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ABSTRACT

Does the Stock Market Harm Investment Incentives?*

We examine whether stock market-listed firms in the U.S. invest suboptimally due to agency costs resulting from separation of ownership and control. We derive testable predictions to distinguish between underinvestment due to rational “short-termism” and overinvestment due to “empire building.” Empirical identification relies on a proxy for optimal investment derived from a rich new data source on unlisted U.S. firms. Listed firms invest less and are less responsive to changes in investment opportunities compared to matched unlisted firms, especially in industries in which stock prices are particularly sensitive to current profits. Listed firms also tend to smooth their earnings growth and dividends and are reluctant to report negative earnings. These findings are consistent with short-termism and contrary to what one would expect if empire-building were the dominant agency problem in the stock market. Our results suggest that the stock market harms investment incentives, at least for the fast-growing companies in our sample.

JEL Classification: D21, G31, G32 and G34
Keywords: agency problems, corporate investment, empire building, ipos, managerial incentives, managerial myopia, private companies and short-termism

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Stock market-listed companies in the U.S. enjoy access to a deep pool of capital. Stock market investors supply their savings at relatively low cost because the ability to buy small stakes in many companies helps diversify risk and because the ability to sell at short notice means their savings are relatively liquid. A stock market listing should thus reduce the cost to companies of funding their investment plans and thereby contribute to innovation and economic growth.

But a stock market listing may also lead to agency problems as ownership and managerial control are separated and managers' interests diverge from those of their investors (Berle and Means (1932), Jensen and Meckling (1976)). As a result, managers might invest capital sub-optimally from the point of view of shareholders. Depending on their preferences and incentives, managers may overinvest or underinvest. Overinvestment – or 'empire-building' – results from a preference for scale (Baumol (1959), Stulz (1990)). Underinvestment – due to 'managerial myopia' or 'short-termism' – results if managers care about the current stock price and shareholders cannot tell if current profits are low because the company is in trouble or because it has made investments whose expected contributions to profits will take many years to materialize (Miller and Rock (1985), Stein (1989), Shleifer and Vishny (1990), Bebchuk and Stole (1993), von Thadden (1995), and Holmström (1999)).[1]

In this paper, we examine empirically whether stock market listings, on net, harm investment incentives. We do so by comparing the investment decisions of U.S. stock market-listed (or 'public') companies to those of similar-sized unlisted (or 'private') U.S. firms in the same industry. We find some evidence that public firms invest less compared to private firms and strong evidence that they are significantly less responsive to changes in investment opportunities. These results are most pronounced in industries in which stock prices are particularly sensitive to current profits. Based on straightforward extensions to the models of Stein (1989) and Stein (2003), we show that these patterns are more nearly consistent with managerial myopia than with empire-building.[2] We also report evidence that public firms smooth both their earnings growth and their dividends and avoid

[1] The argument that shareholders cannot easily understand the sources of fluctuations in reported profits is widely made in finance and economics. Duffie (2010), for example, uses it to explain the growth in over-the-counter derivatives.
[2] The lack of support for empire building echoes Bertrand and Mullainathan's (2003) conclusion that public-firm managers would rather "enjoy the quiet life" than overinvest.

reporting negative earnings, compared to private firms. These findings suggest that, from an investment perspective at least, the agency costs of a stock market listing outweigh the benefits of a reduced cost of capital, at least for the fast-growing entrepreneurial firms in our sample.

The maintained hypothesis of our empirical design is that the agency problems that public firms are subject to are less prevalent, or even absent, among private ones. It is easy to see why. First, the fact that the shares of private firms cannot easily be traded means that managers need not worry about the short-term effect of investing in long-term projects on the current value of their stock. Thus, short-termism is unlikely to arise. Second, private firms are typically owned by a small number of controlling shareholders who either manage the firm themselves or oversee management closely (Cornelli, Kominek, and Ljungqvist (2010)). As a result, their shareholders tend to have access to the same or similar information as management, making it easier to control any remaining agency conflicts. Public companies, by contrast, face frictions that exacerbate information asymmetries between shareholders and managers. They restrict the information flow to their investors, in part to avoid disclosing information that could be used by product-market competitors (Bhattacharya and Chiesa (1995)) and in part to comply with the SEC's Regulation Fair Disclosure, which bans selective disclosure of non-public information.

Private firms are subject to no public reporting requirements, so very little is known about their investment behavior.[3] Our study is possible only because a new database on private U.S. firms, created by Sageworks Inc. in cooperation with hundreds of accounting firms, has recently become available. The contents of the Sageworks database mirror Compustat, the standard database for public U.S. firms. It contains balance sheet and income statement data for 95,370 unique private firms over the period 2002 to 2007. Matching Sageworks and Compustat on firm size and industry, we identify matched panels of (small) public and (large) private firms and then estimate standard empirical investment equations.

[3] A notable exception, albeit affecting relatively few companies, is unlisted firms that have sold public bonds in the U.S.; see Helwege and Packer (2009).

To better understand our identification strategy, it is instructive to think about what the ideal empirical experiment would look like. To shed light on the trade-off between lower funding costs and greater agency problems as ownership and control are separated, we need a benchmark for what investment would look like absent agency problems. Finding such a counterfactual is the primary identification challenge. The Sageworks data on private firms provide a plausible proxy for the counterfactual.[4]

A secondary identification challenge is that our public and private sample firms could differ in unobserved ways, and these unobserved differences – rather than their public or private status – might drive observed differences in investment behavior. One way to address this problem is to hold firm characteristics constant by estimating within-firm changes in investment behavior as a firm transitions from private to public. Leaving aside for a moment concerns about selection biases due to the endogeneity of the IPO decision, such a test is not possible in the Sageworks data as company names are masked. Instead, we use data from Thomson Financial for a subset of U.S. firms that go public, namely those that do so without raising capital. The identifying assumption is that these firms went public for reasons other than to fund investment, which reduces – but does not eliminate – endogeneity concerns. Our tests show that these firms were significantly more responsive to variation in investment opportunities in the five years before they went public than after; indeed, once they are public, their investment sensitivity is indistinguishable from that of other public companies.

These experiments do not rule out selection biases. Prime candidates for instrumenting the IPO decision are discontinuities around stock exchange listing standards and the 2002 Sarbanes-Oxley Act, whose Section 404 created compliance costs that are largely fixed and so make it relatively less attractive to be a small publicly traded company.[5] However, neither provides sufficient variation. Most listing standards can be satisfied by the very act of going public, and the remaining standards –

[4] Effectively, we are able to exploit variation along the extensive (public/private) margin. Given data constraints, existing identification strategies instead rely on variation on the intensive margin, measuring differences in investment behavior between public firms with higher or lower proxies for agency costs. See, for example, Wurgler (2000) and Knyazeva et al. (2007).
[5] See, for example, the survey evidence available at http://www.foley.com/news/news_detail.aspx?newsid=3074.

concerning profitability – are set so low that they are not a binding constraint for most of our private firms.[6] Our sample post-dates Sarbanes-Oxley, and while some of its provisions were phased in over time for small firms, they did not come into force until after the end of our sample period.

Ultimately, short of a trial that randomly assigns firms to public or private status, we cannot rule out selection biases. However, it is not easy to see how our findings would result from reasonable alternative stories. Such stories would have to explain how *absent* agency problems such as short-termism, the lower cost of capital of a stock market listing would induce firms with higher sensitivity to investment opportunities to choose to stay private while those with lower sensitivity go public.

Rational models of short-termism come in two flavors: Signal-jamming models such as Stein's (1989) which combine ex ante symmetric information with an unobserved action managers can take to boost reported earnings; and signaling models such as Miller and Rock's (1985) in which managers signal their better information about the firm's true profits by paying a high dividend.[7] In either type of model, a manager's utility is assumed to be a function of the company's long-term value and its current stock price.[8] As a result, the manager has an incentive to 'manipulate' the stock price, either by reporting high earnings or high dividends, even if that means reducing investment. In equilibrium, shareholders aren't fooled and the manager gains nothing, but since shareholders rationally anticipate his behavior the manager has no incentive to deviate from it.

To derive empirical predictions representative of short-termism models, Section 1 adapts the Stein (1989) model to derive a necessary and sufficient condition for short-termism. If public-firm managers' incentives are distorted by short-termism and if this distortion outweighs the financing benefit of a stock market listing, our model predicts that investment will both be higher and more

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[6] See http://www.nasdaq.com/about/nasdaq_listing_req_fees.pdf.

[7] There are also behavioral models of short-termism, based on the assumption that noise traders cause inefficiencies in stock prices. Our focus in this paper is on rational models of short-termism.

[8] Garvey, Grant, and King (1999) show how optimal compensation contracts can include a focus on current stock prices. Alternatively, Shleifer and Vishny (1990) argue that it is costlier to arbitrage mispricing of assets whose returns are long-term compared to short-term assets, and hence long-term assets will be more mispriced; such rational short horizons among arbitrageurs can then lead to short horizons among managers if managers seek to avoid undervaluation that could lead to their being fired. Holden and Lundstrum (2009) provide supporting evidence from a quasi experiment.

responsive to investment opportunities among private than among public firms – precisely what we find in the data.[9]

Empire-building models, by contrast, essentially posit that the manager's utility depends, in part, on the size of the firm. As we show in Section 1, under empire building we should expect the investment of public firms to be both higher and more sensitive to investment opportunities than that of private firms – the opposite of what we find in the data. Our simple set-up thus gives us sharp predictions with which to evaluate whether the average public firm suffers from empire-building syndrome or from managerial myopia.

Our paper makes several contributions. First, we provide rare direct evidence of an important cost of going public by documenting that the public firms in our matched sample invest suboptimally *on average* and that they do so in a manner consistent with myopia. Extant studies of short-termism tend to have a narrower focus. Their upshot is that *some* public-firm managers take costly actions to avoid negative earnings surprises as these could lead to share price declines (Skinner and Sloan (2002)).[10]

Second, our results contribute to the agency literature by documenting that the dominant problem for small fast-growing public companies, when it comes to investment, appears to be short-termism rather than empire-building. This adds to existing survey evidence that short-termism is widespread. Poterba and Summers (1995) find that managers of U.S. stock-market listed companies prefer investment projects with shorter time horizons in the belief that stock market investors fail to properly value long-term investments. Similarly, Graham, Harvey, and Rajgopal (2005, p. 3) report

[9] Not everyone agrees that stock market listings induce short-termism. Edmans (2010) argues that even dispersed investors have an incentive to passively monitor management because they can profit by trading on the information they uncover. Assuming short sale constraints, Edmans' model shows that such trading can cause prices to reflect fundamental information rather than current earnings, removing the incentive for managers to pursue short-term objectives.

[10] Bhojraj et al. (2009) focus on public U.S. firms that barely beat earnings forecasts by cutting discretionary spending, a proxy for myopic behavior. Such firms avoid the short-run stock price hit associated with missing earnings forecasts, but over longer horizons are outperformed by firms that miss forecasts and maintain discretionary spending. This suggests that myopia is costly. R&D spending appears to be a particular focus. Baber, Fairfield, and Haggard (1991) document that firms cut R&D spending to ensure reporting positive reported earnings, while Dechow and Sloan (1991) find that CEOs nearing retirement cut R&D spending to increase earnings. Bushee (1998) shows that these tendencies are mitigated in the presence of high institutional ownership. Roychowdhury (2006) documents that firms aim to meet earnings targets also by boosting sales (through temporary price discounts), overproducing (to lower the cost of goods sold), and reducing discretionary spending (to lift reported profit margins). Bhojraj and Libby (2005) provide related evidence from a series of laboratory experiments. Sheen (2009) analyzes hand-collected investment data for public and private firms in the commodity chemical industry, finding results similar to ours.

the startling finding that “the majority of managers would avoid initiating a positive NPV [net present value] project if it meant falling short of the current quarter’s consensus earnings [forecast].”

Third, we contribute to the empirical investment literature. An enduring empirical puzzle is that public firms’ investment decisions are less sensitive to investment opportunities than neo-classical *Q* theory predicts (see Bond and Van Reenen (2007) for a review). Our paper may shed light on this puzzle by highlighting how short-termism weakens the investment sensitivity of public firms.

There is a small but growing literature contrasting public and private firms. Saunders and Steffen (2009) use data from the U.K. to show that private firms face higher borrowing costs than do public firms, consistent with our modeling assumption. Also using British data, Michaely and Roberts (2007) show that private firms smooth dividends less than public firms. Gao, Lemmon, and Li (2010) compare CEO pay in public and private firms in the U.S., finding that public-firm pay – but not private-firm pay – is sensitive to measureable performance variables such as stock prices and profitability. When a company goes public, pay becomes more performance-sensitive. Since the point of an incentive contract is to overcome an agency problem, these patterns are consistent with our maintained hypothesis that private firms are subject to fewer agency problems than public firms.

The paper that is closest in spirit to ours is Mortal and Reisel (2009). Using the Amadeus database of public and private European companies, Mortal and Reisel find that public firms have *greater* investment sensitivities than private ones. Thus, unlike in our U.S. data, the predominant agency problem in Europe appears to be empire-building.[11]

1. A Model of Investment Behavior and Short-termism

To guide our empirical tests, we model the effects of two major agency problems related to firm investment: Short-termism and empire building. As we will see, despite their relative simplicity, the models have all the features we need to discipline our empirical analysis. All technical derivations and proofs can be found in Appendix A.

[11] A possible caveat is that the Amadeus database suffers from a severe form of survivorship bias because the historical data of dead firms are eliminated from the database. See Popov and Roosenboom (2009) for further details.

