)
$SD\_SALES_{i,t-1}$	−0.008	0.002	0.000	−0.032	0.022	−0.001	0.003
	(0.012)	(0.018)	(0.009)	(0.030)	(0.029)	(0.004)	(0.004)
$SD\_INV_{i,t-1}$	−0.224***	−0.196***	0.187***	−0.282***	0.291***	0.001	−0.006
	(0.039)	(0.056)	(0.033)	(0.079)	(0.078)	(0.009)	(0.009)
$Z_{i,t-1}$	−0.000	0.016***	0.001	0.001	−0.001	0.001*	0.001
	(0.002)	(0.002)	(0.002)	(0.004)	(0.003)	(0.001)	(0.001)
$TANG_{i,t-1}$	0.085**	0.144***	−0.047*	0.062	−0.085	0.003	0.019*
	(0.034)	(0.042)	(0.027)	(0.076)	(0.074)	(0.010)	(0.011)
$LEV_{i,t-1}$	−0.205***	−0.182***	0.077***	−0.214***	0.208***	−0.041***	−0.033***
	(0.020)	(0.026)	(0.016)	(0.052)	(0.051)	(0.005)	(0.005)
$IND\_LEV_{i,t-1}$	−0.027	−0.019	0.038*	−0.033	0.030	0.001	0.007
	(0.028)	(0.039)	(0.023)	(0.076)	(0.075)	(0.009)	(0.010)
$CFO\_SALES_{i,t-1}$	−0.004	0.006	0.003*	−0.001	0.003	0.000	−0.001
	(0.002)	(0.012)	(0.002)	(0.003)	(0.003)	(0.001)	(0.001)
$SLACK_{i,t-1}$	−0.001	−0.000	−0.000	0.000	−0.000	−0.000***	−0.000*
	(0.000)	(0.001)	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)
$DIVIDEND_{i,t-1}$	0.006	0.003	−0.000	−0.000	0.005	0.003	0.001
	(0.007)	(0.008)	(0.005)	(0.019)	(0.018)	(0.002)	(0.002)
$AGE_{i,t-1}$	−0.050*	0.041	0.018	−0.097**	0.096***	−0.019***	−0.011*
	(0.026)	(0.029)	(0.020)	(0.038)	(0.035)	(0.006)	(0.006)
$OPERATINGCYCLE_{i,t-1}$	−0.005	0.006	0.001	0.012	−0.015	−0.000	0.002
	(0.007)	(0.009)	(0.006)	(0.014)	(0.013)	(0.002)	(0.002)
$LOSS_{i,t-1}$	−0.027***	−0.013**	0.005	−0.021*	0.016	−0.010***	−0.006***
	(0.004)	(0.006)	(0.004)	(0.011)	(0.011)	(0.001)	(0.001)
$CASH_{i,t-1}$	0.139***	0.181***	−0.059***	0.145***	−0.149***	0.011*	0.024***
	(0.024)	(0.044)	(0.019)	(0.054)	(0.051)	(0.007)	(0.007)
$ANALYSTS_{i,t-1}$		0.001					
		(0.001)					
$INVG_{i,t-1}$		0.002					
		(0.002)					
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	18,279	7470	18,279	6162	6162	12,117	12,117
R-squared	0.177	0.196	0.120	0.259	0.193	0.210	0.337
Number of firms	3051	1433	3051	2248	2248	2786	2786

Panel B. Using the individual ownership level prior to the passage of the 2003 tax cut as a treatment status variable

Model	Full sample			Overinvesting firms		Underinvesting firms	
	$INV_{i,t}$	$INV_{i,t}$	$INVEFF_{i,t}$	$INV_{i,t}$	$INVEFF_{i,t}$	$INV_{i,t}$	$INVEFF_{i,t}$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$IndOwn_{i,2002} \times POST_t$	−0.003	−0.004	0.024***	−0.063**	0.062**	0.013***	0.009**
	(0.011)	(0.025)	(0.009)	(0.028)	(0.027)	(0.003)	(0.004)
Control variables in Panel A	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Governance controls	No	Yes	No	No	No	No	No
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	18,279	7470	18,279	6162	6162	12,117	12,117

(*continued on next page*)

Table 3 (*continued*)

Panel B. Using the individual ownership level prior to the passage of the 2003 tax cut as a treatment status variable

Model	Full sample			Overinvesting firms		Underinvesting firms	
	$INV_{i,t}$ (1)	$INV_{i,t}$ (2)	$INVEFF_{i,t}$ (3)	$INV_{i,t}$ (4)	$INVEFF_{i,t}$ (5)	$INV_{i,t}$ (6)	$INVEFF_{i,t}$ (7)
R-squared	0.177	0.196	0.120	0.258	0.192	0.210	0.337
Number of firms	3051	1433	3051	2248	2248	2786	2786

This table reports the results of the difference-in-differences analyses designed to identify the effect of the 2003 tax cut on investment and investment efficiency using a sample of U.S. public firms. The sample consists of nonfinancial firms for a 9-year period surrounding the 2003 capital taxation reform, from 1999 to 2007. We exclude firm-year observations in 2003 since the 2003 tax reform was enacted on May 28, 2003. The first three columns are based on the full sample, the middle two columns are the subsample of overinvesting firms, and the last two columns are the subsample of underinvesting firms. In Panel A, we use a dummy variable indicating high individual ownership prior to the passage of the 2003 tax cut, namely, $TREAT_i$, as a treatment status variable. In Panel B, we use the level of individual ownership prior to the passage of the 2003 tax cut, namely, $IndOwn_{i,2002}$, as a treatment status variable. In Columns (1), (2), (4), and (6) (Columns (3), (5), and (7)), the dependent variables are the level of investment (the magnitude of investment efficiency), as defined in Section 3.3.1. The definition of control variables are provided in Section 3.3.2. The detailed definitions of all variables are provided in Appendix A. The superscripts ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

did not significantly increase or decrease corporate investment when the continuous treatment variable ($IndOwn_{i,2002}$) was used. These results are consistent with Yagan's (2015) main findings based on data for U.S. private firms.

Then, we investigate whether the 2003 tax reform had heterogeneous effects on the investment of overinvesting and underinvesting firms. Columns (4) and (6) report the subsample results. Note that the tax cut lowered the investment of overinvesting firms by 4.5 percentage points but increased the investment of underinvesting firms by 0.9 percentage points. These effects are statistically significant at the 1% level. The treatment effect amounts to approximately 14.5% (= |−0.045|/0.311) of mean investment (0.311) for overinvesting firms and approximately 11.4% (= 0.009/0.079) of mean investment (0.079) for underinvesting firms. Therefore, the effect that the 2003 tax reform has on corporate investment is economically meaningful.[13] These results are similar to those of Alstadsæter et al.'s (2017) study, which documents heterogeneous responses of cash-rich and cash-constrained firms to a large-scale dividend tax cut in Sweden.

The first set of results for the full sample seem consistent with "neutrality view" neoclassical model predictions that dividend taxes have no impact on corporate investment. However, the subsample results reject Hypothesis 1 based on the "neutrality view" neoclassical models. The results seem rather consistent with "agency conflict" models. Thus, in the following subsections, we further investigate whether and through what mechanisms the investment efficiency of over- and underinvesting firms was differently impacted by the 2003 personal taxation reform.

4.1.2. DD analysis of the effect of the tax reform on corporate investment efficiency

We then examine whether the tax reform influenced the efficiency of corporate investment decisions. Although the dividend tax cut did not increase corporate investment significantly, it is possible that the dividend tax cut reduced underinvestment problems and overinvestment problems. If both underinvestment and overinvestment problems are reduced by the dividend tax cut, it is possible that the level of investment is not significantly influenced by the dividend tax cut. The preliminary analyses reported in the previous subsection provide empirical evidence supporting this possibility. We estimate the DD models described in Section 3.1.2 using the full sample as well as the subsamples of overinvesting and underinvesting firms.

Columns (3), (5), and (7) in Panel A, Table 3 report the results of the DD analyses with investment efficiency as the dependent variable. The results show that in all three models, the coefficients on the interaction term, $TREAT_i \times POST_t$, are positive and significant at the 1% level, suggesting that the tax reform increased investment efficiency for firms in the full sample as well as the subsamples of over- and underinvesting firms.[14] In the full sample, the tax reform increased investment efficiency by 1.8% of total assets, with a statistical significance at the 1% level. The increase in investment efficiency amounts to approximately 12.2% (= 0.018/0.148) of one standard deviation (0.148). Therefore, the positive effect of the tax reform on overall investment efficiency seems to be economically meaningful.