1.1. A Model of Investment Behavior and Short-termism

We formalize the effect of short-term incentives on investment decisions by adapting Stein (1989). Consider a firm, either public or private, whose manager decides how much to spend on investment, i_t. The firm's reported free cash flows[12] in period t, e_t, can be written as the sum of the return on prior-period investment, $Qf\left(i_{t-1}\right)$, the dollar cost of current investment, i_t, and a serially correlated stochastic disturbance, e_t^n: $e_t = Qf\left(i_{t-1}\right) - i_t + e_t^n$. Q indexes the firm's investment opportunities and is taken to be common knowledge. We assume a standard functional form for the production function, namely $f\left(i\right) = i^{\alpha}$, with $0 < \alpha < 1$.[13]

The process generating the stochastic cash flow shock is $e_t^n = z_t + v_t$, where $z_t = z_{t-1} + u_t$ and v_t and u_t are drawn from normal distributions with mean zero and variances σ_v^2 and σ_u^2. Neither management nor investors observe v_t and u_t, but everyone knows their distributions.

The manager chooses investment at the beginning of period t, without knowing e_t^n.[14] In the case of a private firm, the manager solves the following problem:

$$\max_i U_t^{Private}\left(i\right) \equiv \max_i E_t^M\left[\left(e_t\left(i\right) + \frac{e_{t+1}\left(i\right)}{\left(1+r\right)\left(1+\delta\right)} + \ldots\right)\right].$$

The manager of a public firm, on the other hand, solves:

$$\max_i U_t^{Pubic}\left(i\right) \equiv \max_i E_t^M\left[\left(e_t\left(i\right) + \frac{e_{t+1}\left(i\right)}{1+r} + \ldots\right) + \pi\left(P_t\left(i\right) + \frac{P_{t+1}\left(i\right)}{1+r} + \ldots\right)\right].$$

The expectations operator E_t^M denotes the expectation conditional on the manager's information set at the beginning of period t, which is the same for public and private firms. This information set

[12] In finance, free cash flow is defined as cash flow available for distribution to the firm's shareholders and creditors. Formally, it is EBITDA (earnings before interest, taxes, depreciation, and amortization) less investment (i.e., capital expenditures and the change in net working capital) less taxes.

[13] Under mild regularity conditions on the 3rd derivative, our qualitative results go through for any investment function with decreasing marginal returns. The main effect of our functional form assumption is to simplify condition (4), below.

[14] The main difference between our model and Stein's (1989) is that the manager's choice variable is current investment rather than 'borrowing' against future earnings at an unfavorable implicit rate of interest.

includes previous cash flows, $\{e_{t-j} : j > 0\}$, and past investments, $\{i_{t-j} : j > 0\}$. We denote by r the one-period discount rate. Private firms may face a higher cost of capital as their shares are relative less liquid than those of publicly traded firms.[15] We capture this possibility by assuming that their future cash flows are discounted at $(1+r)(1+\delta)$, where $\delta \geq 0$, rather than at $(1+r)$.

For public firms, the second sum in the manager's utility function captures the idea that each period, the manager of a public firm has some interest, indexed by π, in his firm's current stock price, P_t. The manager may care about the current stock price because he intends to sell some of his stockholdings (as suggested in Stein (1989) and confirmed empirically by Bhojraj et al. (2009)), because his compensation is tied to the stock price (see Garvey, Grant, and King (1999) for the micro-foundations of such a scheme), or because he fears losing his job in the event of a hostile takeover (Stein (1988)). For private firms, which are unlisted, our maintained assumption is that no such distortion exists as private firms have more concentrated ownership. Note that our tests are biased against finding differences in investment behavior between public and private firms to the extent that our maintained assumption is wrong.

The (ex-dividend) market price of a public firm's stock at time t equals the market's expected present value of the firm's future cash flows:

$$P_t = E_t^I\left[\sum_{j=1}^{\infty}\frac{e_{t+j}}{(1+r)^j}\right]$$

where the expectations operator E_t^I denotes the expectation at time t conditional on the information available to investors. Investors' information set includes current and previous cash flows, $\{e_{t-j} : j \geq 0\}$, and past investments, $\{i_{t-j} : j > 0\}$, but not current-period investment i_t.[16]

Denote by $\bar{i}$ investors' beliefs about a public firm's investment level. (In our model, the

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[15] Pagano, Panetta, and Zingales (1998) show that Italian firms enjoy lower funding costs once they have gone public.

[16] This can be interpreted as a reduced-form of the market having imperfect information regarding the firm's current-period investment, and in particular not being able to distinguish how much of investment is devoted to new productive activities, replacement of depreciated assets, the manager's pet projects, etc.

manager's problem is stationary, so these beliefs are not time dependent.) Then we have that

$$E_t^I\left[e_{t+j}\right]=E_t^I\left[Qf\left(i_{t+j-1}\right)-i_{t+j}+e_{t+j}^n\right]=Qf\left(\bar{i}\right)-\bar{i}+E_t^I\left[e_{t+j}^n\right]$$

for all $j>0$. Because investors do not observe i_t, current investment i_t affects their expectation of future cash flows only through its effect on $E_t^I\left[e_{t+j}^n\right]$. Since the stochastic element of cash flows, e_t^n, is persistent, we can write this expectation as a distributed-lag function:

$$E_t^I\left[e_{t+j}^n\right]=\gamma_0 E_t^I\left[e_t^n\right]+\sum_{k=1}^{\infty}\gamma_k e_{t-k}^n \tag{1}$$

for all $j>0,$ such that $\gamma_k\geq 0,$ $\sum_{k=0}^{\infty}\gamma_k=1$. This follows Stein (1989) and Holmström (1982).

Equation (1) can be interpreted as follows. Past investment levels are observable to everyone, so investors can back out the stochastic element of cash flows e_{t-k}^n in all past periods. However, investors do not observe current investment and so must infer the current level of the cash flow shock, $E_t^I\left[e_t^n\right]$. This enables the manager of a public firm to affect the stock price, P_{t+j}, $j\geq 0$, by choosing investment i_t in such a way as to manipulate $E_t^I\left[e_t^n\right]$ and thus investors' future cash flow expectations, $E_t^I\left[e_{t+j}\right]$. Specifically, we have that $E_t^I\left[e_t^n\right]=e_t-Qf\left(i_{t-1}\right)+\bar{i}$. Therefore, the manager of a public firm can manipulate $E_t^I\left[e_t^n\right]$ by reducing current investment, i_t, which is not observable at time *t*, and thereby increasing current cash flows $e_t=Qf\left(i_{t-1}\right)-i_t+e_t^n$, which investors do observe. Obviously, for such a signal-jamming equilibrium to exist, it must be the case that investors' beliefs about current investment, $\bar{i}$, are consistent with the firm's actual investment i_t. Thus, in the resulting perfect Bayesian Nash equilibrium, the market is not fooled by the manager's cash flow-boosting behavior, and yet the perceived ability to manipulate the stock price causes the manager of a public firm to make myopic investment decisions. As Stein (1989) notes, the reason is akin to the prisoners' dilemma: If investors assumed no inflation, the manager would inflate current cash flows; and given that investors will, therefore, assume inflation, the manager is better off actually inflating cash flows by cutting investment.

The parameter $\gamma_0 \geq 0$ in equation (1) captures the extent to which investors' inferred level of the firm's current stochastic cash flows, $E_t^I\left[e_t^n\right]$, affects their expectations of the firm's future cash flows, and hence its stock price. If $\gamma_0 = 0$, current cash flows are uninformative about future cash flows, and so investors will ignore current cash flows, removing the manager's short-term incentives.

1.1.1 Optimal Investment

A public-firm manager chooses investment i_t^{Public} that maximizes the following expression:[17]

$$i_t^{Public} = \arg\max_i \left\{ \frac{Qf\left(i\right)}{1+r} - \left(1 + \pi \frac{1+r}{r^2}\gamma_0\right) i \right\}$$

Given the concavity of the maximand, i_t^{Public} is implicitly defined by the following necessary and sufficient first-order condition:

$$\frac{Q}{1+r}\frac{\partial f\left(i_t^{Public}\right)}{\partial\, i} - \left(1 + \pi \frac{1+r}{r^2}\gamma_0\right) = 0 \qquad (2)$$

The manager of a private firm chooses $i_t^{Private}$ to maximize

$$i_t^{Private} = \arg\max_i \left\{ \frac{Qf\left(i\right)}{\left(1+r\right)\left(1+\delta\right)} - i \right\}$$

In this case, the necessary and sufficient first-order condition reads

$$\frac{Q}{\left(1+r\right)\left(1+\delta\right)}\frac{\partial f\left(i_t^{Private}\right)}{\partial\, i} - 1 = 0 \qquad (3)$$

1.1.2 Testable Implications

Solving for the optimal levels of investment allows us to derive the following result:

Result S1: *Assuming public-firm managers behave myopically, the investment level of public*

[17] The objective functions are written out in full in the proof of Result S1, which can be found in Appendix A.

firms is lower than that of private firms with the same investment opportunities if and only if

$$\delta < \pi \frac{1+r}{r^2} \gamma_0 \tag{4}$$

The same condition determines the relative sensitivity to investment opportunities:

Result S2: *Assuming public-firm managers behave myopically, the investment of public firms is less (positively) responsive to improvements in investment opportunities than that of comparable private firms if and only if condition (4) holds.*

The intuition is as follows. The right-hand side of condition (4) captures the marginal agency cost of a stock market listing. This cost arises because the manager has an incentive to boost current cash flows by reducing investment spending. The left-hand side captures the marginal financing benefit of a stock market listing. Listed firms likely face a lower cost of capital because their shares are liquid. Ceteris paribus, when the marginal agency cost exceeds the marginal financing benefit, private firms will invest more and be more responsive to investment opportunities than public firms.[18]

However, this does not imply that going public would be a poor choice for *every* firm. Our final result explores what factors make it more costly for a firm to be public, by exploring under what conditions we should expect the agency cost of being a stock-market listed company to induce a greater distortion in the investment decisions of public firms.

Result S3. *If managers behave myopically, then as* γ_0 *increases, the difference between private and public firms in a) investment levels and b) sensitivity to investment opportunities increases.*

As mentioned earlier, the parameter γ_0 captures the extent to which investors base their expectations of the firm's future cash flows on the inferred level of the firm's current cash flow shock. Firms with high γ_0 are firms for which the permanent component of stochastic cash flows

[18] Setting $\delta = 0$ in condition (4) allows us to compare the investment of a public firm suffering from short-termism with the first-best investment policy. Results S1 and S2 then imply that short-termism induces both sub-optimal investment levels and sub-optimal sensitivity to investment opportunities.

tends to evolve rapidly (that is, $\sigma_u^2 >> \sigma_v^2$), thereby making past cash flow levels less helpful in forecasting future ones. Conversely, in firms with low γ_0, current stochastic cash flows are subject to a lot of transitory noise (that is, $\sigma_v^2 >> \sigma_u^2$), so the whole series of past cash flows is important in forming accurate predictions about future cash flows; see Stein (1989) for details. Therefore, the higher is γ_0, the more informative are current cash flows for forecasting future cash flows, and so the greater is the agency cost induced by the manager's incentive to manipulate investment.

Empirically, for firms in industries with low γ_0, we expect little difference in the sensitivity of investment to investment opportunities between public and private firms. The opposite should be true for firms in industries with high γ_0.

1.2. A Model of Investment Behavior and Empire Building

We follow Stein (2003) and model empire building by assuming that a public-firm manager with empire-building tendencies chooses investment $i_t^{Public,\ Empire}$ that maximizes the following expression:

$$i_t^{Public,\ Empire} = \arg\max_i \left\{ \frac{Qf(i)}{1+r} - i + \frac{B(i)}{1+r} \right\},$$

As in Stein (2003), $B(i) = \theta Qf(i)$, where $0 < \theta < 1$ measures the intensity of the agency conflict. This captures the idea that a public-firm manager derives a private benefit from gross investment output, as in Stulz (1990). Investment $i_t^{Public,\ Empire}$ is then implicitly defined by the following necessary and sufficient first-order condition:

$$\frac{Q}{1+r} \frac{\partial f\left(i_t^{Public,\ Empire}\right)}{\partial\, i} - 1 + \frac{1}{1+r} \frac{\partial B\left(i_t^{Public,\ Empire}\right)}{\partial\, i} = 0 \qquad (5)$$

The remaining features of the model are the same as in our short-termism model, with private-firm investment $i_t^{Private}$ defined in equation (3). Results S1 and S2 change as follows:

Result E1: *Assuming managers are empire-builders, the investment level of public firms is* <u>*higher*</u> *than that of private firms with the same investment opportunities.*

Result E2: *Assuming managers are empire-builders, public-firm investment is more (positively) responsive to improvements in investment opportunities than that of comparable private firms.*

Results E1 and E2 now make the opposite predictions compared to Results S1 and S2. Thus, our model allows us to empirically distinguish between short-termism and empire-building on the basis of differences in sensitivities to investment opportunities and differences in investment levels between public and private firms. Result S3 uniquely applies to short-termism.

2. Sample and Data

Our dataset combines data on public companies obtained from standard sources such as Compustat and CRSP with data on private firms obtained from a new database vendor, Sageworks Inc. Sageworks is similar to Compustat in that it contains accounting data from income statements and balance sheets, except that it exclusively covers private firms. Unlike Compustat, all data are held anonymously so that no individual firm can be identified by name, though basic demographic information such as NAICS industry codes and geographic location is available. One consequence of anonymity is that we cannot observe transitions from private to public status in the Sageworks database. We will later describe how we assemble a dataset of IPO companies from other sources.

Sageworks obtains data directly from private companies' auditors, that is, from audited financial statements or tax returns, rather than from the private companies themselves. The auditors Sageworks co-operates with include most national mid-market accounting firms (those below the 'Big Four') and hundreds of regional players. Sageworks began operations in 2000 but records are patchy before 2002. We have data through 2007, giving a six-year panel with more than 100,000 firm-years. Companies leave the panel mainly through mortality or, potentially, if they switch to an accounting firm that doesn't co-operate with Sageworks.

2.1 Sample Construction

To construct our sample of private companies, we exclude from the Sageworks database all

Canadian companies as well as observations with data quality problems (specifically, those that fail to satisfy basic accounting identities). To be part of the sample of public firms, a company has to be recorded in both the Compustat and CRSP databases during our sample period; be incorporated in the U.S. and listed on a major U.S. exchange (NYSE, NASDAQ, or AMEX); have valid stock prices in CRSP; and have a CRSP share code of 10 or 11 (which screens out non-operating entities such as real estate investment trusts, mutual funds, or closed-end funds).

As is customary, we exclude from both the public and private samples financial firms (SIC 6), regulated utilities (SIC 49), and government entities (SIC 9). In addition, we exclude companies for which we have fewer than two observations with complete data for all the variables used in our analysis; since we estimate panel-data models, such companies do not contribute to the estimation.

Both the public-firm and private-firm samples cover the period from 2002 (the beginning of the Sageworks database) through 2007 (the last year with complete data in the Sageworks database). The public-firm sample consists of 3,926 firms and 19,203 firm-years; the private-firm sample contains 32,207 firms and 88,579 firm-years.

2.2 Matching

Not surprisingly, public companies are substantially larger than private ones. The top graph in Figure 1 shows the distribution of assets in log 2000 dollars for each group of firms. The distributions overlap only to a limited extent. The average (median) public sample firm has total real assets of $1,364.4 million ($246.2 million), compared to $7.1 million ($1.3 million) for private firms.

Much of our empirical analysis therefore uses a matched dataset designed to identify large private companies and small public companies (which are much more comparable in size) and to balance the sample's industry distribution. Our matching procedure is thus a variant of nearest-neighbor matching used in the program evaluation literature, surveyed in Imbens and Wooldridge (2009). The matched dataset is essentially drawn from the region where the two size distributions shown in Figure 1 overlap. It is constructed as follows. Starting in 2002, for each public firm, we identify the private firm in the same four-digit NAICS industry (equivalent to three-digit SIC) and fiscal year closest in terms of total assets (*TA*), such that $\max(TA_{public}, TA_{private}) / \min(TA_{public}, TA_{private}) < 2$. If no match can be found in a given fiscal year, the observation is discarded and a new match is attempted for that company in the following year. Once a match is formed, it is kept intact for as long as both the public and private firms remain in our sample, to maximize the available time series for each firm. If a matching firm exits the panel, a new match is spliced in.

The matched sample contains 4,975 public-firm-years and an equal number of private-firm-years. Because we match with replacement, to maximize the match rate, the matched sample contains 1,666 public firms and 620 private firms. Our results are not sensitive to matching without replacement. Standard errors are appropriately clustered to account for the resampling of private firms.[19]

The matched sample is much more balanced in terms of firm size. The bottom graph in Figure 1 shows the distribution of log real assets for public and private firms in the matched sample. The overlap is near perfect. The means are $144.7 million and $120.0 million for public and private firms, respectively, and the difference between them is not statistically significant at the 5% level.

2.3 Measures of Investment Opportunities

The empirical investment literature controls for a firm's investment opportunities using either Tobin's *Q* or sales growth. *Q* is usually constructed as the ratio of the firm's market value to the book value of its assets but since private firms are not traded, their market value is not observed. We therefore favor sales growth, which can be constructed at the firm level for any firm, whether public or private. Sales growth is measured as the annual percentage increase in sales (Compustat item *sale* or its Sageworks equivalent) and has been widely used as a measure of investment opportunities in both economics and finance. See for example Rozeff (1982), Lehn and Poulsen (1989), Martin (1996), Shin and Stulz (1998), Whited (2006), and Acharya, Almeida, and Campello (2007).

For robustness purposes, we also explore two *Q* measures. To get around the lack of market

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[19] It is not possible to account for the variance introduced by the matching procedure. Abadie and Imbens (2008) show that bootstrapping is not valid in the nearest-neighbor setting. We are unaware of any alternative correction to the covariance matrix for the exact matching procedure and panel data models we need to adopt. This is a common problem in this kind of setting.

values for private firms, Q has to be imputed. Our first measure, which we call predicted Q, is constructed following Campello and Graham (2007). First, we regress each public firm's Q (Compustat items *prcc_f×cshpri + pstkl + dltt + dlc – txditc* divided by beginning-of-year total assets) on four variables that have been shown to be informative about a firm's marginal product of capital, namely sales growth, ROA, net income before extraordinary items, and book leverage. We also include year and industry fixed effects (using three-digit NAICS industries). We then use the regression coefficients to generate predicted Q for each public and each private firm.

Our second Q measure follows Cummins, Hassett, and Oliner (2006). Pointing to bubbles, Cummins et al. propose constructing an estimate of a firm's intrinsic value not from market values but from the earnings forecasts that financial analysts periodically publish. We call this the analysts Q measure and construct it using earnings forecasts obtained from Thomson Financial's I/B/E/S database. Because analysts do not cover private firms, we impute private-firm Qs using the median value of analysts Q in the same four-digit NAICS industry, and for comparability we do the same for public firms. Analysts Q is particularly troublesome: It can be estimated directly only for public firms that are covered by one or more financial analysts, raising selection concerns,[20] and the fact that we are using medians means we are using 'a proxy for a proxy.' The net effect of this measurement error problem is to dampen any effect that analysts Q has on investment decisions.

2.4 Summary Statistics

Table 1 reports summary statistics for the full samples of public and private firms (denoted 'F') and for the matched sample (denoted 'M'). The last four columns in each panel report pairwise differences in means or medians between the relevant samples. Gross investment is the annual increase in gross fixed assets (Compustat data item *ppegt* or its Sageworks equivalent) scaled by beginning-of-year total assets (Compustat item *at* or its Sageworks equivalent). Net investment is defined analogously using net fixed assets (Compustat item *ppent* or its Sageworks equivalent). The

[20] See Carpenter and Guariglia (2007) for a critique of Cummins et al.'s (2006) approach and findings.

difference between the two is depreciation. To the extent that depreciation represents assets that need replacing due to wear and tear or obsolescence, gross investment better captures the company's investment spending decisions.[21]

On average, private companies invest significantly *more* than public companies using either measure, consistent with Result S1 and contrary to Result E1. In the full samples, private firms increase their gross and net fixed assets by an average of 7.6% and 3.3% of total assets a year, compared to 4.5% and 2.2% among public companies. In the matched sample, the average differences are 5.6 and 7.2 percentage points in favor of private companies, respectively. The median differences are smaller, at -0.1 and 0.9 percentage points in favor of private companies, largely because neither the median public company nor the median private firm invests very much.

Public and private firms also differ systematically in their profitability, cash holdings, and use of debt. Private firms have significantly higher return on assets (ROA), defined as operating income before depreciation scaled by beginning-of-year total assets. In the matched sample, average ROA amounts to 0.084 for private firms versus -0.06 for public firms (median: 0.123 versus 0.051). At the same time, private firms hold significantly lower cash balances (beginning-of-year cash and short-term investments) and have significantly higher leverage (beginning-of-year long-term and short-term liabilities divided by beginning-of-year total assets). A greater reliance on borrowing is not surprising considering that, by definition, private firms have no access to the stock market and so face a higher cost of raising equity capital.

Finally, note that matched-sample firms grow significantly faster than full-sample firms. For public firms, annual sales growth averages 18.3% in the full sample and 25.6% in the matched sample. For private firms, the corresponding numbers are 17.7% and 32.7%. This suggests that our empirical focus on small public and large private companies identifies fast-growing entrepreneurial firms for which, arguably, making optimal investment decisions is particularly important.

[21] Occasionally, studies of investment model annual capital expenditures (CAPEX). CAPEX appears on a firm's statement of cash flows which is unfortunately not available in Sageworks. Therefore, we confine our attention to modeling changes in gross or net fixed assets, to ensure we use the same investment measure for public and private firms.

3. How Do Public and Private Firms Respond to Investment Opportunities?

3.1 Baseline Models

Neoclassical models of corporate investment predict that investment spending is a function of investment opportunities. In Table 2, we estimate standard investment regressions of gross investment on investment opportunities.[22] A long line of literature shows that standard proxies for investment opportunities are not, as neoclassical theory predicts, a sufficient statistic for investment, and that changes in net worth, measured as ROA, have a positive effect on investment spending. A significant ROA effect is often interpreted as a sign of financing constraints (Fazzari, Hubbard, and Petersen (1988)), though some disagree (see Kaplan and Zingales (1997, 2000), Cleary (1999), Erickson and Whited (2000), or Alti (2003)). While we follow the literature by controlling for ROA, we are agnostic about the debate surrounding its interpretation. We remove unobserved time-invariant heterogeneity using firm fixed effects and also include (but do not report) year effects.

The results in column 1 suggest that public firms' investment decisions are significantly *less* sensitive to changes in investment opportunities as measured by sales growth. The coefficient estimated for sales growth is 0.136 for private firms and 0.136 – 0.097 = 0.039 for public firms, and the difference between these estimates is statistically significant at the 1% level. This supports Result S2 and runs counter to Result E2. Thus, the data favor short-termism over empire-building. We also find that investment is sensitive to ROA and that this sensitivity is significantly lower among public firms, consistent with the interpretation that public firms are less financially constrained.

The specification in column 1 cannot accommodate a public-firm indicator in levels (as opposed to one interacted with sales growth and ROA). The reason is that public and private status is fixed in the sample because we cannot observe transitions from one to the other using the Sageworks data. In columns 2 and 3, we estimate the investment model separately for public and private firms. The point estimates continue to suggest that private firms' investment is more sensitive to investment

[22] As we will show later, we obtain similar results using net investment instead.

opportunities than is that of public firms, and the magnitudes mirror those found for the matched sample used in column 1. Moreover, the R^2 is considerably higher for private firms (42.5%) than for public ones (5.5%), suggesting, interestingly, that private firms' investment behavior is better explained by variation in investment opportunities (and in ROA) than that of public firms.

These findings are robust to using our two measures of *Q* to proxy for investment opportunities; see columns 4 and 5. In light of this robustness, and because sales growth is the methodologically soundest proxy in our setting, we report results using only sales growth in the remainder of the paper.

Table 3 reports alternative specifications. Columns 1 to 6 add other controls sometimes used in empirical investment models. Column 1 adds cash holdings and column 2 interacts cash holdings with public status. Columns 3 and 4 control for leverage instead of cash holdings, while columns 5 and 6 control for size with or without a leverage control. In each of these specifications, the variable of interest – the interaction of public status and investment opportunities – remains negative and statistically significant, with similar point estimates as before, ranging from -0.099 to -0.056. This suggests that the greater sensitivity of investment to investment opportunities we found among private firms is not driven by differences in cash holdings, borrowing, or firm size. Nor does it depend on modeling gross rather than net investment, as column 7 shows.

The results presented so far use the size-and-industry matched sample. While size is a reasonable matching criterion, it is not the only plausible one. In column 8, we use an alternative sample matched on sales growth (*SG*) and industry. Similar to before, we retain only matches that satisfy $\max(SG_{public}, SG_{private}) / \min(SG_{public}, SG_{private}) < 2$. In column 9, we estimate the investment equation in the universe of public Compustat and private Sageworks firms, requiring only that firms be based in the U.S. and excluding financial firms (SIC 6), regulated utilities (SIC 49), and government entities (SIC 9). Both specifications continue to show significantly lower sensitivity to investment opportunities among public firms than among private ones. This suggests that our core empirical result is unlikely to be an artifact of our matching choices.

3.2 Measurement Error

Investment equations such as those reported in Table 2 are subject to measurement error. The problem arises because investment opportunities is a latent variable that must be proxied for. The literature proposes three ways to correct for the resulting attenuation bias. First, Erickson and Whited (2000) derive a GMM estimator that relies on higher-order moments, particularly skewness, of the latent variable. Second, Arellano and Bond (1991) provide a GMM estimator in first-differences for which lagged mismeasured regressors are potentially valid instruments under mild assumptions about serial correlation in the latent variable and the innovations of the model. Third, an exogenous shock to investment opportunities can help identify the effect on investment. Rauh (2006), for example, exploits exogenous variation in companies' pension contributions as an instrument.

Almeida, Campello, and Galvao (2010) evaluate the first two estimators using Monte Carlo simulations. They find that the Arellano-Bond estimator is well-behaved whereas the higher-order one is not. Thus, we focus on the former.[23] In the next section, we explore a quasi experiment.

The results are reported in Table 4. For ease of comparison, column 1 reproduces the baseline within-groups estimates from column 1 in Table 2. In columns 2 and 3, we estimate a static Arellano-Bond model, using investment and sales growth dated t-5 to t-3 and year effects as instruments.[24] The specification in column 4 is dynamic and so includes first lags of all variables; however, for brevity, we only report the coefficient for lagged investment. In the dynamic case, only variables dated t-5 and t-4 can be used as instruments, which greatly affects identification as our panel is relatively short. Columns 5 and 6 show results from system GMM which jointly estimates a first-differenced equation as in columns 2 and 3 (instrumented with lagged variables in levels) and an equation in levels instrumented with lagged differences (see Blundell and Bond (1998)). This allows us to include a dummy for public companies as a control. Each specification includes year effects.