The subsample results show that the tax reform enhanced investment efficiency for both overinvesting and underinvesting firms, but the degree of enhancement was notably different between the two groups. Tax reform increased investment efficiency by 4.5% of total assets in the subsample of overinvesting firms and by only 0.6% of total assets in the subsample of underinvesting firms. The increase in investment efficiency amounts to approximately 19.4% (= 0.045/0.232) of one standard deviation (0.232) for overinvesting firms and approximately 9.5% (= 0.006/0.063) of one standard deviation

[13] Similarly, Panel B of Table 3 based on the continuous treatment variable shows that the tax cut lowered the investment of overinvesting firms but increased the investment of underinvesting firms.

[14] Panel B of Table 3 also shows that the effects based on the continuous treatment variable are very similar to those based on the baseline treatment indicator.

(0.063) for underinvesting firms. These results suggest that the effects of the 2003 tax reform on the investment efficiency of both over- and underinvesting firms are economically significant.

Overall, our DD results suggest that the 2003 tax cut led to a significant reduction in the tendency to overinvest and underinvest, although it did not cause a significant increase in corporate investment. Thus, Hypothesis 2A and Hypothesis 3A are supported by our results.

Our estimated coefficient of 0.018 in the regression of investment efficiency is comparable with the coefficients reported in the literature.[15] For example, a recent study by Bae et al. (2017, Table 2, p. 29) examining whether auditors' knowledge and resources affect their clients' investment efficiency report a coefficient of 0.017 on the dummy variable representing Big N auditors for their full sample.[16] However, our estimated coefficient of 0.045 for overinvesting firms is much larger than the 0.025 on the Big N dummy variable for the overinvesting firm sample in Bae et al.'s (2017) study. Thus, we surmise that the tax reform was particularly effective in improving the investment efficiency of overinvesting firms in our sample. On the other hand, in the case of underinvesting firms, our estimate of 0.006 appears to be somewhat smaller than the estimate of 0.012 for underinvesting firms, as reported in Bae et al.'s (2017) study. Thus, the tax reform was much less powerful in directing underinvesting firms to ramp up their investment efficiency.

Note that the degree of enhancement in investment efficiency is significantly different between overinvesting and underinvesting firms in terms of both magnitude and statistical significance. The results for overinvesting firms are consistent with the "agency conflict" model, and the results for underinvesting firms are consistent with the "traditional view" neoclassical models. However, theories do not provide any clear predictions on which type of firms, between overinvesting and underinvesting firms, will be severely influenced by a large cut in dividend tax rates. Chetty and Saez (2010) provide some clues on this question. On page 16 of their paper, they predict that a dividend tax cut lowers total investment (i.e., productive investment plus pet investment) for cash-rich firms (i.e., financially unconstrained firms) with an agency problem, and this prediction contrasts with the "traditional" view model, where a tax cut raises investment, and with the "neutrality" view model, where a tax cut has no effect on investment. On the other hand, they state that the effect of a tax cut for cash-poor firms (i.e., financially constrained firms) is ambiguous in the agency model because a tax cut increases equity issues and productive (as well as unproductive) investment by such firms.[17] Thus, Chetty and Saez's (2010) model predicts that the magnitude of the effect of a dividend tax cut on investment efficiency is greater for overinvesting firms than for underinvesting firms.

4.2. DD results using alternative treatment status indicators

4.2.1. DD results based on two refined treatment status indicators

Although numerous authors have used the share of individual ownership based on 13F filings to determine treatment status, it is subject to controversy (e.g., Chetty and Saez, 2005; Hanlon and Heitzman, 2010; Blouin et al., 2017). In particular, this ratio may contain significant measurement errors in identifying tax-insensitive and tax-sensitive institutional investors. Among the 13F institutional investors who are assumed to be tax-insensitive, investment advisers whose main clientele consists of high net-worth individuals would be incorrectly classified as tax-insensitive. In reality, these may be the most tax-sensitive institutional investors. Based on a stringent classification, Chetty and Saez (2005) find that only 15 percent of institutional owners in the 13F database are fully nontaxable. On the other hand, many of the shares owned by individual investors are held in tax-free individual retirement accounts. Misclassification errors may be distributed heterogeneously because certain securities may be more advantageously held in tax-free accounts. For example, an individual investor may prefer to hold shares of dividend paying stocks in their individual retirement account and shares of a firm that repurchases shares in their taxable accounts.

To mitigate concerns regarding errors in measuring tax-sensitive ownership, we employ alternative treatment status indicators and examine the robustness of our baseline results. We try refinements in defining the treatment status of our sample firms by using two alternative indicators. The first is a treatment status indicator based on the association between tax-sensitive individual ownership and stock turnover motivated by Barber and Odean (2003). The second is a treatment status indicator based on a refined classification according to a two-step approach taken by Blouin et al. (2017), of tax-sensitive versus tax-insensitive institutional investors. Based on each of these refined indicators, we first select a more tax-sensitive subset from our original treatment group. Then, we compare it with a more tax-insensitive subset selected from our original control group based on the same indicator. The motivation for these refinements is the possibility that measurement errors can contaminate both our tax-sensitive treatment group and tax-insensitive control group. Let us assume that both our treatment and control groups suffer from significant measurement errors and that our baseline results are not

[15] As explained above, this number indicates that the tax reform improved investment efficiency by 1.8% of total assets.

[16] The reported coefficients in Bae et al.'s (2017) study have negative signs because Bae et al. (2017) use the absolute value of abnormal investment as their measure of investment inefficiency, which has the opposite sign of our measure of investment efficiency.

[17] In addition, Chetty and Saez (2010) show that the introduction of dividend taxes has a second-order (i.e., small) efficiency cost for cash-poor firms, while it has a first-order efficiency cost for cash-rich firms. Although the efficiency of taxation measured by excess burden is not the same concept as the efficiency of corporate investment, the two efficiency measures are likely to be positively correlated.

reliable. Then, we would obtain quite different results when we refine each of our treatment and control groups by selecting and comparing a more tax-sensitive treatment subset with a more tax-insensitive control subset based on alternative proxies. If our original proxy for tax-sensitive investors is a reasonable measure and our refined indicator variables are better or equally viable proxies for the tax sensitivity of the marginal investors, then we would expect to obtain stronger or similar results.

The detailed procedures to construct these two refined indicators are provided below. The first refined treatment status indicator is constructed based on both individual ownership and the stock turnover ratio. Barber and Odean (2003) find that high stock turnover is associated with trading in tax-sensitive individual accounts. Thus, firms that have both high individual ownership and high stock turnover are more likely to be held in tax-sensitive individual accounts, while firms that have both low individual ownership and low stock turnover are more likely to be held in tax-insensitive individual accounts. We partition our original treated and control firms into quintiles, respectively, based on stock turnover. Then, we select the most tax-sensitive quintile from the treatment sample and compare it with the most tax-insensitive quintile in the control sample. Specifically, our first refined treatment indicator $TREAT_i^{TO}$ is defined as follows: $TREAT_i^{TO}$ equals one if $TREAT_i = 1$ and firm i's stock turnover belongs to the top quintile in 2002 and equals zero if $TREAT_i = 0$ and firm i's stock turnover belongs to the bottom quintile in 2002.

The second refined treatment status indicator is constructed based on both individual ownership and the proportion of tax-sensitive institutional ownership. This indicator is based on Blouin et al. (2017) who propose a two-step procedure to classify institutional investors as tax-sensitive and tax-insensitive.[18] In the first step, the authors create a calibration sample of institutional investors based on detailed information about their investor clientele from the Form ADV filings collected in 2006. They classify investment advisers whose majority clientele consists of high net-worth individuals as tax-sensitive. Since this calibration sample is very limited, covering only 463 investment advisers, they estimate a logistic regression to generate a model that can be used to predict tax-sensitivity of other institutional investors (i.e., a classification sample) who file Form 13F but lack investor clientele data. Tax-motivated trading variables and other portfolio characteristics of investment advisers are found to be useful variables to predict tax-sensitivity. In the second step, the estimated coefficients from the logistic regression of the calibration sample are applied to the portfolio characteristics of a large classification sample. Each institutional investor is classified as tax-sensitive based on whether its predicted probability of being tax-sensitive is above the base-rate percentage of tax-sensitive institutions in the calibration sample in a given year.[19] To construct our second refined treatment status indicator, we merge Blouin et al.'s (2017) classification data into the 13F database and calculate firm i's tax-sensitive ($InsOwn_{i,2002}^{TSI}$) and tax-insensitive institutional ownership in 2002 ($InsOwn_{i,2002}^{TII}$).[20] Based on the proportion of tax-sensitive institutional ownership, we divide our original treated and control firms into two subgroups, respectively. We then select a more tax-sensitive subgroup from the treatment sample and compare it with a more tax-insensitive subgroup selected from the control sample. Specifically, our second refined treatment indicator $TREAT_i^{TS}$ is defined as follows: $TREAT_i^{TS}$ equals one if $TREAT_i = 1$ and firm i's tax-sensitive institutional ownership proportion is above the sample median in 2002 and equals zero if $TREAT_i = 0$ and firm i's tax-sensitive institutional ownership proportion is below the sample median in 2002.