Across columns 2 to 6, the variable of interest – the interaction of public status and investment

[23] Our data reject the distributional assumptions of the Erickson and Whited (2000) estimator (results not reported).
[24] Variables dated t-2 are mechanically correlated with lagged sales growth and so cannot be included in the instrument set.

opportunities – is always negative, mirroring our earlier findings. The point estimate is generally around -0.2, which is about twice as large as in the panel regressions reported in Tables 2 and 3, consistent with a measurement error-induced attenuation bias. The exception is the dynamic GMM model in column 4. This model appears to be misspecified, probably because of the paucity of suitably lagged instruments in our short panel. Significance varies depending on the specification. The interaction term is statistically significant in columns 3, 5, and 6. Each specification passes the standard specification tests, i.e., the Hansen test of over-identification restrictions and a test for the absence of third-order serial correlation in first differences.

3.3 Quasi Experiment: State Corporate Tax Changes

As an alternative to the GMM approach, we use changes in state corporate income taxes as a plausibly exogenous shock to investment opportunities. A cut in a state's corporate income tax rate reduces the cost of capital for firms operating in that state, which should have a positive effect on investment, and vice versa for tax increases. If private firms' investment decisions are more sensitive to investment opportunities, we expect private firms to be more sensitive to changes in state corporate income taxes, increasing investment when taxes are cut and decreasing it when they are raised. We test this prediction using a difference-in-difference approach by interacting public status with an indicator variable set equal to 1 (-1) for companies headquartered in a state that passed a tax cut (tax increase) that became effective during the fiscal year in question, and zero otherwise.[25,26]

We focus on tax changes that can unequivocally be categorized as either an increase or a cut.[27] Using data from the Tax Foundation, we identify eight tax cuts and two tax increases in a total of eight states (IN, MI, ND, NJ, NY, TN, VT, and WV); see Appendix B for details. For example, ND

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[25] We impose symmetry for parsimony. We obtain similar results when we instead use separate indicators for tax cuts and tax increases, and the data cannot reject the hypothesis that the effects are indeed symmetric.

[26] If we use tax changes as an instrument for sales growth instead, we obtain results that go in the same direction as the diff-in-diff test, but these are noisy, for two likely reasons. First, relatively few sample firms are affected by state corporate income tax changes in our sample period. Second, for sales growth to increase, a company must first increase investment which in turn takes some time to become productive (Wurgler (2000)).

[27] Some states introduced tax changes whose net effect on firms is unclear. For instance, in 2006 OH started phasing in a gross receipts tax while phasing out the corporate income tax. Similarly, TX replaced a 4.5% corporate income tax with a 1% tax on gross receipts in 2007. We do not use these tax changes in our analysis.

decreased its corporate income tax rate from 10.5% to 7% beginning in the 2004 fiscal year.

For the purpose of this test, we exclude S Corps, LLCs, and partnerships from the private-firm sample as only C Corps are subject to the same state corporate income tax regime as public companies. This reduces the number of private-firm-years from 88,579 to 33,078. In total, 355 public and 354 private sample firms are affected by a tax cut, while 206 public sample firms and 179 private ones are affected by a tax increase. Unfortunately, we cannot use the industry-and-size-matched sample for this test because there are only five ‘treated’ private firms in our matched sample.

Table 5 reports the results. As in Tables 2 and 3, we estimate fixed-effects least-squares regressions. Column 1 shows that private firms – but not public ones – significantly increase their investment spending in response to tax cuts and lower it in response to tax increases. The point estimates are economically meaningful. For example, all else equal, private firms on average increase their investment by 2.3 percentage points of the capital stock when their home state cuts corporate income tax. The unconditional average of gross investment among private C Corps in our sample is 5.8% of the capital stock, implying a 39.7% increase in investment spending (0.023/0.058). For public firms, the effect of a tax change is essentially zero (0.023–0.026 = –0.003). This finding is not sensitive to whether or not we also include sales growth in the model (see column 2).

Columns 3 and 4 investigate possible pre- and post-trends in the tax change effect by adding indicators identifying firms in states that will undergo a tax change in one (t-1) or two (t-2) years or that underwent a tax change one (t+1) or two (t+2) years ago.[28] None of the indicators is statistically significant at the 5% level, suggesting that firms a) do not anticipate future tax changes in their investments and b) adjust their investment spending as soon as a tax change comes into effect.

Column 5 reports a validity test of our identification strategy. Since only C Corps are affected by state corporate income tax changes, we should not find any tax effect on the investment behavior of private *non*-C Corps. This is precisely what we find: The coefficient estimate for the tax change

[28] Our relatively short panel prevents us from including further leads and lags.

indicator in the non-C Corp sample is 0.001 with a standard error of 0.009.

A possible confound in Table 5 is due to size. If larger firms are more likely to have operations in multiple states, their investment decisions will be less sensitive to a tax change in their home state. The reason is that state taxes are paid in each state a firm operates in, according to the share of business done in that state (e.g., a firm headquartered in VT with a plant in ME will pay taxes in ME for the income generated by the ME plant). This could explain the absence of sensitivity to tax changes among public firms. Fixing this confound is not straightforward. We cannot control for the effect directly as data on the geographic breakdown of public firms' operations is generally unavailable. And as we noted earlier, we have too few treated firms to perform our tests in the size-and-industry-matched sample, so the public firms used here are much larger than the private ones.

To alleviate this concern somewhat, we limit the sample of public firms to the smallest quartile by total real assets (specifically, those with assets below $65.4 million). Total real assets average $31.3 million in the bottom quartile, which is much more comparable to the private C Corp average of $12.9 million. The resulting sample contains 97 public firms affected by a tax decrease and 65 affected by a tax increase (and the same number of treated private firms as before). As columns 6 and 7 show, restricting the sample in this way has virtually no effect on the point estimates for the tax change variables. (Adding pre- and post-trends, not tabulated, again leaves the results unchanged.)

3.4 Within-firm Changes in Investment Behavior Around IPOs

So far, our tests have compared the behavior of public and private firms. While we are the first to have access to comprehensive financial data on a large sample of private companies in the U.S., we cannot rule out that private and public companies differ along unobserved dimensions that in turn correlate with their investment behavior. This is true of any matching algorithm since matching can only be done on observables. To conclusively rule out possible biases stemming from unobserved heterogeneity would require a randomized trial. However, it is clearly infeasible to randomly assign firms to a stock-market 'treatment' group and a 'control' group of unlisted firms.

An alternative research design is to examine how a *given* firm's investment behavior changes as it transitions from private to public status or vice versa. This would enable us to remove unobserved time-invariant heterogeneity using firm fixed effects, but of course firms go public (or private) for a reason, and that reason could correlate with their investment behavior – most obviously a desire to fund a planned increase in investment.[29] Going public is hence not a natural experiment.

Instead, we focus on a subset of the population of firms that go public, namely those that do so *without raising capital*. It seems reasonable to assume that these firms went public for reasons other than to fund investment, which reduces – but does not eliminate – identification concerns. We hence offer the following evidence in the spirit of a reality check on our large-sample findings.

Our IPO dataset consists of all 90 non-financial and non-utility firms that went public between 1990 and 2007 for the sole purpose of allowing existing shareholders to cash out, as opposed to raising equity to fund the company's operations, investment plans, or to repay debt. Suitable IPOs are identified from Thomson Reuters' SDC database. Appendix C lists their names, dates, and circumstances. We collect post-IPO accounting data from Compustat and hand-collect pre-IPO accounting data from the sample firms' IPO prospectuses and 10-K filings available in the SEC-Edgar and Thomson Research databases. Since this sample doesn't involve Sageworks data, we can collect data on capital expenditures (CAPEX) and spending on R&D from the cash flow statements. On average, we have 4.4 pre-IPO years of accounting data.

Table 6 provides descriptive statistics for the 90 firms, separately for the pre-IPO and post-IPO periods, as well as for a control sample of public companies matched on three-digit SIC code and total assets, similar to before. The most notable change from pre- to post-IPO is that firms increase their average and median cash holdings. By sample construction, this does not reflect an inflow of cash from the IPO – there isn't one – but instead a deliberate attempt to increase cash holdings post-IPO through a variety of means, including greater borrowing. Compared to other public companies in

[29] See the survey evidence reported in Brau and Fawcett (2006) for reasons why firms go public.

our control sample, mean and median cash holdings are significantly lower pre-IPO and indistinguishable post-IPO. Finally, the level of investment spending declines marginally post-IPO.

Table 7 estimates whether the sensitivity of investment to investment opportunities changes around the IPO, within a given firm. We measure investment either as CAPEX or the sum of CAPEX and R&D. As before, we use sales growth to measure investment opportunities since it is the only measure available for the pre-IPO observations.

Columns 1 and 2 report own-difference results for the IPO sample, where we interact the investment opportunities and ROA variables with an indicator variable that equals one if an observation is post-IPO. Consistent with the descriptive statistics shown in Table 6, we find no difference in average investment levels, but we do find significant differences in the time profile: Whether we focus on CAPEX or CAPEX plus R&D, investment is significantly sensitive to sales growth before a company goes public and then becomes significantly less sensitive after. These findings suggest that companies alter their investment behavior once they are public, even though they demonstrably went public for reasons other than to fund investment. This finding is consistent with the large-sample evidence reported in our earlier tables.

It is possible that investment sensitivities changed for reasons unrelated to the IPO itself. To shed light on this possibility, columns 3 and 4 report difference-in-difference results based on combining data from the IPO sample with data for matched public controls. While we cannot rule out that treated and control firms differ in unobserved ways, the results continue to tell the same story: Investment is sensitive to changes in investment opportunities; before they go public, IPO firms are significantly more sensitive to investment opportunities; and once they are public, their investment sensitivity is indistinguishable from that of other public companies matched on industry and size.

3.5 Discussion

The results of the four separate identification strategies reported in this section – within-firm, Arellano-Bond, IV, and the IPO approach – all paint the same picture: On average, stock market-listed firms make investment decisions that are less sensitive to changes in investment opportunities, consistent with Result S2 and contrary to Result E2. Thus, public firms exhibit myopic investment behavior rather than engaging in empire-building, at least in our sample.

4. Cross-sectional Variation in Myopic Investment Behavior

According to Result S3, public-firm managers have an incentive to make myopic investment decisions to boost current cash flows (and thus their stock price) only to the extent that their stock price is in fact sensitive to current cash flows, that is, if $\gamma_0 > 0$. This suggests cross-sectional variation in myopic investment incentives among public companies. To test for this, we follow the accounting literature and use the earnings response coefficient (ERC) to capture a firm's stock price sensitivity to earnings (see Ball and Brown (1968) and Beaver (1968) for seminal contributions). If ERC is a good proxy for γ_0, Result S3 predicts that a triple interaction of investment opportunities, public status, and ERC should be significantly negative. In other words, we expect that the difference in investment sensitivity between private and public companies should increase in ERC.

Since our sample contains unlisted companies, we implement this test using ERC estimates that are estimated at the industry-year level. Specifically, each year between 2001 and 2006, we regress one plus a firm's stock return during the fiscal year on a constant and the firm's earnings per share (EPS) in the full sample of public companies, allowing the slope coefficient to vary by industry (see Dechow, Hutton, and Sloan (1999) for a similar approach). We use the Fama and French (1997) classification of 30 industry groups, available from Kenneth French's webpage.[30] Results are robust to using the Fama-French 38 or 49 industries instead. The slope coefficients (one for each industry and year) are our estimates of each industry's ERC. We use these estimates to form the required interaction terms for inclusion in the investment regressions.

Panel A of Table 8 reports the triple-difference estimation results using our matched sample of private and public firms, as in Tables 2 to 4. As in those tables, we exploit within-firm variation by

[30] Private firms are grouped into the 30 Fama-French industries based on their NAICS codes, which we map to SIC codes using the U.S. Census Bureau's NAICS-SIC bridge, available at http://www.census.gov/epcd/naics02/index.html.

including firm fixed effects and include (but do not report) year effects. The triple interaction is negative and significant, as Result S3 predicts, suggesting that the difference in investment sensitivity between private and public firms documented in our previous tables is indeed driven by public firms whose stock prices are highly sensitive to earnings announcements.

Panel B shows the implied investment sensitivity to investment opportunities, as estimated in Panel A, for private and public firms at the 25th and 75th percentile of the ERC distribution within the matched sample. In low-ERC industries, the difference in investment sensitivity between public and private firms is small and statistically insignificant. In other words, in industries where changes in current earnings have relatively little effect on the stock price, managers of public companies are similarly responsive to changes in investment opportunities as their private-company counterparts. In high-ERC industries, on the other hand, the difference is large and highly significant. These patterns are consistent with Result S3.

Panel B suggests a possible confound. We would have expected the investment sensitivity to increase in ERC for public firms but not for private firms. While the difference between the two behaves exactly as predicted, the levels do not: The investment sensitivity among public firms increases only marginally as we increase ERC from the 25th to the 75th percentile, while that of private firms more than doubles. This suggests that ERC itself captures something that correlates with investment sensitivity. For example, high-ERC industries might be industries with good investment opportunities that are not fully captured by our proxy for investment opportunities.[31] Absent short-termism, we would then expect both public and private companies to exhibit greater investment sensitivities as ERC increases, and the fact that only private companies do so is, in fact, consistent with short-termism. Regardless of what the confound is, we stress that it can be differenced away using the triple-difference structure. The fact that the difference in investment sensitivities between private and public firms increases in ERC therefore supports Result S3.

[31] This seems quite likely. High-ERC industries are industries with high stock price-earnings ratios (Kothari 2001, p. 141). Price-earnings ratios in turn likely correlate positively with growth opportunities.

5. Auxiliary Evidence: Income Smoothing and Dividend Policy

Our data favor models of managerial myopia. A common feature of such models is that earnings and/or dividends are smoother than they would be if managers didn't try to manipulate investors' cash flow expectations. According to Stein (1989), "If [the manager] is overly concerned about current performance, he may [engage in myopic activities] so as to smooth profits over time" (p. 658).[32] Alternatively, if the market uses today's dividend to form its expectations of future profits, the manager may sacrifice investment to keep the dividend and thereby signal that the company's profitability remains sound, as in Miller and Rock (1985).[33] In this section, we test the conjecture that if short-termism is a feature of stock markets, public firms will have smoother profit growth and/or smoother dividend payout policies than do private ones.

We measure smoothness of profit growth as the within-firm time-series standard deviation of the real annual growth in either a) net income before extraordinary items or b) operating income after depreciation. Note that the unit of observation in this test is a firm rather than a firm-year. Similarly, we use the within-firm time-series standard deviation of the payouts paid by each firm to its shareholders to measure the smoothness of dividend policy.

The results, using our matched sample, are reported in Table 9. The covariate of interest is an indicator variable set equal to one for public companies. We control for firm size since, all else equal, larger firms have more volatile profit growth and dividend payout levels. In the two profit growth regressions, we also control for whether a firm has had negative income during its time in our sample, in order to account for the fact that the income of such a firm might be more volatile. In the dividend payout regression, we control for whether the firm does not pay dividends during its time in our sample, in order to account for the fact that such a firm will have smooth payouts by construction.

In all three regressions, we find that the public status indicator has a negative and statistically

[32] This differs from earnings management through accounting 'tricks' that have no effect on the firm's cash flows.
[33] A third mechanism, suggested by Graham, Harvey, and Rajgopal's (2005) survey evidence quoted in the introduction, is that managers wish to avoid their reported earnings falling short of the consensus forecasts made by financial analysts. However, since financial analysts make no earnings forecasts for private firms, we cannot pursue this mechanism.

significant coefficient, as predicted. The graphs in Figure 2 illustrate this by plotting kernel density estimates of the profit-growth and dividend-policy regression residuals separately for public and private firms. In each graph, much of the private-firm density lies to the right of the corresponding public-firm density. Thus, public firms appear to have both smoother profit growth and smoother dividend payout policies compared to private ones, as implied by models of short-termism.

Our final auxiliary test asks whether short-termism induces public firms to make sub-optimal investment decisions in an effort to avoid reporting accounting losses, as Baber, Fairfield, and Haggard (1991) claim. If so, we expect a greater fraction of public firms than of private firms to report earnings just above zero. We measure earnings as net income scaled by total assets and focus on two intervals around zero, namely (-0.10, 0.10) and (-0.05, 0.05). The results, reported in Panels A and B of Table 10, indicate that public firms are more likely to report small positive earnings than are private firms, and the differences are both economically and statistically significant. Panel C reports placebo tests, which test for differences in the fractions of public and private firms reporting earnings above six arbitrary thresholds away from zero, namely -0.3, -0.2, -0.1, 0.1, 0.2, and 0.3. Interestingly, at each of the placebo points in the earnings distribution, a significantly *smaller* fraction of public firms report earnings above the threshold compared to private firms – contrary to what happens at the zero earnings threshold. This is consistent with public firms taking measures to avoid reporting negative earnings.

6. Conclusions

Our aim in this paper is to examine whether the stock market harms investment incentives. The theory literature in economics and finance has long argued that the separation of ownership and control following a stock market listing can lead to agency problems between managers and dispersed stock market investors and hence to suboptimal investment decisions. The literature is divided on whether overinvestment (i.e., empire building) or underinvestment (due to rational short-termism) will result, or indeed whether effective corporate governance mechanisms can be devised to ensure investment does not suffer (Tirole (2001), Shleifer and Vishny (1997)).

We embed Stein's (1989) short-termism problem and the empire-building problem of Baumol (1959), Stulz (1990), and Stein (2003) in a nested model to derive testable predictions that allow us to empirically distinguish between the two. In order to test the model, we need a proxy for "optimal" investment decisions, that is, for the investment decisions managers would have made absent agency problems. We obtain such a proxy from a rich new data source on private (i.e., unlisted) U.S. firms provided by Sageworks Inc. Our maintained hypothesis is that the agency problems that public firms are subject to are less prevalent, or even absent, among private ones, which, however, face a higher cost of capital.

Matching Sageworks to standard Compustat data on stock-market listed companies by size and industry, we identify matched panels of (small) public and (large) private firms and then estimate standard investment equations. Using several distinct sources of identification, our results show that compared to private firms, public companies invest both less and in a manner that is significantly less responsive to changes in investment opportunities, especially in industries in which stock prices are particularly sensitive to current profits. These findings are consistent with short-termism and contrary to what one would expect if empire-building were the dominant agency problem in the stock market.

We also show that public companies tend to smooth their earnings growth and dividends and are reluctant to report negative earnings. One interpretation for these patterns is that public firms treat investment spending as the residual after having paid dividends out of their cash flows, whereas private firms treat dividends as the residual after paying for their investment out of cash flows.

Overall, at least for our fast-growing sample firms, the benefit of cheaper funding via the stock market appears to be outweighed by the distortions that short-termism induces in the investment behavior of public firms, due to the agency costs associated with separation of ownership and control.

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Appendix A.

A.1 Proof of Result S1 (Short-termism)

For private firms, the objective function, in terms of current investment *i*, can be written as

$$U_t^{Private}(i) = Qf(i_{t-1}) - i + \frac{Qf(i)}{(1+r)(1+\delta)} + \frac{1}{(1+r)(1+\delta)} E_t^M\left[(1+r)(1+\delta)e_t^n - i_{t+1} + e_{t+1}^n\right] + C^{Private}.$$

For public firms, objective function is

$$U_t^{Public}(i) = Qf(i_{t-1}) - i + \frac{Qf(i)}{1+r} + \frac{1}{1+r} E_t^M\left[(1+r)e_t^n - i_{t+1} + e_{t+1}^n\right] + C^{Public} +$$
$$+\pi\frac{1+r}{r^2}\left(Qf(\bar{i}) - \bar{i} + \gamma_0\left(-i + \bar{i} + E_t^M\left[e_t^n\right]\right) + \sum_{k=1}^{\infty}\gamma_k e_{t-k}^n\right)$$

The *C* terms collect future cash flows that do not contain any terms that are a function of current investment. The first-order conditions characterizing the optimal levels of investment for private and public firms are easily derived.

Then, given our functional form assumption $f(i) = i^\alpha$, with $0 < \alpha < 1$, we can explicitly solve for the optimal level of investment for private firms:

$$i_t^{Private} = \left(\frac{1}{1+\delta}\right)^{1/(1-\alpha)}\left(\frac{Q}{1+r}\alpha\right)^{1/(1-\alpha)}.$$

Analogously, for public firms we have

$$i_t^{Public} = \left(\frac{1}{1+\pi\frac{1+r}{r^2}\gamma_0}\right)^{1/(1-\alpha)}\left(\frac{Q}{1+r}\alpha\right)^{1/(1-\alpha)}$$

Therefore, we have the following:

$$i_t^{Private} - i_t^{Public} = \left(\left(\frac{1}{1+\delta}\right)^{1/(1-\alpha)} - \left(\frac{1}{1+\pi\frac{1+r}{r^2}\gamma_0}\right)^{1/(1-\alpha)}\right)\left(\frac{Q}{1+r}\alpha\right)^{1/(1-\alpha)} > 0 \Leftrightarrow \delta < \pi\frac{1+r}{r^2}\gamma_0$$

A.2 Proof of Result S2 (Short-termism)

Differentiating the expressions for the optimal investment level, we have that

$$\frac{\partial\, i_t^{Private}}{\partial Q} = \left(\frac{1}{1+\delta}\right)^{1/(1-\alpha)} \left(\frac{\alpha}{1+r}\right)^{1/(1-\alpha)} \frac{1}{(1-\alpha)} (Q)^{1/(1-\alpha)-1}$$

and

$$\frac{\partial\, i_t^{Public}}{\partial Q} = \left(\frac{1}{1+\pi\frac{1+r}{r^2}\gamma_0}\right)^{1/(1-\alpha)} \left(\frac{\alpha}{1+r}\right)^{1/(1-\alpha)} \frac{1}{(1-\alpha)} (Q)^{1/(1-\alpha)-1}$$

Therefore, we have that:

$$\frac{\partial\, i_t^{Private}}{\partial Q} - \frac{\partial\, i_t^{Public}}{\partial Q} = \left(\left(\frac{1}{1+\delta}\right)^{1/(1-\alpha)} - \left(\frac{1}{1+\pi\frac{1+r}{r^2}\gamma_0}\right)^{1/(1-\alpha)}\right) \left(\frac{\alpha}{1+r}\right)^{1/(1-\alpha)} \frac{1}{(1-\alpha)} (Q)^{1/(1-\alpha)-1} > 0 \Leftrightarrow$$

$$\Leftrightarrow \delta < \pi\frac{1+r}{r^2}\gamma_0$$

A.3 Proof of Result S3 (Short-termism)

Result S3 follows directly from the fact that the expression $\left(\left(\frac{1}{1+\delta}\right)^{1/(1-\alpha)} - \left(\frac{1}{1+\pi\frac{1+r}{r^2}\gamma_0}\right)^{1/(1-\alpha)}\right)$

is increasing in γ_0, and therefore both $i_t^{Private} - i_t^{Public}$ and $\frac{\partial\, i_t^{Private}}{\partial Q} - \frac{\partial\, i_t^{Public}}{\partial Q}$ are increasing in γ_0.

A.4 Proof of Result E1 (Empire building)

For a public firm with an empire builder manager, the objective function can be written as

$$U_t^{Public,\,Empire}(i) = Qf(i_{t-1}) - i + \frac{Qf(i)}{1+r} + \frac{1}{1+r} E_t^M\left[(1+r)e_t^n - i_{t+1} + e_{t+1}^n\right] + C_1 + \\ + B(i_{t-1}) + \frac{1}{1+r} B(i) + C_2 ,$$

where C_1 collects future cash flows that do not contain any terms that are a function of current investment and C_2 collects future private benefits that are independent of current investment. The first-order condition (5) is easily derived.

Then, given that $B(i) = \theta Q f(i)$, with $0 < \theta < 1$, and $f(i) = i^{\alpha}$, with $0 < \alpha < 1$, we can explicitly solve for the optimal level of investment for public firms under the empire-building assumption:

$$i_t^{Public,\,Empire} = (1+\theta)^{1/(1-\alpha)} \left(\frac{Q}{1+r} \alpha \right)^{1/(1-\alpha)}.$$

The optimal investment for private firms remains unchanged. Hence we have that

$$i_t^{Private} - i_t^{Public,\,Empire} = \left(\left(\frac{1}{1+\delta} \right)^{1/(1-\alpha)} - (1+\theta)^{1/(1-\alpha)} \right) \left(\frac{Q}{1+r} \alpha \right)^{1/(1-\alpha)} < 0.$$

A.5 Proof of Result E2 (Empire building)

Given that

$$\frac{\partial\, i_t^{Public,\,Empire}}{\partial Q} = (1+\theta)^{1/(1-\alpha)} \left(\frac{\alpha}{1+r} \right)^{1/(1-\alpha)} \frac{1}{1-\alpha} (Q)^{1/(1-\alpha)-1},$$

it follows that

$$\frac{\partial\, i_t^{Private}}{\partial Q} - \frac{\partial\, i_t^{Public,\,Empire}}{\partial Q} = \left(\left(\frac{1}{1+\delta} \right)^{1/(1-\alpha)} - (1+\theta)^{1/(1-\alpha)} \right) \left(\frac{\alpha}{1+r} \right)^{1/(1-\alpha)} \frac{1}{(1-\alpha)} (Q)^{1/(1-\alpha)-1} < 0.$$

Appendix B. List of state corporate income tax changes

This table lists the state corporate income tax changes that we use for the analysis in Table 5. We limit our attention to state corporate income tax changes that occurred during our sample period (2002-2007) and that we can unequivocally categorize as either a tax increase or a tax decrease. In particular, we exclude changes such as the one that occurred in Texas in 2007, where a 4.5% tax on net taxable earned surplus was replaced by a 1% gross receipts tax, given that we cannot determine the net effect on investment incentives of this change. In states with more than one tax bracket, we report the change to the top bracket; lower tax brackets were also affected. We use data from the Tax Foundation available at http://www.taxfoundation.org/taxdata/show/230.html to identify these changes, and verify the information using the relevant tax forms from each state. The Indiana fiscal impact statement can be found at http://www.agecon.purdue.edu/crd/Localgov/Second%20Level%20pages/LSA_fiscal_note_HB1001ss.pdf.