Panels A and B of Table 4 report the estimation results based on the two refined treatment status indicators. The results with $TREAT_i^{TO}$ are presented in Panel A of Table 4, while the results with $TREAT_i^{TS}$ are shown in Panel B of Table 4. The following discussions are mainly based on Panel A because the results in Panels A and B are similar.

First, Column (1) of Panel A shows that $TREAT_i^{TO} \times POST_t$ has a statistically insignificant relationship with the level of investment in the full sample, consistent with our baseline results. Second, Column (2) of Panel A also shows that the coefficient remains insignificant when we include two variables measuring corporate governance, i.e., analyst coverage and the inverse of Gompers et al.'s (2003) G index. Once again, our full-sample results based on the refined classification of treated and control firms are highly consistent with Yagan's (2015) main findings based on data for U.S. private firms. Third, Column (3) in Panel A shows that the coefficient on the interaction term, $TREAT_i^{TO} \times POST_t$, is positive and significant at the 1% level, suggesting that the tax reform increased investment efficiency for average firms in the full sample. Note that the coefficient becomes much larger (i.e., nearly doubles). The coefficient of 0.033 indicates that the tax reform increased investment efficiency by 3.3% of total assets for average firms in the full sample. The increase in investment efficiency amounts to approximately 22.3% (= 0.033/0.148) of one standard deviation (0.148), suggesting that the positive effect of the tax reform on overall investment efficiency in the full sample is economically significant.

Second, Columns (4) and (6) of Panel A indicate that the tax cut lowered overinvesting firms' investment by 7.7% of total assets but increased underinvesting firms' investment by 1.6% of total assets. Both of the effects are statistically significant at the 1% level. The treatment effect amounts to approximately 28.9% (= |−0.077|/0.266) of one standard deviation of investment (0.266) for overinvesting firms and approximately 26.7% (= 0.016/0.060) of one standard deviation of investment (0.060) for underinvesting firms. Therefore, the effects of the 2003 tax reform on corporate investment are economically meaningful. Note also that the effect of the tax reform on the level of investment is much stronger for overinvesting firms.

[18] We obtained the extended tax-sensitivity classification data from Professor Brian Bushee's personal website: https://accounting.wharton.upenn.edu/profile/bushee/#research.

[19] See Blouin et al. (2017) for more details.

[20] The mean (median) of tax-sensitive institutional ownership in 2002 was 5.9% (3.9%), while the mean (median) of tax-insensitive institutional ownership in 2002 was 40.6% (41.6%). Notably, the mean (median) proportion of tax-sensitive institutional ownership in 2002 was 13.9% (8.6%).

Table 4
Alternative approaches to determine treatment status.

Panel A. Using individual ownership and stock turnover to define a treated firm indicator

Model	Full sample			Overinvesting firms		Underinvesting firms	
	$INV_{i,t}$ (1)	$INV_{i,t}$ (2)	$INVEFF_{i,t}$ (3)	$INV_{i,t}$ (4)	$INVEFF_{i,t}$ (5)	$INV_{i,t}$ (6)	$INVEFF_{i,t}$ (7)
$TREAT_i^{TO} \times POST_t$	−0.010	−0.010	0.033***	−0.077***	0.073***	0.016***	0.019***
	(0.013)	(0.013)	(0.010)	(0.027)	(0.027)	(0.004)	(0.004)
Control variables in Table 3	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Governance controls	No	Yes	No	No	No	No	No
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5896	5896	5896	1954	1954	3942	3942
R-squared	0.205	0.205	0.153	0.351	0.291	0.226	0.383
Number of firms	956	956	956	706	706	872	872

Panel B. Using tax-sensitive and tax-insensitive institutional ownership to define a treated firm indicator

Model	Full sample			Overinvesting firms		Underinvesting firms	
	$INV_{i,t}$ (1)	$INV_{i,t}$ (2)	$INVEFF_{i,t}$ (3)	$INV_{i,t}$ (4)	$INVEFF_{i,t}$ (5)	$INV_{i,t}$ (6)	$INVEFF_{i,t}$ (7)
$TREAT_i^{TS} \times POST_t$	−0.013	−0.013	0.024***	−0.042*	0.042*	0.007***	0.007**
	(0.009)	(0.009)	(0.007)	(0.023)	(0.022)	(0.003)	(0.003)
Control variables in Table 3	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Governance controls	No	Yes	No	No	No	No	No
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	9795	9795	9795	3352	3352	6443	6443
R-squared	0.207	0.207	0.141	0.325	0.257	0.228	0.371
Number of firms	1686	1686	1686	1224	1224	1526	1526

Panel C. Using firms' cumulative abnormal returns to define a treated firm indicator

Model	Full sample			Overinvesting firms		Underinvesting firms	
	$INV_{i,t}$ (1)	$INV_{i,t}$ (2)	$INVEFF_{i,t}$ (3)	$INV_{i,t}$ (4)	$INVEFF_{i,t}$ (5)	$INV_{i,t}$ (6)	$INVEFF_{i,t}$ (7)
$HighCAR_i \times POST_t$	−0.021	0.012	0.033***	−0.099**	0.100**	0.006	0.007
	(0.016)	(0.022)	(0.013)	(0.041)	(0.041)	(0.005)	(0.005)
Control variables in Table 3	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Governance controls	No	Yes	No	No	No	No	No
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	18,098	7458	18,098	6102	6102	11,996	11,996
R-squared	0.178	0.196	0.120	0.261	0.196	0.208	0.337
Number of firms	2972	1428	2972	2211	2211	2721	2721

This table presents the results of the two alternative difference-in-differences analyses designed to identify the effect of the 2003 tax cut on investment and investment efficiency. The sample consists of nonfinancial U.S. firms for a 9-year period surrounding the 2003 capital taxation reform, from 1999 to 2007. We exclude firm-year observations in 2003 since the 2003 tax reform was enacted on May 28, 2003. In Panel A, we use a refined treatment status indicator based on both individual ownership and stock turnover, $TREAT_i^{TO}$, which equals one if $TREAT_i = 1$ and firm i's stock turnover is above the fifth quintile in 2002 (among the treated firms based on the original proxy) and equals zero if $TREAT_i = 0$ and firm i's stock turnover is below the first quintile in 2002 (among the control firms based on the original proxy). In Panel B, we use a refined treatment status indicator based on both individual ownership and the proportion of tax-sensitive institutional ownership, $TREAT_i^{TS}$, which equals one if $TREAT_i = 1$ and firm i's tax-sensitive institutional ownership proportion is above the sample median in 2002 (among the treated firms based on the original proxy) and equals zero if $TREAT_i = 0$ and firm i's tax-sensitive institutional ownership proportion is below the sample median in 2002 (among the control firms based on the original proxy). In Panel C, we use a treatment status indicator based on firms' cumulative abnormal stock returns in two event windows when the market received positive signals about the passage of the 2003 tax cut, i.e., January 3–9, 2003, and May 14–28, 2003. We estimate the cumulative abnormal returns (CARs) for the two event windows following Lin and Flannery (2013) and denote the January CAR as $CAR_i^{January}$ and the May CAR as CAR_i^{May}. Based on the two CARs, we define an indicator variable $HighCAR_i$ for the treatment status of a firm: $HighCAR_i = 1$ if both $CAR_i^{January}$ and CAR_i^{May} are greater than the fifth quintile and $HighCAR_i = 0$ otherwise. In each panel, the first three columns are based on the full sample, the middle two columns the subsample of overinvesting firms, and the last two columns the subsample of underinvesting firms. In Columns (1), (2), (4), and (6) (Columns (3), (5), and (7)), the dependent variables are the level of investment (the magnitude of investment efficiency), as defined in Section 3.3.1. The definition of control variables are provided in Section 3.3.2. The detailed definitions of all variables are provided in Appendix A. The superscripts ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Third, Columns (5) and (7) of Panel A indicate that the 2003 tax reform increased investment efficiency by 7.3% of total assets for overinvesting firms and only 1.9% of total assets for underinvesting firms. Both of the coefficients are statistically significant at the 1% level. The treatment effect amounts to approximately 31.5% (= 0.073/0.232) of one standard deviation of investment efficiency (0.232) for overinvesting firms and approximately 30.2% (= 0.019/0.063) of one standard deviation of

investment efficiency (0.063) for underinvesting firms. Therefore, the effects of the 2003 tax reform on investment efficiency are also economically meaningful. Again, these results indicate that the effect of the 2003 tax reform on investment efficiency is stronger for overinvesting firms.