State	Year	Type of tax change	Brief description of tax change
IN	2004	Decrease	Corporate tax rate increased while the gross income tax and the supplemental net income tax were repealed. The overall effect was a tax decrease, according to the fiscal impact statement of the bill prepared by the Indiana Legislative Services Agency, Office of Fiscal and Management Analysis.
MI	2002	Decrease	Corporate income tax rate falls from 2% to 1.9%
ND	2004	Decrease	Corporate income tax rate falls from 10.5% to 7%
ND	2007	Decrease	Corporate income tax rate falls from 7% to 6.5%
NJ	2003	Increase	Introduction of an Alternative Minimum Assessment tax based on gross receipts, which applies if it exceeds the corporate franchise tax
NY	2002	Decrease	Corporate income tax rate falls from 8% to 7.5%
TN	2004	Increase	Corporate income tax rate increased from 6% to 6.5%
VT	2006	Decrease	Corporate income tax rate falls from 9.75% to 8.9%
VT	2007	Decrease	Corporate income tax rate falls from 8.9% to 8.5%
WV	2007	Decrease	Corporate income tax rate falls from 9% to 8.75%

Appendix C. List of IPO firms

The sample used in Tables 6 and 7 consists of 90 firms which went public between 1990 and 2007 for the sole purpose of allowing existing shareholders to cash out, as opposed to raising equity to fund the company's operations, investment plans, or to repay debt. Suitable IPOs are identified from Thomson Reuters' SDC database. In step 1, we filter on SDC field 'share type offered' to equal S (for secondary IPO, i.e. an IPO in which none of the proceeds is paid to the company). In step 2, we filter all non-secondary IPOs using SDC field 'use of proceeds' to include SDC codes 13, 18, 40, 79, 91, and 116 (which identify the use of proceeds as being a stock repurchase, the payment of a dividend, or redemption of preferred securities). In step 3, we verify, using IPO prospectuses, that the sole purpose of the non-secondary IPOs was indeed to allow shareholders to cash out and drop IPOs whose use of proceeds included the funding of operations, investments plans, or debt repayment. We exclude financial firms (SIC 6000-6999), regulated utilities (SIC 4900-4999), government entities (SIC ≥ 9000), and companies with CRSP share codes greater than 11.

IPO date	Name of IPO company	Purpose of IPO/use of proceeds
12-Apr-90	RMI Titanium Co	Secondary IPO (proceeds paid to pre-IPO shareholders)
26-Jul-90	Banner Aerospace Inc	Secondary IPO (proceeds paid to pre-IPO shareholders)
18-Sep-90	Pamida Holdings Corp	Secondary IPO (proceeds paid to pre-IPO shareholders)
11-Nov-91	Bally Gaming International Inc	Secondary IPO (proceeds paid to pre-IPO shareholders)
25-Nov-91	Broderbund Software Inc	Secondary IPO (proceeds paid to pre-IPO shareholders)
30-Jan-92	ElectroCom Automation Inc	Secondary IPO (proceeds paid to pre-IPO shareholders)
12-Feb-92	TNT Freightways Corp	Secondary IPO (proceeds paid to pre-IPO shareholders)
13-Feb-92	Living Centers of America Inc	Secondary IPO (proceeds paid to pre-IPO shareholders)
30-Mar-92	Eskimo Pie Corp	Secondary IPO (proceeds paid to pre-IPO shareholders)
28-Apr-92	Ben Franklin Retail Stores Inc	Secondary IPO (proceeds paid to pre-IPO shareholders)
29-Apr-93	Geon Co	Secondary IPO (proceeds paid to pre-IPO shareholders)
10-Jun-93	Department 56 Inc	Secondary IPO (proceeds paid to pre-IPO shareholders)
29-Sep-93	Belden Inc	Secondary IPO (proceeds paid to pre-IPO shareholders)
10-Dec-93	Camco International Inc	Secondary IPO (proceeds paid to pre-IPO shareholders)
26-Jan-94	O'Sullivan Industries Holdings	Secondary IPO (proceeds paid to pre-IPO shareholders)
27-Jan-94	Interim Services Inc	Secondary IPO (proceeds paid to pre-IPO shareholders)
10-May-94	Advocat Inc	Secondary IPO (proceeds paid to pre-IPO shareholders)
25-May-94	Merix Corp	Secondary IPO (proceeds paid to pre-IPO shareholders)
24-Jun-94	Case Corp	Secondary IPO (proceeds paid to pre-IPO shareholders)
30-Jun-94	Rawlings Sporting Goods Co	Secondary IPO (proceeds paid to pre-IPO shareholders)
27-Sep-94	Sterile Concepts Inc	Secondary IPO (proceeds paid to pre-IPO shareholders)
08-Nov-94	Thompson PBE Inc	Repurchase redeemable capital stock
01-Feb-95	Congoleum Corporation	Repurchase Class B stock
06-Mar-95	Dollar Tree Stores Inc	Redeem preferred stock
06-Mar-95	Riviana Foods Inc	Secondary IPO (proceeds paid to pre-IPO shareholders)
06-Sep-95	Ballantyne of Omaha Inc	Secondary IPO (proceeds paid to pre-IPO shareholders)
21-Sep-95	Midwest Express Holdings Inc	Secondary IPO (proceeds paid to pre-IPO shareholders)
14-Nov-95	Lexmark International Group	Secondary IPO (proceeds paid to pre-IPO shareholders)
25-Jan-96	World Color Press Inc	Secondary IPO (proceeds paid to pre-IPO shareholders)
01-Mar-96	Berg Electronics Corp	Redeem preferred stock
28-Mar-96	Century Aluminum Co	Secondary IPO (proceeds paid to pre-IPO shareholders)
03-Apr-96	Lucent Technologies Inc	Secondary IPO (proceeds paid to pre-IPO shareholders)
27-Jun-96	FactSet Research Systems Inc	Secondary IPO (proceeds paid to pre-IPO shareholders)
25-Jul-96	Strayer Education Inc	Pay S Corp dividend to shareholders
15-Aug-96	Consolidated Cigar Holdings Inc	Pay dividend to parent
09-Oct-96	Splash Technology Holdings Inc	Redeem preferred stock
25-Nov-96	Linens n Things Inc	Secondary IPO (proceeds paid to pre-IPO shareholders)
17-Dec-96	Swisher International Group Inc	Pay dividend to parent
15-May-97	General Cable Corp	Secondary IPO (proceeds paid to pre-IPO shareholders)
10-Oct-97	Stoneridge Inc	Secondary IPO (proceeds paid to pre-IPO shareholders)

IPO date	Name of IPO company	Purpose of IPO/use of proceeds
15-Oct-97	CH Robinson Worldwide Inc	Secondary IPO (proceeds paid to pre-IPO shareholders)
23-Oct-97	ITC Deltacom Inc	Secondary IPO (proceeds paid to pre-IPO shareholders)
11-Dec-97	Spectra Physics Lasers Inc	Secondary IPO (proceeds paid to pre-IPO shareholders)
28-Jan-98	Keebler Foods Co	Secondary IPO (proceeds paid to pre-IPO shareholders)
17-Feb-98	Steelcase Inc	Secondary IPO (proceeds paid to pre-IPO shareholders)
26-Mar-98	Columbia Sportswear Co	Secondary IPO (proceeds paid to pre-IPO shareholders)
22-Jul-98	USEC Inc	Secondary IPO (proceeds paid to pre-IPO shareholders)
21-Oct-98	Conoco	Secondary IPO (proceeds paid to pre-IPO shareholders)
22-Feb-99	Corporate Executive Board Co	Secondary IPO (proceeds paid to pre-IPO shareholders)
09-Jun-99	DiTech Corp	Redeem preferred stock
09-Nov-99	United Parcel Service Inc{UPS}	Redeem A Class shares
17-Nov-99	Agilent Technologies Inc	Pay dividend to parent
27-Jan-00	Packaging Corp of America	Redeem preferred stock
04-Apr-00	Cabot Microelectronics Corp	Pay dividend to parent
10-Jul-00	Axcelis Technologies Inc	Pay dividend to parent
27-Mar-01	Agere Systems Inc	Secondary IPO (proceeds paid to pre-IPO shareholders)
12-Nov-01	Advisory Board Co	Secondary IPO (proceeds paid to pre-IPO shareholders)
14-Nov-01	Weight Watchers Intl Inc	Secondary IPO (proceeds paid to pre-IPO shareholders)
10-Dec-01	Aramark Worldwide Corp	Repurchase stock from company's retirement plan
10-Jul-02	Kirkland's Inc	Repurchase preferreds and common stock
14-Nov-02	Constar International Inc	Secondary IPO (proceeds paid to pre-IPO shareholders)
24-Sep-03	Anchor Glass Container Corp	Redeem Series C participating preferreds
30-Oct-03	Overnite Corp	Secondary IPO (proceeds paid to pre-IPO shareholders)
19-Nov-03	Whiting Petroleum Corp	Secondary IPO (proceeds paid to pre-IPO shareholders)
24-Nov-03	Pinnacle Airlines Corp	Secondary IPO (proceeds paid to pre-IPO shareholders)
11-Dec-03	Compass Minerals Intl Inc	Secondary IPO (proceeds paid to pre-IPO shareholders)
13-Jan-04	CrossTex Energy Inc	Secondary IPO (proceeds paid to pre-IPO shareholders)
04-Feb-04	TODCO	Secondary IPO (proceeds paid to pre-IPO shareholders)
16-Jun-04	ADESA Inc	Repurchase stock from company's retirement plan
21-Jun-04	Jackson Hewitt Tax Service Inc	Secondary IPO (proceeds paid to pre-IPO shareholders)
21-Jul-04	Blackbaud Inc	Secondary IPO (proceeds paid to pre-IPO shareholders)
06-Aug-04	NAVTEQ Corp	Secondary IPO (proceeds paid to pre-IPO shareholders)
08-Dec-04	Foundation Coal Holdings Inc	Pay dividend to stockholders
20-Jan-05	Celanese Corp	Pay dividend to stockholders
27-Jan-05	W&T Offshore Inc	Secondary IPO (proceeds paid to pre-IPO shareholders)
08-Feb-05	FTD Group Inc	Repurchase preferred stock and junior preferred stock
02-May-05	Morningstar Inc	Secondary IPO (proceeds paid to pre-IPO shareholders)
13-Jun-05	Premium Standard Farms Inc	Secondary IPO (proceeds paid to pre-IPO shareholders)
28-Jun-05	NeuStar Inc	Secondary IPO (proceeds paid to pre-IPO shareholders)
22-Jul-05	Maidenform Brands Inc	Redeem all outstanding shares of preferred stock
04-Aug-05	Dresser-Rand Group Inc	Pay dividend to stockholders
08-Aug-05	K&F Industries Holdings Inc	Redeem junior preferred stock and pay a special dividend
10-Nov-05	IHS Inc	Secondary IPO (proceeds paid to pre-IPO shareholders)
21-Nov-05	Tronox Inc	Pay dividend to parent
14-Mar-06	Transdigm Group Inc	Secondary IPO (proceeds paid to pre-IPO shareholders)
03-May-06	DynCorp International Inc	Redeem preferred stock, pay prepayment penalties, and pay a special dividend
27-Jun-06	J Crew Group Inc	Redeem preferred stock
25-Jul-06	Chart Industries Inc	Pay dividend to stockholders
28-Feb-07	Coleman Cable Inc	Secondary IPO (proceeds paid to pre-IPO shareholders)
12-Jun-07	Bway Holding Co	Secondary IPO (proceeds paid to pre-IPO shareholders)

Figure 1. Size distribution of public and private firms.

This figure shows the size distribution of public and private firms in our full (top graph) and matched (bottom graph) samples. Both samples are defined in Table 1. The graphs show two Epanechnikov kernel densities, one each for public and private companies, of the natural logarithm of total assets, in $ millions of 2000 purchasing power, of the firms in our full and matched samples. We set 0.4 to be the width of the kernel density window around each point.

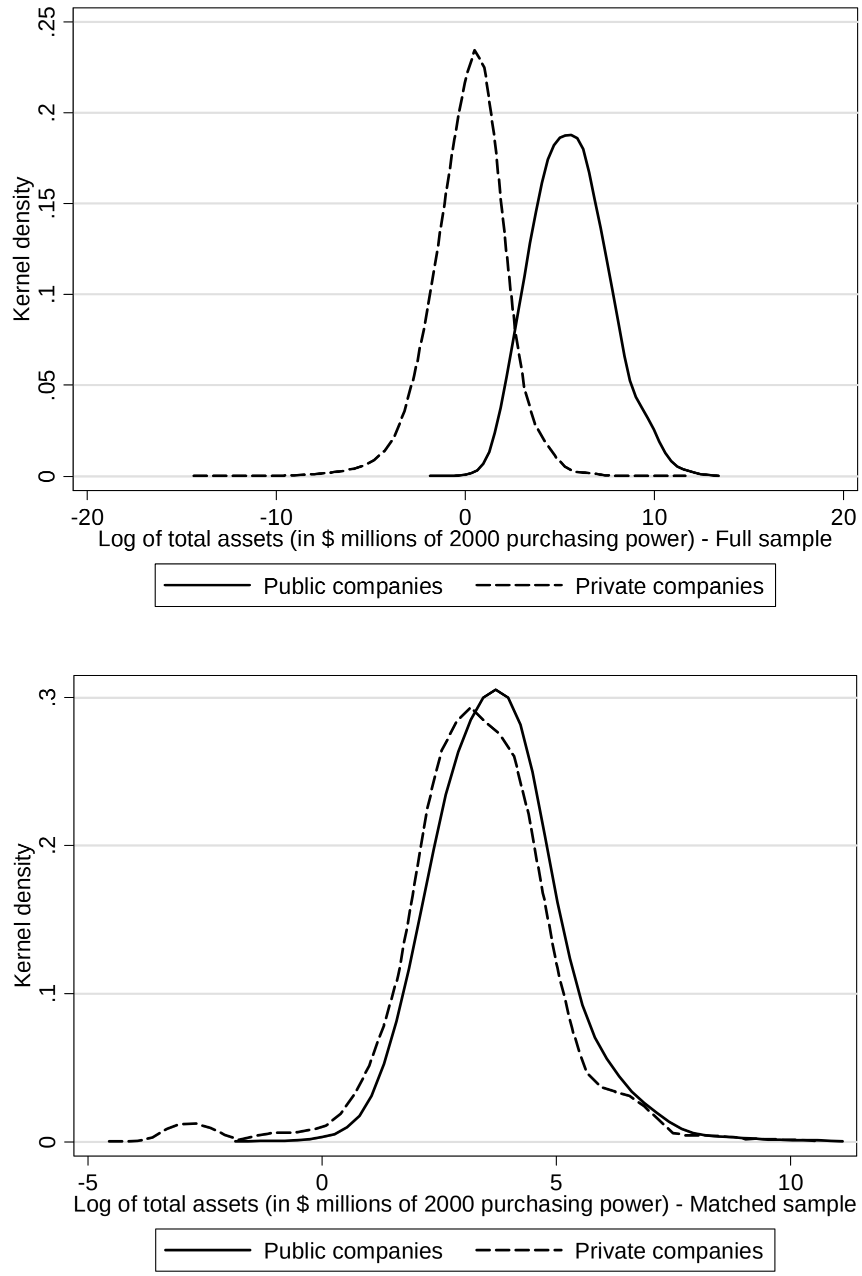

Figure 2. Do public companies have smoother earnings and payout policies?

This figure illustrates the conjecture that if short-termism is a feature of stock markets, public firms will have smoother profit growth smoother dividend payout policies than private ones. Each of the three graphs show two Epanechnikov kernel densities, one each for public and private companies. The top two graphs show the density of the residuals of regressions of the within-firm time-series standard deviation of profit growth on the time-series mean of firm size and an indicator variable set equal to one if the company's earnings time series includes at least one instance of negative income. The regressions use two alternative measures of earnings: Net income before extraordinary items (top left) and operating income after depreciation (top right). The regressions from which the residuals are estimated are analogous to the ones reported in columns 1 and 2 of Table 9, respectively, but without the indicator variable capturing public companies. The bottom graph shows the density of the residuals of a regression of the within-firm time-series standard deviation of payouts on the time-series mean of firm size and an indicator variable set equal to one if the company pays no dividends. The regression from which the residuals are estimated is analogous to the one reported in column 3 of Table 9, again without the indicator variable capturing public companies. We use as bandwidth for the kernels the width that would minimize the mean integrated squared error if the data were Gaussian and Gaussian kernels were used.

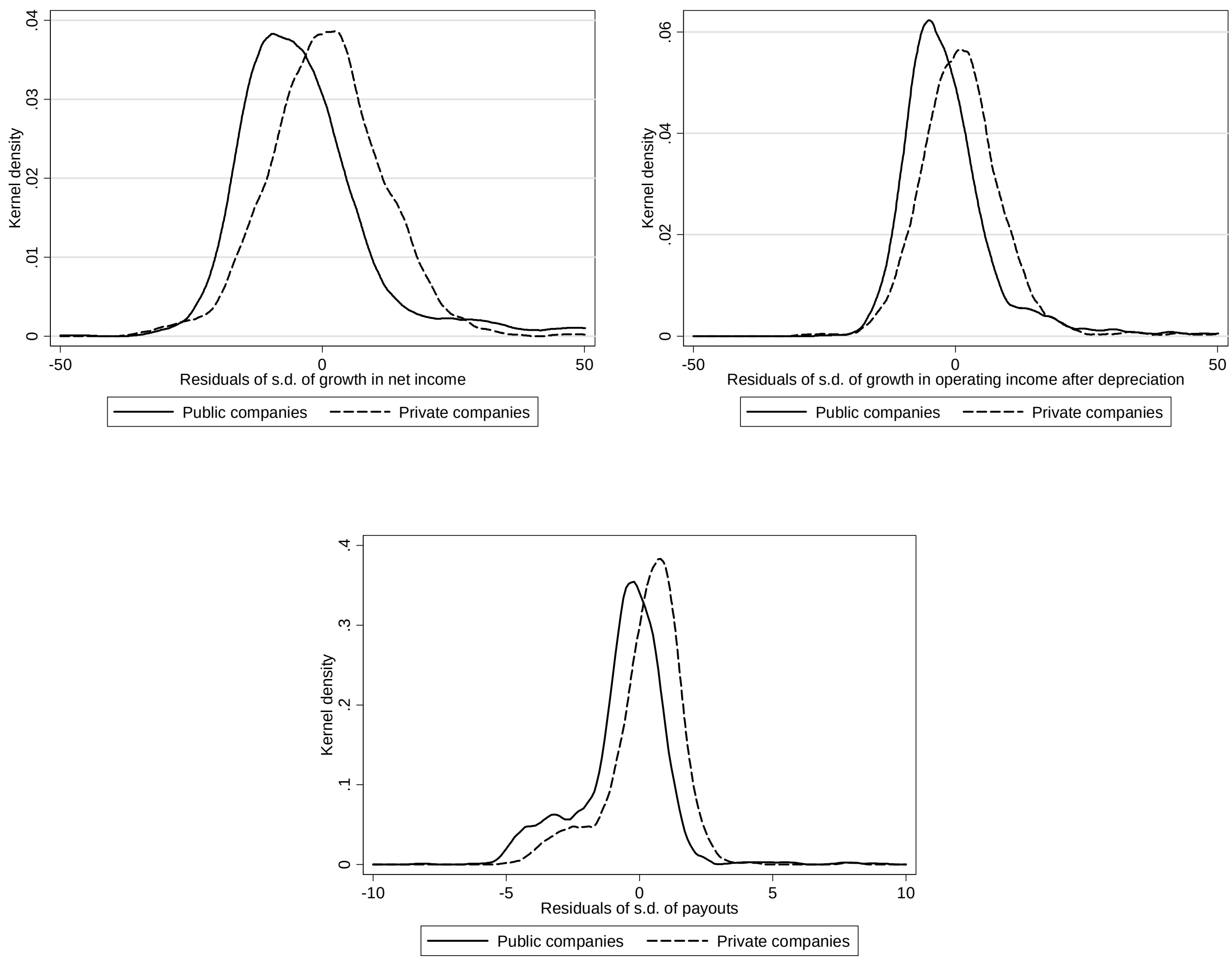

Table 1. Descriptive Statistics – Public and private samples.

This table presents descriptive statistics of the variables used in our analysis of investment behavior in public and privately-held companies. Our data cover the period from 2002 to 2007. We report statistics for the full samples of public and private firms (denoted 'F') and for a matched sample (denoted 'M'). To be part of the full sample of public firms, a company has to be recorded in both the Compustat and CRSP databases over our sample period; be incorporated in the U.S. and listed on a major U.S. exchange (NYSE, NASDAQ or AMEX); have valid stock prices in CRSP; and have a CRSP share code of 10 or 11 (which screens out REITs, funds, ADRs, etc.). The full sample of private companies is drawn from the Sageworks Inc. database of privately-held North American firms, from which we exclude Canadian companies as well as observations with data quality problems (specifically, those that fail to satisfy basic accounting identities). As is customary, we exclude from both the public and private samples financial firms (SIC 6000-6999), regulated utilities (SIC 4900-4999), and government entities (SIC ≥ 9000). In addition, we exclude companies for which we have fewer than two observations with complete data for all the variables used in our baseline analysis (see Table 2); because we estimate panel-data models with firm fixed effects, such companies do not contribute to the estimation. The matched sample of public and private companies is constructed as follows: Starting in the first year of our sample period, for each public company, we identify the private company in the same industry (four-digit NAICS, equivalent to three-digit SIC) and fiscal year that is closest in terms of total assets (*TA*). For a match to be consummated, we require $\max(TA_{public}, TA_{private}) / \min(TA_{public}, TA_{private})$ to be less than 2. If no match can be found in a given fiscal year, the observation is discarded and a new match is attempted for the company in the following year. Once a match is formed, it is kept intact for as long as both the public and private firms remain in our sample, to maximize the available time series for each firm. If a matching firm exits the panel, a new match is spliced in. Matching is done with replacement. The table reports means, standard deviations (in italics underneath the means), and medians (in brackets underneath the standard deviations). The variables are defined as follows. Total assets (Compustat item *at* or its Sageworks equivalent) is in $ millions of 2000 purchasing power, deflated using the annual GDP deflator, at the beginning of the fiscal year. Gross investment is the annual increase in gross fixed assets (Compustat data item *ppegt* or its Sageworks equivalent) scaled by beginning-of-year nominal total assets; net investment is defined analogously using net fixed assets (Compustat item *ppent* or its Sageworks equivalent). Sales growth is the annual percentage increase in sales (Compustat item *sale* or its Sageworks equivalent). Predicted *Q* is computed as follows. Following Campello and Graham (2007), we first regress each public firm's Tobin's *Q* (Compustat items *prcc_f*×*cshpri* + *pstkl* + *dltt* + *dlc* – *txditc* divided by beginning-of-year total assets) on the firm's sales growth, ROA, net income before extraordinary items, book leverage, and year and industry fixed effects (using three-digit NAICS industries). We then use the regression coefficients to generate predicted *Q* for each firm, both public and private ones. Analysts *Q* is the four-digit NAICS industry median of Tobin's *Q* based on sell-side research analysts' earnings forecasts from I/B/E/S, constructed using the Cummins et al. (2006) definition of a firm's intrinsic value (when available and positive) scaled by its book value (Compustat item *at* or its Sageworks equivalent). ROA is operating income before depreciation (Compustat item *oibdp* or its Sageworks equivalent) scaled by beginning-of-year total assets. Cash holdings is beginning-of-year cash and short-term investments (Compustat item *che* or its Sageworks equivalent) and Leverage (book) is beginning-of-year long-term and short-term liabilities (Compustat items *dltt* + *dlc* or their Sageworks equivalents), both scaled by beginning-of-year total assets. All variables (except predicted *Q*) are winsorized 0.5% in each tail to reduce the impact of outliers. The last four columns report pairwise differences in means or medians between the relevant samples, with ***, **, and * indicating a difference that is significant in a *t*-test (for means) or a Pearson χ^2 test (for medians) at the 1%, 5%, and 10% level, respectively.

	Full sample (F)		Matched sample (M)		Differences in means (*t*-test) or medians (Pearson χ^2 test)			
	Public	*Private*	*Public*	*Private*	*F: Pub – Pri*	*M: Pub – Pri*	*Pub: F – M*	*Pri: F – M*
Firm size								
Total assets ($m)	1,364.4	7.1	144.7	120.0	1,357.3***	24.7*	1,219.7***	-112.9***
	2,958.1	*190.2*	*692.8*	*675.5*				
	[246.2]	[1.3]	[40.3]	[28.0]	245.0***	12.3***	205.9***	-26.7***
Investment spending								
Gross investment	0.045	0.076	0.040	0.097	-0.031***	-0.056***	0.005*	-0.020***
	0.154	*0.261*	*0.191*	*0.304*				
	[0.023]	[0.017]	[0.017]	[0.016]	0.005***	0.001	0.006***	0.001
Net investment	0.022	0.033	0.022	0.094	-0.011***	-0.072***	0.000	-0.061***
	0.123	*0.205*	*0.150*	*0.302*				
	[0.002]	[0.000]	[0.000]	[0.009]	0.002***	-0.009***	0.003***	-0.009***
Investment opportunities								
Sales growth	0.183	0.177	0.256	0.327	0.006	-0.071***	-0.073***	-0.150***
	0.674	*0.652*	*0.925*	*1.075*				
	[0.087]	[0.070]	[0.091]	[0.111]	0.016***	-0.020***	-0.004	-0.041***
Predicted *Q*	1.817	1.473	2.119	1.964	0.344***	0.155***	-0.302***	-0.491***
	0.663	*1.082*	*0.774*	*1.229*				
	[1.778]	[1.386]	[2.047]	[1.889]	0.392***	0.158***	-0.270***	-0.504***
Analysts *Q*	1.136	1.004	1.188	1.188	0.133***	0.000	-0.052***	-0.185***
	0.478	*0.447*	*0.396*	*0.396*				
	[1.103]	[0.941]	[1.164]	[1.164]	0.162***	0.000	-0.060***	-0.223***
Firm characteristics								
ROA	0.065	0.075	-0.060	0.084	-0.010**	-0.144***	0.124***	-0.009
	0.286	*1.069*	*0.437*	*0.986*				
	[0.111]	[0.095]	[0.051]	[0.123]	0.016***	-0.072***	0.060***	-0.028***
Cash holdings	0.225	0.152	0.304	0.151	0.073***	0.152***	-0.078***	0.001
	0.239	*0.202*	*0.267*	*0.200*				
	[0.131]	[0.073]	[0.228]	[0.074]	0.058***	0.154***	-0.097***	-0.001
Leverage (book)	0.199	0.311	0.149	0.218	-0.111***	-0.069***	0.050***	0.092***
	0.230	*0.455*	*0.250*	*0.264*				
	[0.145]	[0.157]	[0.055]	[0.132]	-0.012***	-0.077***	0.090***	0.025***
No. of observations	19,203	88,579	4,975	4,975				
No. of firms	3,926	32,207	1,666	620				

Table 2. Comparing public and private firms' sensitivity to investment opportunities.