The full-sample results in Panel B are very similar to those in Panel A. In Columns (1) and (2), the coefficient of $TREAT_i^{TS} \times POST_t$ is negative and statistically insignificant at a conventional level. In Column (3), the coefficient of $TREAT_i^{TS} \times POST_t$ is 0.024 and statistically significant at the 1% level. The estimated coefficient is far greater than that in our baseline results. The subsample coefficients in Panel B are somewhat smaller than those in Panel A. However, the coefficients are comparable to those in our baseline results in terms of the magnitudes. Although the results are slightly weaker for overinvesting firms in terms of statistical significance, these results are similar to our baseline results.

Overall, our DD results based on the two refined treatment status indicators confirm that the 2003 tax cut led to a highly significant reduction in the tendency to overinvest (and a less significant reduction in the tendency to underinvest), although it did not result in a significant increase in corporate investment. The results presented in Panels A and B of Table 4, therefore, suggest that our baseline findings are not driven by the measurement error of individual ownership.

4.2.2. DD results based on a treatment status indicator using cumulative abnormal returns

In this subsection, we provide evidence on the impact of the 2003 tax reform on corporate investment and its efficiency by using a treatment status indicator based on firms' abnormal stock returns associated with news about the 2003 tax cut. This treatment status indicator does not utilize the 13F database, so this indicator is not subject to criticisms about the treatment status indicator solely based on the share of individual ownership.

The tax cut is expected to increase the value of future dividends to individual investors. Thus, the marginal investors of the firms that had positive abnormal returns during the periods when the tax cut became more likely to be implemented were likely to be individual investors. Following Amromin et al. (2008) and Lin and Flannery (2013), we examine firms' abnormal stock returns in two event windows when the market received positive signals about the passage of the 2003 tax cut, i.e., January 3−9, 2003, and May, 14−28, 2003. According to them, the newspaper coverage about the tax cut initially spiked on January 3−9, 2003, there was a major breakthrough in Senate negotiations on May 14, 2003, and the president signed the bill on May 28, 2003. We estimate the cumulative abnormal returns (CARs) for the two event windows and denote the January CAR as $CAR_i^{January}$ and the May CAR as CAR_i^{May}. Based on the two CARs, we define an indicator variable $HighCAR_i$ for the treatment status of a firm: $HighCAR_i = 1$ if both $CAR_i^{January}$ and CAR_i^{May} are greater than the fifth quintile and $HighCAR_i = 0$ otherwise.

The results with $HighCAR_i$ as a treatment status indicator are presented in Panel C of Table 4. Columns (1) through (3) in Panel C present the full sample results. First, Column (1) shows that $HighCAR_i \times POST_t$ has a statistically insignificant relationship with the level of investment in the full sample, consistent with our baseline results in Panel A, Table 3. Second, Column (2) shows that the coefficient is insignificant when we include two variables measuring corporate governance, i.e., analyst coverage and the inverse of Gompers et al.'s (2003) G index. Once again, our results based on the new classification of treated firms and control firms are consistent with Yagan's (2015) main findings based on data for U.S. private firms. Third, Column (3) shows that the coefficient on the interaction term $HighCAR_i \times POST_t$ is positive and significant at the 1% level, suggesting that the tax reform increased investment efficiency for average firms in the full sample. Note that the coefficient becomes much larger (i.e., nearly doubles). The tax reform increased investment efficiency by 3.3% of total assets for firms in the full sample. The increase in investment efficiency amounts to approximately 22.3% (= 0.033/0.148) of one standard deviation (0.148), suggesting that the positive effect of the tax reform on overall investment efficiency has a strong economic significance.

Columns (4) and (5) in Panel C present the estimation results for overinvesting firms, while Columns (6) and (7) in Panel C present the estimation results for underinvesting firms. First, Columns (4) and (6) show that the tax cut lowered overinvesting firms' investment by 9.9 percentage points but increased underinvesting firms' investment by 0.6 percentage points. The former effect is statistically significant at the 5% level, while the latter is not significant at a conventional level. The treatment effect amounts to approximately 31.8% (= |−0.099|/0.311) of the mean investment (0.311) for overinvesting firms and approximately 7.6% (= 0.006/0.079) of the mean investment (0.079) for underinvesting firms. Therefore, the effect of the 2003 tax reform on corporate investment is economically meaningful. Second, Columns (5) and (7) indicate that the tax reform increased investment efficiency by 10.0% of total assets for overinvesting firms and only 0.7% of total assets for underinvesting firms. The coefficient for overinvesting firms is significant at the 5% level, while the coefficient for underinvesting firms is not significant at a conventional level. The significance of the coefficients might be affected by sample selection, but it is clear that the effect of the tax reform on investment efficiency is much weaker for underinvesting firms.

Once again, our DD results based on CARs associated with news about the 2003 tax cut confirm that the 2003 tax cut led to a highly significant reduction in the tendency to overinvest (and a less significant reduction in the tendency to underinvest), although it did not cause a significant increase in corporate investment.

These results collectively suggest that our main findings are overall robust to the choice of the approach to identify treated firms, although the magnitudes of the key coefficients are not identical. The substantial variation in the magnitudes of the coefficients is quite natural since we can only use some plausible proxies because of the inability to observe the true treatment status. The analyses attempted in this section, however, imply that our simple proxy based on individual ownership prior to the passage of the 2003 tax reform does a reasonable job of identifying treated firms despite inherent difficulties and challenges associated with identifying tax-sensitive investors.

Table 5
Testing the validity of the parallel trends assumption.

Model	Full sample			Overinvesting firms		Underinvesting firms	
	$INV_{i,t}$ (1)	$INV_{i,t}$ (2)	$INVEFF_{i,t}$ (3)	$INV_{i,t}$ (4)	$INVEFF_{i,t}$ (5)	$INV_{i,t}$ (6)	$INVEFF_{i,t}$ (7)
$TREAT_i \times YR2000_t$	0.013	−0.023	−0.006	−0.013	0.011	0.002	0.001
	(0.013)	(0.032)	(0.011)	(0.033)	(0.033)	(0.003)	(0.004)
$TREAT_i \times YR2001_t$	0.008	−0.023	0.009	−0.034	0.035	0.006**	0.003
	(0.011)	(0.029)	(0.009)	(0.029)	(0.028)	(0.003)	(0.004)
$TREAT_i \times YR2002_t$	0.004	−0.013	0.006	−0.036	0.035	0.007**	0.002
	(0.011)	(0.028)	(0.009)	(0.029)	(0.029)	(0.003)	(0.004)
$TREAT_i \times POST_t$	0.003	−0.019	0.020**	−0.070**	0.069**	0.013***	0.008**
	(0.012)	(0.025)	(0.009)	(0.033)	(0.031)	(0.003)	(0.004)
Control variables in Table 3	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Governance controls	No	Yes	No	No	No	No	No
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	18,279	7470	18,279	6162	6162	12,117	12,117
R-squared	0.177	0.196	0.120	0.259	0.193	0.211	0.337
Number of firms	3051	1433	3051	2248	2248	2786	2786

This table reports the results of the difference-in-differences analyses designed to test the validity of the parallel trends assumption prior to the passage of the 2003 tax cut. We add three interaction terms, $TREAT_i \times YR2000_t$, $TREAT_i \times YR2001_t$, and $TREAT_i \times YR2002_t$, to examine whether there are parallel prereform trends between the treated and control groups, where $YR2000_t$, $YR2001_t$, and $YR2002_t$ are dummy variables indicating the fiscal years of 2000, 2001, and 2002, respectively. The sample consists of nonfinancial firms for a 9-year period surrounding the 2003 capital taxation reform, from 1999 to 2007. We exclude firm-year observations in 2003 since the 2003 tax reform was enacted on May 28, 2003. The first three columns are based on the full sample, the middle two columns are the subsample of overinvesting firms, and the last two columns are the subsample of underinvesting firms. In Columns (1), (2), (4), and (6) (Columns (3), (5), and (7)), the dependent variables are the level of investment (the magnitude of investment efficiency), as defined in Section 3.3.1. The definition of control variables are provided in Section 3.3.2. The detailed definitions of all variables are provided in Appendix A. The superscripts ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

4.3. Testing the validity of the parallel trends assumption

Since the validity of the DD estimate critically depends on the validity of the parallel trends assumption, we include three interaction terms, $TREAT_i \times YR2000_t$, $TREAT_i \times YR2001_t$, and $TREAT_i \times YR2002_t$, to examine whether there are parallel prereform trends for the treated and control groups, where $YR2000_t$, $YR2001_t$, and $YR2002_t$ are dummy variables indicating the fiscal years of 2000, 2001, and 2002, respectively. If the coefficients on them are statistically significant, one could argue that the parallel prereform trends assumption is violated.

Table 5 shows that all three interaction terms are not statistically significant in all models except in Column (6), while the coefficients on $TREAT_i \times POST_t$ are similar to those in Table 3.[21] For example, for full-sample firms, overinvesting firms, and underinvesting firms, the results in Columns (3), (5), and (7), respectively, indicate that the investment efficiency for the treated and control firms trends closely in parallel in the three years leading up to the 2003 tax reform. In all three samples, the differences in investment efficiency between the treatment and control groups begin to increase after the 2003 tax reform, indicating a positive effect of the tax cut on investment efficiency. Overall, these test results provide evidence that the parallel prereform trends assumption is not violated.

4.4. Falsification tests

A key identification assumption for the DD methodology is that identification terms are zero in the absence of the intervention (Angrist and Krueger, 1999). Following Lin and Flannery (2013), we repeat the tests in Table 3 using two sample periods in which there was no change in dividend tax rates. If similar results are found using the hypothetical event periods, we should cast doubt on the implications of the results in Section 4.1.

Table 6 reports the results of falsification tests based on two sample periods when there were no changes in dividend tax rates. In Panel A, the sample consists of nonfinancial firms for a 5-year period surrounding a hypothetical 1984 reform from 1982 to 1986. We define indicator variables $TREAT_i$ and $POST_t$ as follows: $TREAT_i$ equals one if firm i has individual ownership higher than the sample median in 1983 and zero otherwise; and $POST_t$ equals one if the year is after 1984 (i.e., 1985–1986) and zero if firm observations are before 1984 (i.e., 1982–1983). In Panel B, the sample consists of nonfinancial firms for a 9-year period surrounding a hypothetical 2008 reform, i.e., the period from 2004 to 2012. We define indicator variables $TREAT_i$ and $POST_t$ as follows: $TREAT_i$ equals one if firm i has individual ownership higher than the sample median in 2007 and equals zero

[21] The results in Column (6) indicate that the parallel trends assumption is weakly violated for the level of investment for underinvesting firms. However, based on the results in Column (7), we can draw a meaningful inference for the investment efficiency of underinvesting firms.

Table 6
Falsification tests.

Panel A. Using the sample period 1982–1986

Model	Full sample		Overinvesting firms		Underinvesting firms	
	$INV_{i,t}$ (1)	$INVEFF_{i,t}$ (2)	$INV_{i,t}$ (3)	$INVEFF_{i,t}$ (4)	$INV_{i,t}$ (5)	$INVEFF_{i,t}$ (6)
$TREAT_i \times POST_t$	0.004	−0.003	0.013	−0.015	0.001	0.002
	(0.006)	(0.005)	(0.014)	(0.014)	(0.003)	(0.003)
Control variables in Table 3	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6266	6266	2255	2255	4011	4011
R-squared	0.157	0.110	0.308	0.248	0.221	0.291
Number of firms	1836	1836	1176	1176	1603	1603

Panel B. Using the sample period 2004–2012

Model	Full sample		Overinvesting firms		Underinvesting firms	
	$INV_{i,t}$ (1)	$INVEFF_{i,t}$ (2)	$INV_{i,t}$ (3)	$INVEFF_{i,t}$ (4)	$INV_{i,t}$ (5)	$INVEFF_{i,t}$ (6)
$TREAT_i \times POST_t$	0.004	−0.002	0.005	−0.004	0.000	0.001
	(0.006)	(0.005)	(0.014)	(0.013)	(0.002)	(0.002)
Control variables in Table 3	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	15,944	15,944	5325	5325	10,619	10,619
R-squared	0.173	0.116	0.246	0.192	0.164	0.289
Number of firms	2557	2557	1896	1896	2338	2338

This table reports the results of falsification tests based on two sample periods when there were no changes in dividend tax rates. In Panel A, the sample consists of nonfinancial firms for a 5-year period surrounding a hypothetical 1984 reform, from 1982 to 1986. We define indicator variables $TREAT_i$ and $POST_t$ as follows: $TREAT_i$ equals one if firm i has individual ownership higher than the sample median in 1983 and zero otherwise; $POST_t$ equals one if the year is after 1984 (i.e., 1985–1986) and zero if firm observations are before 1984 (i.e., 1982–1983). In Panel B, the sample consists of nonfinancial firms for a 9-year period surrounding a hypothetical 2008 reform, from 2004 to 2012. We define indicator variables $TREAT_i$ and $POST_t$ as follows: $TREAT_i$ equals one if firm i has individual ownership higher than the sample median in 2007 and zero otherwise; $POST_t$ equals one if the year is after 2008 (i.e., 2009–2012) and zero if firm observations are before 2008 (i.e., 2004–2007). The first two columns are based on the full sample, the middle two columns are the subsample of overinvesting firms, and the last two columns are the subsample of underinvesting firms. In both panels, the dependent variables are the level of investment in odd-numbered columns and the magnitude of investment efficiency in even-numbered columns. The control variables in Table 3 are included in all models. The detailed definitions of all variables are provided in Appendix A. The superscripts ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

otherwise; $POST_t$ equals one if the year is after 2008 (i.e., 2009–2012) and zero if firm observations are before 2008 (i.e., 2004–2007).

In both Panel A and Panel B, none of the coefficients of $TREAT_i \times POST_t$ differ significantly from zero. The results suggest that in the absence of dividend tax rate changes, the assumption that individual ownership generally does not matter for firms' investment decisions or investment efficiency is supported. Thus, these results suggest that the effects documented in Section 4.1 are not driven by some spurious correlations.

4.5. Robustness tests

4.5.1. Alternative measure of investment opportunities

In this subsection, we provide evidence for the impact of the 2003 tax reform using an alternative measure of investment opportunities. Specifically, we compute the magnitude of investment efficiency as minus one multiplied by the absolute value of the residuals from the industry-year cross-sectional regressions of investment on lagged Tobin's q instead of lagged sales growth. Although Tobin's q is significantly contaminated by noisy stock prices, it contains forward-looking information about a firm's future investment opportunities. Our main results are very similar when we use Tobin's q as a measure of investment opportunities. Across all seven columns in Panel A of Table 7, the signs and even the magnitudes of the DD coefficients are almost identical to those in Table 3.

4.5.2. Alternative test windows

In this subsection, we examine whether we find similar results when we use narrower test windows. Unlike our main analysis based on a nine-year window (1999–2007 excluding 2003), the robustness tests are based on a 7-year window (2000–2006 excluding 2003) and a 5-year window (2001–2005 excluding 2003). Panel B of Table 7 reports the DD results with narrower test windows. When narrower test windows are used, we have similar findings. The tax reform increased

Table 7
Robustness tests.

Panel A. Using Tobin's q as a measure of investment opportunities

Model	Full sample			Overinvesting firms		Underinvesting firms	
	$INV_{i,t}$ (1)	$INV_{i,t}$ (2)	$INVEFF_{i,t}$ (3)	$INV_{i,t}$ (4)	$INVEFF_{i,t}$ (5)	$INV_{i,t}$ (6)	$INVEFF_{i,t}$ (7)
$TREAT_i \times POST_t$	−0.002	−0.005	0.018***	−0.042**	0.041**	0.008***	0.006***
	(0.007)	(0.012)	(0.005)	(0.017)	(0.016)	(0.002)	(0.002)
Control variables in Table 3	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Governance controls	No	Yes	No	No	No	No	No
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	17,715	7144	17,715	6013	6013	11,702	11,702
R-squared	0.181	0.211	0.125	0.267	0.197	0.205	0.342
Number of firms	2900	1336	2900	2164	2164	2663	2663

Panel B. Effect of the tax reform on investment efficiency—Alternative test windows

Model	Sample period: 2000–2006			Sample period: 2001–2005		
	Full sample (1)	Overinvesting firms (2)	Underinvesting firms (3)	Full sample (4)	Overinvesting firms (5)	Underinvesting firms (6)
$TREAT_i \times POST_t$	0.016***	0.034**	0.005**	0.011**	0.030*	0.004*
	(0.005)	(0.016)	(0.002)	(0.005)	(0.017)	(0.002)
Control variables in Table 3	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	14,241	4847	9394	9814	3376	6438
R-squared	0.123	0.226	0.309	0.087	0.197	0.232
Number of firms	3007	2055	2696	2947	1790	2522

Panel A presents the results of the difference-in-differences analyses when we compute the magnitude of investment efficiency as minus one multiplied by the absolute value of the residuals from the industry-year cross-sectional regressions of investment on lagged Tobin's q instead of lagged sales growth. The sample consists of nonfinancial firms for a 9-year period surrounding the 2003 capital taxation reform, from 1999 to 2007. We exclude firm-year observations in 2003 since the 2003 tax reform was enacted on May 28, 2003. The first three columns are based on the full sample, the middle two columns the subsample of overinvesting firms, and the last two columns the subsample of underinvesting firms. In Columns (1), (2), (4), and (6) (Columns (3), (5), and (7)), the dependent variables are the level of investment (the magnitude of investment efficiency), as defined in Section 3.3.1. The definition of control variables are provided in Section 3.3.2. Panel B presents the results of the difference-in-differences analyses with narrower test windows. Unlike our main analysis based on a nine-year window (1999–2007 excluding 2003), the robustness tests are based on a 7-year window (2000–2006 excluding 2003) and a 5-year window (2001–2005 excluding 2003). We exclude firm-year observations in 2003 since the 2003 tax reform was enacted on May 28, 2003. In all models in Panel B, the dependent variable is investment efficiency, as defined in Section 3.3.1. The main control variables in Table 3 are included in all models. For both narrower test windows, we estimate the results using the full sample and the subsamples of overinvesting firms and underinvesting firms. The detailed definitions of all variables in both panels are provided in Appendix A. The superscripts ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

investment efficiency for firms in the full sample and the subsamples of overinvesting firms and underinvesting firms. In addition, the tax reform's effect of increasing investment efficiency is stronger among overinvesting firms. However, the magnitudes of the effects are slightly smaller and weakly significant when a narrower test window is used compared to when a longer test window is used. This finding is consistent with our prediction that it takes several years for a tax reform to have effects on the efficiency of investment decisions.

5. Testing economic mechanisms

In this section, we investigate the economic mechanisms through which the 2003 capital taxation reform increased the efficiency of corporate investment.

5.1. Relaxation of financing constraints

In this subsection, we examine whether the tax reform relaxed financing constraints and lowered the costs of equity and debt. "Traditional view" neoclassical models on the effect of dividend taxes on investment (e.g., Harberger, 1962; Feldstein, 1970; Poterba and Summers, 1984) suggest that a dividend tax cut reduces firms' cost of capital—the pretax rate of return required on marginal investments—because it reduces the taxes that must be paid when profits are distributed to shareholders. Thus, underinvesting firms can raise new investment funds and increase investment. In line with the "traditional view" predictions, we posit that the 2003 tax reform increased investment efficiency by relaxing the financial constraints of underinvesting firms. Relatedly, using a linear regression framework, Dhaliwal et al. (2007a) show that the cost of equity capital decreases after the 2003 tax reform and that the magnitude of the effect of the tax reform decreases in ownership by

Table 8
Effect of the tax reform on financing constraints.

Model	KZ index ($KZ_{i,t}$)			SA index ($SA_{i,t}$)		
	Full sample (1)	Overinvesting firms (2)	Underinvesting firms (3)	Full sample (4)	Overinvesting firms (5)	Underinvesting firms (6)
$TREAT_i \times POST_t$	−5.386***	−3.396	−5.081***	−0.059***	−0.078***	−0.043***
	(1.364)	(2.621)	(1.729)	(0.009)	(0.018)	(0.009)
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4300	1446	2854	14,241	4847	9394
R-squared	0.089	0.104	0.137	0.229	0.327	0.196
Number of firms	1436	801	1203	3007	2055	2696

This table reports the results of the difference-in-differences analyses of the effect of the 2003 tax cut on financing constraints. The sample consists of nonfinancial firms for a 9-year period surrounding the 2003 capital taxation reform, from 1999 to 2007. We exclude firm-year observations in 2003 since the 2003 tax reform was enacted on May 28, 2003. We estimate the results using the full sample and the subsamples of overinvesting firms and underinvesting firms. The dependent variables are Kaplan and Zingales' (1997) KZ index in Columns (1) through (3) and Hadlock and Pierce's (2010) SA index in Columns (4) through (6). The detailed definitions of all variables are provided in Appendix A. The superscripts ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 9
Effect of the tax reform on external financing costs.

Model	Cost of equity ($COE_{i,t}$)			Cost of debt ($COD_{i,t}$)		
	Full sample (1)	Overinvesting firms (2)	Underinvesting firms (3)	Full sample (4)	Overinvesting firms (5)	Underinvesting firms (6)
$TREAT_i \times POST_t$	−0.381***	−0.581***	−0.385***	−0.138**	−0.156	−0.145
	(0.113)	(0.188)	(0.142)	(0.063)	(0.105)	(0.090)
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	11,521	4049	7472	4935	1828	3107
R-squared	0.167	0.227	0.196	0.176	0.390	0.257
Number of firms	2292	1589	2030	1848	1107	1431

This table reports the results of the difference-in-differences analyses of the effect of the 2003 tax cut on external financing costs, i.e., costs of debt and equity. The sample consists of nonfinancial firms for a 9-year period surrounding the 2003 capital taxation reform, from 1999 to 2007. We exclude firm-year observations in 2003 since the 2003 tax reform was enacted on May 28, 2003. We estimate the results using the full sample and the subsamples of overinvesting firms and underinvesting firms. The dependent variables are the cost of equity ($COE_{i,t}$) in Columns (1) through (3) and the cost of debt ($COD_{i,t}$) in Columns (4) through (6). The detailed definitions of all variables are provided in Appendix A. The superscripts ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

institutional investors. Thus, our DD analyses are quite likely to find a negative effect of the tax reform on the cost of equity capital, as the treated firms are defined as firms with high individual ownership, i.e., low institutional ownership.

First, we perform DD analyses for the effect of the 2003 tax cut on the financing constraint measures: Kaplan and Zingales' (1997) KZ index and Hadlock and Pierce's (2010) SA index. To examine whether this mechanism works better for underinvesting firms, we perform the analyses using the full sample and the subsamples of overinvesting and underinvesting firms. Table 8 reports the results of the DD analyses in which the dependent variable is the degree of financing constraints. The right-hand side of the DD model is the same as Eq. (1) except for the control variables.[22] The DD results in the first three columns suggest that the tax reform relaxed financing constraints measured by the KZ index only for underinvesting firms. The SA index is the dependent variable in the last three columns, where the results are slightly different. The tax reform lowered the SA index for both overinvesting firms and underinvesting firms.

The analyses of the accounting cash-flow identity presented in the online appendix (Section B) suggest that the 2003 tax reform strengthened the balance sheet and lowered leverage. Thus, it is probable that the cost of debt was also lowered by the tax reform. To examine whether both costs of debt and equity were lowered by the dividend tax cut, we perform DD analyses for both costs of capital. Again, we perform the analyses using the full sample and the subsamples of overinvesting and underinvesting firms. Table 9 reports the results of the DD analyses in which the dependent variable is the cost of debt or equity. The dependent variables are the cost of equity ($COE_{i,t}$) in the first three columns and the cost of debt ($COD_{i,t}$) in the last three columns.[23] The results show that tax reform significantly lowered the cost of equity but did not significantly lower the

[22] We do not need to control for the determinants of financial constraints as we measure the degree of financing constraints using the determinants of financing constraints. For example, the KZ index is measured using cash flows, market-to-book ratio, leverage, dividends, and cash holdings. See Appendix A for details. Notably, we obtain very similar results when we include all control variables.

[23] We do not include control variables for these analyses, but the results are almost identical when we include all control variables.

Table 10
Agency problems and the effect of the tax reform on investment efficiency.

Model	$HighAC_{i,t-1} = LessBlock_{i,t-1}$			$HighAC_{i,t-1} = LowCeoOwn_{i,t-1}$		
	Full sample (1)	Overinvesting firms (2)	Underinvesting firms (3)	Full sample (4)	Overinvesting firms (5)	Underinvesting firms (6)
$HighAC_{i,t-1} \times TREAT_i \times POST_t$	0.027***	0.071***	0.003	0.063***	0.118**	0.009
	(0.008)	(0.023)	(0.003)	(0.022)	(0.054)	(0.012)
$HighAC_{i,t-1} \times TREAT_i$	−0.019*	−0.027	−0.008*	−0.005	−0.054	−0.006
	(0.010)	(0.037)	(0.004)	(0.027)	(0.044)	(0.013)
$HighAC_{i,t-1} \times POST_t$	−0.019***	−0.047**	0.001	−0.014	−0.013	−0.002
	(0.007)	(0.022)	(0.003)	(0.012)	(0.027)	(0.006)
$HighAC_{i,t-1}$	0.010*	0.014	0.001	0.008	0.019	0.001
	(0.005)	(0.014)	(0.002)	(0.010)	(0.025)	(0.005)
Control variables in Table 3	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	10,428	3454	6974	2859	982	1877
R-squared	0.133	0.282	0.376	0.177	0.509	0.434
Number of firms	2487	1600	2192	920	532	780

This table presents the results of the difference-in-differences analyses designed to investigate whether the effect of the 2003 tax reform on investment efficiency is magnified by the severity of shareholder-manager agency conflicts. The sample consists of nonfinancial firms for a 9-year period surrounding the 2003 capital taxation reform, from 1999 to 2007. We exclude firm-year observations in 2003 since the 2003 tax reform was enacted on May 28, 2003. We estimate the results using the full sample and the subsamples of overinvesting firms and underinvesting firms. Using the number of blockholders and the percentage of shares owned by the chief executive officer (CEO), we construct two variables indicating high shareholder-manager agency conflicts: 1) $LessBlock_{i,t-1}$ and 2) $LowCeoOwn_{i,t-1}$. In all models, the dependent variable is the magnitude of investment efficiency, as defined in Section 3.3.1. The control variables in Table 3 are included in all models. The definitions of these variables are provided in Appendix A. The superscripts ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

cost of debt. The results are not very different between overinvesting and underinvesting firms. These results are consistent with the findings of Dhaliwal et al. (2007a) and Lin and Flannery (2013).

Overall, our results support Hypothesis 2B, except that the 2003 tax cut did not significantly lower the cost of debt. We find evidence that the dividend tax cut reduced underinvestment problems by relaxing financing constraints and particularly by lowering the cost of equity.

5.2. Reduction of shareholder-manager conflicts

In this subsection, we investigate whether the effect of the 2003 tax reform on investment efficiency is magnified by the severity of shareholder-manager agency conflicts. Chetty and Saez (2010), using an "agency conflict" model of the firm, show that dividend taxation induces managers to undertake unproductive investments (i.e., overinvest) by retaining earnings. Chetty and Saez further show that dividend taxes create a deadweight cost by distorting the tradeoff between pet project investments and dividend payouts, even if the marginal source of investment is retained earnings. Thus, a dividend tax cut leads to an increase in dividend payments and a decrease in overinvestment because it increases the manager's preference for dividends relative to pet projects and increases the amount of monitoring by large shareholders.

Table 10 reports the DD results. To measure shareholder-manager agency conflicts, we use the number of blockholders, the percentage of shares owned by the chief executive officer (CEO), and the percentage of shares owned by all executives. As an indicator variable for high shareholder-manager agency conflicts ($HighAC_{i,t-1}$), we define the following three variables: 1) $LessBlock_{i,t-1}$; 2) $LowCeoOwn_{i,t-1}$; and $LowExeOwn_{i,t-1}$. The definitions of the three variables are provided in Appendix A. The first three columns in Table 10 report the DD results when $LessBlock_{i,t-1}$ is used as an indicator variable for high agency conflicts ($HighAC_{i,t-1}$), while the last three columns in Table 10 report the DD results when $LowCeoOwn_{i,t-1}$ is used. The results in Columns (1) and (4) suggest that regardless of agency conflict measures, the effect of dividend tax cuts on investment efficiency is greatest among firms with high agency conflicts.[24] The subsample analyses show that high agency conflicts, as measured by fewer blockholders and low CEO ownership, magnify the effect of dividend tax cuts on investment efficiency only for overinvesting firms.

Our results suggest that the 2003 dividend tax cut increases the investment efficiency of overinvesting firms more significantly when shareholder-manager agency conflicts are severe, supporting Hypothesis 3B. The results are consistent with Chetty and Saez's (2010) agency conflict models predicting that the 2003 dividend tax cut lowered the problem of overinvesting free cash flows by increasing the manager's preference for dividends relative to pet projects and increasing the amount of monitoring by large shareholders.

[24] We find consistent results when we use $LowExeOwn_{i,t-1}$ as an indicator variable for high agency conflicts. However, the results for overinvesting firms are not significant at a conventional level. This finding suggests that the percentage of shares owned by all executives is negatively, but weakly, associated with the severity of agency conflicts.

Table 11
Dividend-paying firms versus nondividend-paying firms.

Model	Nondividend-paying firms			Dividend-paying firms		
	Full sample (1)	Overinvesting firms (2)	Underinvesting firms (3)	Full sample (4)	Overinvesting firms (5)	Underinvesting firms (6)
$TREAT_i \times POST_t$	0.020***	0.048**	0.008***	0.009	0.016	0.002
	(0.007)	(0.020)	(0.003)	(0.007)	(0.027)	(0.003)
Control variables in Table 3	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	12,565	4534	8031	5714	1628	4086
R-squared	0.131	0.217	0.313	0.148	0.395	0.466
Number of firms	2201	1621	1969	850	627	817

This table reports the results of the difference-in-differences analyses designed to examine whether there are cross-sectional differences depending on whether firms paid dividends in 2002. Each sample consists of nonfinancial firms for a 9-year period surrounding the 2003 capital taxation reform, from 1999 to 2007. We exclude firm-year observations in 2003 since the 2003 tax reform was enacted on May 28, 2003. The results are presented for the subsamples of overinvesting and underinvesting firms and the full sample that contains both overinvesting and underinvesting firms. In all models, the dependent variable is the magnitude of investment efficiency, as defined in Section 3.3.1. The control variables in Table 3 are included in all models. The detailed definitions of all variables are provided in Appendix A. The superscripts ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

6. Additional analyses

6.1. Dividend-paying firms versus nondividend-paying firms

In this subsection, we examine whether there are cross-sectional differences in the DD effects of a large dividend tax cut on investment efficiency, particularly depending on whether firms were paying dividends prior to the 2003 tax reform. To do so, we group firms into dividend-paying firms and nondividend-paying firms based on whether firms paid dividends in 2002.

We expect that the two major economic mechanisms presented in Section 2 and tested in Section 5 are magnified for nondividend-paying firms compared to dividend-paying firms. Cash-rich nondividend-paying firms face more severe shareholder–manager agency conflicts due to excessive free cash flow problems (Jensen, 1986), and therefore, the effect that a large dividend tax cut has on the investment efficiency of overinvesting firms is expected to be greater for nondividend-paying firms. In addition, nondividend-paying firms are in general more financially constrained (Almeida et al., 2004; Faulkender and Wang, 2006). Thus, it is expected that the extent to which a large cut in dividend taxes relaxes financing constraints is greater for cash-poor underinvesting firms that do not pay out dividends and that the extent to which a large dividend tax cut improves the investment efficiency of underinvesting firms is therefore also greater for nondividend-paying firms.

Table 11 presents the DD results separately for nondividend-paying firms and dividend-paying firms. Consistent with our prediction, Panel A clearly shows that the DD effects of the 2003 tax reform on corporate investment efficiency exist for nondividend-paying firms but not for dividend-paying firms. The strong positive DD effects of the tax reform on investment efficiency for nondividend-paying firms and the negligible DD effects for dividend-paying firms are found not only in the full sample but also in the subsamples of overinvesting and underinvesting firms. Notably, the DD effects for nondividend-paying firms are somewhat stronger than those reported in Table 3.

These results suggest that corporate reactions to the dividend tax cut are concentrated on firms that did not pay out dividends prior to the tax reform, implying that nondividend-paying firms are particularly well suited for empirically testing the hypotheses set out in this paper. Although we do not delve into the issue of dividend payout policy further in this paper, it is highly likely that firms with more severe agency conflicts and firms under more severe financial constraints are more commonly found among nondividend-paying firms rather than among dividend-paying firms. Dividend-paying firms are generally less financially constrained and unlikely to face severe agency conflicts driven by excessive free cash flows.

6.2. DD results based on alternative U.S. dividend tax rate changes

In this subsection, we examine whether alternative U.S. dividend tax rate changes (e.g., Tax Reform Act of 1986, Omnibus Budget Reconciliation Act of 1993, American Taxpayer Relief Act of 2012) impacted corporate investment and its efficiency in a consistent manner. Our investigation suggests that the DD results are overall consistent with our predictions and our main results presented in Section 4, although the results are somewhat weaker than the results reported in Table 3. For example, the coefficients on the interaction term, $TREAT_i \times POST_t$, in investment efficiency models are all positive in case of the 1986 reform, and the coefficients in investment efficiency models are all negative in cases of the 1993 reform and the 2012 reform. Given that the 1986 reform reduced dividend tax rates and the 1993 and 2012 reforms increased dividend tax rates, all of these results are consistent with the results reported in Section 4. The DD results based on alternative U.S. dividend tax rate changes and more detailed discussions are provided in the online appendix (Section A).

7. Conclusion

The Jobs and Growth Tax Relief Reconciliation Act lowered the maximum individual income tax rate on dividends to 15% in an attempt to stimulate the U.S. economy. Based on existing studies suggesting that a dividend tax cut should substantially reduce a firm's cost of capital (Harberger, 1962; Feldstein, 1970; Poterba and Summers, 1984) and that corporate investment should respond sensitively to the change in the cost of capital (Hall and Jorgenson, 1967; Cummins et al., 1994; Caballero et al., 1995), policymakers expected that the tax cut would increase corporate investment and employment.

Employing a DD method, we examine the effect of the 2003 dividend tax cut on corporate investment efficiency. Since the dividend tax cut does not apply to nontaxable institutional investors, we select treated firms by using the likelihood that a firm's marginal investor was an individual investor prior to the 2003 tax reform. We find that the 2003 tax reform improved firms' investment efficiency by relaxing financial constraints for underinvesting firms and mitigating the free cash flow problem of overinvesting firms; that is, the tax cut increased the level of investment of underinvesting firms but decreased the level of investment of overinvesting firms. In addition, we find that the 2003 tax reform did not have a significant effect on the level of corporate investment in the full sample consisting of both overinvesting and underinvesting firms.

While the results that the 2003 tax reform did not influence firm-level investment or aggregate investment are consistent with the "neutrality view" models, the results that the dividend tax cut reduced the underinvestment problems by relaxing financing constraints are consistent with the "traditional view" models. However, these "traditional view" and "neutrality view" neoclassical models do not explain the results that the dividend tax cut reduced the overinvestment problems. The results are consistent with "agency conflict" models (e.g., Chetty and Saez, 2010) predicting that a dividend tax cut would reduce the problem of overinvesting free cash flows by increasing the manager's preference for dividends relative to investments in pet projects and by increasing the intensity of monitoring by large shareholders.

Employing an improved empirical framework beyond that of the existing studies to test agency-theoretic models based on Jensen and Meckling's (1976) intuition (e.g., Chetty and Saez, 2010), including the data from U.S. public firms with sufficient variation in corporate governance, a longer sample period, an accounting measure of investment efficiency, and subsamples based on whether firms overinvest or underinvest, this study enhances our understanding of the mechanisms through which a large dividend tax cut helps improve corporate investment efficiency and thus increases corporate valuation.

Data availability

Data will be made available on request.

Appendix A. Description of variables

Variable	Description
Investment-related variables	
$INV_{i,t}$	Rate of total investment, measured as the sum of capital expenditures, acquisitions, and R&D expenditures (net of the sale of property, plants, and equipment (PPE)) divided by lagged book total assets
$INVEFF_{i,t}$	Investment efficiency, computed as minus one times the absolute value of the deviation from the expected or optimal level of *INV*
Test variables	
$TREAT_i$	An indicator variable that equals one if firm *i*'s individual ownership in 2002 is greater than or equal to the sample median in 2002 and zero if firm *i*'s individual ownership in 2002 is smaller than the sample median in 2002
$IndOwn_{i,2002}$	Firm *i*'s individual ownership in 2002, defined as one minus firm *i*'s institutional ownership in 2002
$TREAT_i^{TO}$	A refined treatment status indicator based on both individual ownership and stock turnover, which equals one if $TREAT_i = 1$ and firm *i*'s stock turnover is above the fifth quintile in 2002 (among the treated firms based on the original proxy) and equals zero if $TREAT_i = 0$ and firm *i*'s stock turnover is below the first quintile in 2002 (among the control firms based on the original proxy)
$TREAT_i^{TS}$	A refined treatment status indicator based on both individual ownership and the proportion of tax-sensitive institutional ownership, which equals one if $TREAT_i = 1$ and firm *i*'s tax-sensitive institutional ownership proportion is above the sample median in 2002 (among the treated firms based on the original proxy) and equals zero if $TREAT_i = 0$ and firm *i*'s tax-sensitive institutional ownership proportion is below the sample median in 2002 (among the control firms based on the original proxy)
$HighCAR_i$	An indicator variable that equals one if both $CAR_i^{January}$ and CAR_i^{May} are greater than the fifth quintile and zero otherwise, where $CAR_i^{January}$ and CAR_i^{May} indicate the cumulative abnormal returns (CAR) for the two event windows when the market received positive signals about the passage of the 2003 tax cut, i.e., January 3–9, 2003, and May, 14–28, 2003
$POST_t$	An indicator variable that equals one if the year is after 2003 and zero if the year is before 2003
Control variables	
$LOGASSET_{i,t}$	Natural logarithm of the book value of total assets
$Q_{i,t}$	Tobin's Q, computed as the ratio of the market value of total assets to the book value of total assets
$SD\_CFO_{i,t}$	Standard deviation of the cash flow from operations deflated by the average book assets, calculated using the data from year $t-5$ to $t-1$
$SD\_SALES_{i,t}$	Standard deviation of sales deflated by the average book total assets, calculated using the data from year $t-5$ to $t-1$

(*continued on next page*)

(*continued*)

Variable	Description
$SD\_INV_{i,t}$	Standard deviation of the sum of capital expenditures, acquisitions, and R&D expenditures (net of the sale of property, plants, and equipment (PPE)) divided by average book total assets, calculated using the data from year $t-5$ to $t-1$
$Z_{i,t}$	Altman's Z score, calculated as (3.3 × pretax income + sales + 1.4 × retained earnings + 1.2 × (current assets − current liabilities))/total assets
$TANG_{i,t}$	Ratio of PPE to book total assets
$LEV_{i,t}$	Ratio of long-term debt to the sum of long-term debt and the market value of equity
$IND\_LEV_{i,t}$	Mean leverage for firms in the same SIC 3-digit industry
$CFO\_SALES_{i,t}$	Ratio of cash flows from operations to sales
$SLACK_{i,t}$	Ratio of cash to PPE
$DIV_{i,t}$	An indicator variable that takes the value of one if the firm paid a dividend and zero otherwise
$AGE_{i,t}$	Firm age, defined as the number of elapsed years since the firm appeared in CRSP
$OPERATINGCYCLE_{i,t}$	Operating cycle, defined as the natural logarithm of the sum of the ratio of receivables to sales and the ratio of inventory to the costs of goods sold multiplied by 360
$LOSS_{i,t}$	An indicator variable that takes the value of one if net income before extraordinary items is negative and zero otherwise
$CASH_{i,t}$	Ratio of cash to book total assets
$ANALYSTS_{i,t}$	Analyst coverage, defined as the number of analysts following the firm, as provided by I/B/E/S
$INVG_{i,t}$	Gompers et al.'s (2003) G index multiplied by minus one (if the G index is missing, set it to zero)
Components of accounting cash-flow identity	
$DIV_{i,t}$	Dividend ratio, defined as cash dividends divided by total assets
$I_{i,t}$	Investment rate, defined as the sum of capital expenditures, the increase in investments, acquisitions, and other uses of funds less the sale of PPE and investment divided by total assets
$\Delta WC_{i,t}$	Change in working capital, defined as the change in working capital divided by total assets
$CF_{i,t}$	Operating cash flow, defined as the sum of income before extraordinary items, depreciation and amortization expenses, extraordinary items and discontinued operations, deferred taxes, equity in net loss (earnings), other funds from operations, and the gain (loss) from sales of PPE and other investments divided by total assets
$\Delta D_{i,t}$	Net debt issuance, defined as the issuance of long-term debt less the reduction in long-term debt divided by total assets
$\Delta E_{i,t}$	Net equity issuance, defined as the sale of common stock less the purchase of common stock divided by total assets
Financing constraint measures	
$KZ_{i,t}$	Kaplan and Zingales' (1997) KZ index, measured as $KZ = -1.001909 \times CF + 0.2826389 \times MB + 3.139193 \times Debt - 39.3678 \times Div - 1.314759 \times Cash$, where *CF* denotes the income before extraordinary items plus depreciation and amortization expenses divided by PPE, *MB* is defined as the sum of the book value of total debt and the market value of equity minus common equity-deferred tax divided by the book value of total assets, *Debt* is defined as the sum of long-term debt and debt in current liabilities divided by the sum of long-term debt, debt in current liabilities, and shareholders' equity, Div is computed as dividends paid divided by PPE, and *Cash* is defined as cash holdings divided by PPE
$SA_{i,t}$	Hadlock and Pierce's (2010) SA index, which relies on firm size and firm age to measure financial constraints: $SA = -0.737 \times Size + 0.043 \times Size^2 - 0.040 \times Age$, where *Size* is the natural logarithm of inflation-adjusted book assets (base year = 2004) and *Age* is the number of years with information in Compustat (*Size* is winsorized at 4.5 billion dollars and firm age at 37 years)
External financing cost measures	
$COE_{i,t}$	Pástor et al.'s (2008) implied cost of equity, which is defined as the internal rate of return that equates the present value of future dividends with the current stock price, where future dividends are calculated using earnings forecast information from the I/B/E/S database
$COD_{i,t}$	A cost of debt measure, defined as the all-fees-in spread on corporate syndicated loans obtained from the Thompson Reuters DealScan database
Agency conflicts measures	
$HighAC_{i,t}$	An indicator variable for high shareholder-manager agency conflicts
$LessBlock_{i,t}$	An indicator variable that equals one if the number of blockholders is less than or equal to its sample median (i.e., 2) and zero otherwise
$LowCeoOwn_{i,t}$	An indicator variable that equals one if CEO ownership percentage is less than or equal to its sample median and zero otherwise

Appendix B. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.jacceco.2022.101514.

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