In this table, we analyze the sensitivity of investment spending to investment opportunities, exploiting within-firm variation. The dependent variable is gross investment (the annual increase in gross fixed assets, using Compustat data item *ppegt* or its Sageworks equivalent) scaled by beginning-of-year total assets. We obtain similar results using net investment (the scaled increase in net fixed assets); see Table 3. We use three different measures of investment opportunities: Sales growth, our preferred measure, which is available for both public and private firms (columns 1 through 3); predicted *Q*, which is also available for both types of firms (column 4), though we lose a small number of observations due to missing leverage data, which is used in the construction of predicted *Q*; and analysts *Q*, which is only available for the subset of public firms that are covered by I/B/E/S (column 5). In order to be able to define analysts *Q* for all sample firms (including private firms as well as public firms that are not covered by an analyst), we use industry median values instead of firm-specific values. Following parts of the empirical investment literature, we include ROA. This is sometimes interpreted as a possible proxy for financing constraints. All regressions include firm fixed effects. All regressions use the matched sample; see Table 1 for details of how these samples are constructed. In columns 1, 4, and 5, the analysis includes both public and private firms, in which case we interact investment opportunities and ROA with a dummy equal to one if the firm is publicly traded. Columns 2 and 3 include only public and private firms, respectively. All variables are defined in Table 1. Each regression includes an intercept and year fixed-effects, which are not reported to conserve space. The data panel is set up in calendar time; fiscal years ending January 1 through May 31 are treated as ending in the prior calendar year. Heteroskedasticity-consistent standard errors clustered at the firm level are shown in italics underneath the coefficient estimates in all columns except for column 4, where the standard errors are obtained by bootstrapping in order to account for the fact that predicted *Q* is an estimated regressor. When bootstrapping, we use the matched public-private company pairs as resampling clusters and perform 500 replications. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

	Dependent variable: Gross investment / lagged total assets				
Measure of investment opportunities:	Sales growth			Predicted *Q*	Analysts *Q*
Sample	All matched-sample firms	Matched-sample public firms	Matched-sample private firms	All matched-sample firms	All matched-sample firms
	(1)	(2)	(3)	(4)	(5)
Investment opportunities	0.136***	0.038***	0.134***	0.383***	0.225**
	0.013	*0.009*	*0.012*	*0.030*	*0.087*
Investment opp. x public	-0.097***			-0.226***	-0.234***
	0.015			*0.030*	*0.090*
ROA	0.173***	0.038	0.172***	0.519***	0.112
	0.014	*0.023*	*0.013*	*0.034*	*0.080*
ROA x public	-0.135***			-0.342***	-0.063
	0.027			*0.042*	*0.084*
R^2 (within)	29.6 %	5.5 %	42.5 %	28.1%	14.3 %
Wald test: all coeff. = 0 (*F*)	32.1***	5.6***	36.4***	15.1***	2.7***
No. observations	9,950	4,975	4,975	9,931	9,950
No. firms	2,286	1,666	620	2,282	2,286

Table 3. Alternative Specifications.

In this table, we continue working with our preferred specification (matched sample of public and private firms, sales growth as measure of investment opportunities) and exploiting within-firm variation. In columns 1 to 6, we analyze the robustness of the results presented in Table 2, Panel A to using different sets of control variables. In column 7, we change the dependent variable from gross to net investment (i.e., the change in net fixed assets over beginning-of-year total assets). In column 8, we use a different matching algorithm to generate the estimation sample; instead of matching on total assets and industry, we match on sales growth and industry. In column 9, we estimate the investment equation in the universe of public Compustat and private Sageworks firms, requiring only that firms be based in the U.S. and excluding financial firms (SIC 6), regulated utilities (SIC 49), and government entities (SIC 9). All variables are defined in Table 1. Each regression includes an intercept and year fixed-effects (not reported). Heteroskedasticity-consistent standard errors clustered at the firm level are shown in italics underneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

	Dependent variable: Investment / lagged total assets								
Investment measure:	Gross	Gross	Gross	Gross	Gross	Gross	Net	Gross	Gross
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Investment opp. (sales growth)	0.136***	0.136***	0.136***	0.136***	0.092***	0.092***	0.210***	0.083***	0.054***
	0.013	*0.013*	*0.011*	*0.010*	*0.022*	*0.020*	*0.016*	0.020	*0.004*
Investment opp. x public	-0.099***	-0.099***	-0.097***	-0.097***	-0.056**	-0.058***	-0.175***	-0.037*	-0.016**
	0.016	*0.016*	*0.014*	*0.013*	*0.023*	*0.022*	*0.017*	0.022	*0.006*
ROA	0.174***	0.174***	0.173***	0.172***	0.174***	0.174***	-0.006	0.027	0.034***
	0.014	*0.015*	*0.012*	*0.011*	*0.013*	*0.012*	*0.019*	0.019	*0.005*
ROA x public	-0.138***	-0.138***	-0.140***	-0.135***	-0.112***	-0.118***	0.007	0.026	0.021
	0.028	*0.028*	*0.030*	*0.026*	*0.027*	*0.030*	*0.028*	0.038	*0.027*
Cash holdings	0.137**	0.110				0.116*			
	0.067	*0.191*				*0.065*			
Cash holdings x public		0.040							
		0.191							
Book leverage			-0.165***	-0.382***		-0.157**			
			0.059	*0.108*		*0.062*			
Book leverage x public				0.333***					
				0.114					
Size (*ln*(total assets))					-0.055***	-0.055***			
					0.018	*0.017*			
R^2 (within)	30.0 %	30.0 %	30.1 %	31.7 %	31.0 %	32.4 %	50.0 %	3.7 %	3.3 %
Wald test: all coeff. = 0 (F)	32.7***	38.6***	49.7***	58.7***	46.8***	80.1***	27.3***	7.7***	43.9***
No. observations	9,950	9,950	9,931	9,931	9,950	9,931	9,950	35,760	107,782
No. firms	2,286	2,286	2,282	2,282	2,286	2,282	2,286	7,203	36,133

Table 4. GMM estimates of public and private firms' investment sensitivities.

This table explores the robustness of the Table 2 results to the potential measurement error in investment opportunities, using Arellano and Bond's (1991) one-step GMM estimator (or a variation thereof). We focus on our preferred specification, the matched sample of public and private firms with sales growth as the measure of investment opportunities. As in Table 2, we exploit within-firm variation. Specifically, we first-difference the data to remove firm fixed effects. For ease of comparison, column 1 reproduces the within-groups results from column 4 in Panel A of Table 2 as a baseline; columns 2 to 6 contain the GMM results. In columns 2 and 3, we estimate a static GMM model, using investment and sales growth dated *t*-5 to *t*-3 and year effects as instruments. Note that variables dated *t*-2 are mechanically correlated with the first-differences of sales growth and investment and so cannot be included in the instrument set. The specification in column 4 is dynamic and thus includes first lags of all variables; however, for brevity, we only report the coefficient for lagged investment. In this case, only variables dated *t*-5 and *t*-4 can be used as instruments, which greatly affects identification as our panel is relatively short. Columns 5 and 6 show results from a system GMM model which jointly estimates a first-differenced equation as in columns 2 and 3 (instrumented with lagged variables in levels) and an equation in levels instrumented with lagged differences (see Blundell and Bond (1998)). This allows us to include a dummy for public companies as a control. All variables are defined in Table 1. Each regression includes an intercept and year fixed-effects (not reported). GMM equations show the *p*-values of the Hansen test of over-identification restrictions and the Arellano-Bond test for AR(3) in first differences (in column 4, an AR(4) test is not identified). Heteroskedasticity-consistent standard errors clustered at the firm level are shown in italics underneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

	Dependent variable: Gross investment /lagged total assets					
	Within-groups (fixed effects)	First diff. GMM, static	First diff. GMM, static	First diff. GMM, dynamic	System GMM, static	System GMM, static
	(1)	(2)	(3)	(4)	(5)	(6)
Investment lagged				-0.359		
				0.605		
Inv. opportunities (sales growth)	0.136^{***}	0.159	0.146^{**}	2.241	0.220^{*}	0.252^{***}
	0.013	*0.126*	*0.073*	*2.813*	*0.113*	*0.096*
Investment opp. x public	-0.097^{***}	-0.217	-0.202^{*}	-2.236	-0.219^{*}	-0.257^{**}
	0.015	*0.148*	*0.113*	*2.792*	*0.132*	*0.116*
ROA	0.173^{***}	0.113	0.064	0.040	0.059	0.167^{*}
	0.014	*0.421*	*0.156*	*0.135*	*0.304*	*0.100*
ROA x public	-0.135^{***}	-0.052			0.119	
	0.027	*0.419*			*0.314*	
Public					0.031	0.051
					0.053	*0.040*
Instrument set		*Inv. (3-5) Sales growth (3-5)*	*Inv. (3-5) Sales growth (3-5)*	*Inv. (4-5) Sales growth (4-5) ROA (4-5)*	*Inv. (3-5) Sales growth (3-5) Public Levels eq.*	*Inv. (3-5) Sales growth (3-5) Public Levels eq.*
Hansen test of overid. restr. (*p*)		0.818	0.877	0.779	0.485	0.509
Arellano-Bond test: AR(3) (*p*)		0.918	0.933	0.915	0.552	0.602
No. observations	9,950	7,474	7,474	5,055	9,950	9,950
No. firms	2,286	2,217	2,217	1,773	2,286	2,286

Table 5. Public and private firms' reactions to state corporate income tax changes.

In this table, we continue our analysis of the sensitivity of the results presented in Table 2 to potential measurement error in investment opportunities. We use changes in state corporate income taxes as a plausibly exogenous shock to investment opportunities: A decrease in a state's corporate income tax rate reduces the cost of capital for firms operating in that state, which should have a positive effect on investment, and vice versa for tax increases. We focus our analysis on tax changes that occurred during our sample period (2002-2007) and that can unequivocally be categorized as either an increase or a decrease. We identify ten tax changes in eight affected states (see Appendix B for details). The sample of private companies is limited to C Corps because they are subject to the same tax regime as public companies (in contrast to S Corps, LLCs, or partnerships). However, in column 6, we focus on private non-C Corps to validate the identification strategy. Tax change is an indicator variable set equal to 1 (-1) for companies headquartered in a state that passed a tax cut (tax increase) which became effective during the fiscal year in question. Column 1 is similar to the models presented in Table 2, except that it includes tax change as a plausibly exogenous shock to investment opportunities. In column 3, we investigate pre- or post-trends in the tax change effect by adding indicator variables that identify companies in states that will undergo a tax change in one year (*t*-1) or in two years (*t*-2), or that underwent a change one year (*t*+1) or two years (*t*+2) ago. In columns 6 and 7, we repeat the column 1 and 2 analysis limiting the sample of public companies to those with total real assets in the bottom quartile of the data (specifically, less than $65.4 million). Each regression includes an intercept as well as firm and year fixed-effects (not reported). Heteroskedasticity-consistent standard errors clustered at the firm level are shown in italics underneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

	Dependent variable: Gross investment /lagged total assets						
	Full sample of public companies and private C Corps				Private non-C Corps	Bottom quartile public companies, all private C Corps	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Tax change (decrease = 1, increase = -1)	0.023**	0.022*	0.027**	0.026**	0.001	0.025**	0.024**
	0.011	*0.011*	*0.012*	*0.012*	*0.009*	*0.011*	*0.011*
Tax change x public	-0.026**	-0.025**	-0.028**	-0.026**		-0.030*	-0.027*
	0.012	*0.012*	*0.012*	*0.012*		*0.016*	*0.016*
Sales growth	0.056***		0.056***		0.052***	0.056***	
	0.007		*0.007*		*0.005*	*0.007*	
Sales growth x public	-0.018**		-0.018**			-0.038***	
	0.009		*0.009*			*0.009*	
ROA	0.028***		0.028***		0.036***	0.028***	
	0.009		*0.009*		*0.006*	*0.009*	
ROA x public	0.027		0.027			-0.043	
	0.028		*0.028*			*0.034*	
Tax change (*t*-2)			-0.007	-0.008			
			0.014	*0.014*			
Tax change (*t* -1)			0.010	0.010			
			0.009	*0.010*			
Tax change (*t* +1)			0.008	0.009*			
			0.005	*0.005*			
Tax change (*t* +2)			0.001	0.001			
			0.006	*0.006*			
R^2 (within)	3.3 %	0.0 %	3.3 %	0.1 %	4.7 %	2.9 %	0.0 %
Wald test: all coefficients = 0 (*F*)	12.9***	2.2**	9.8***	1.8**	34.8***	8.4***	3.0***
No. observations	52,281	52,281	52,281	52,281	55,501	37,878	37,878
No. firms	15,684	15,684	15,684	15,684	20,449	13,039	13,039

Table 6. Descriptive Statistics – IPO sample.

This table provides descriptive statistics of the variables used in our analysis of investment behavior before and after the IPOs of U.S. companies that go public on the NYSE, AMEX, or Nasdaq exchanges for the sole purpose of allowing existing shareholders to cash out (as opposed to raising equity for the company, which is the usual reason to go public in the U.S.). Suitable IPOs are identified from Thomson Reuters' SDC database and listed in Appendix C. The IPO companies went public between 1990 and 2007. After excluding financial firms (SIC 6000-6999), regulated utilities (SIC 4900-4999), government entities (SIC ≥ 9000), and companies with CRSP share codes greater than 11, we are left with 90 IPO firms. We collect their post-IPO accounting data from Compustat and hand-collect their pre-IPO accounting data from their IPO prospectuses and 10-K filings available in the SEC's Edgar and Thomson Research databases. On average, we have 4.4 pre-IPO years of accounting data. Column 1 reports descriptive statistics for the fiscal years before and up to the IPO (unless the IPO takes place more than 9 months before the end of the fiscal year, in which case we consider the IPO year as belonging to the post-IPO period). Column 2 reports descriptive statistics for the post-IPO fiscal years. Column 3 shows descriptive statistics for a matched control sample of public companies. To be eligible for matching, a public company must be in both Compustat and CRSP; be incorporated in the U.S. and listed on a major U.S. exchange; have valid stock price data in CRSP; and have a CRSP share code of 10 or 11. Each IPO company is matched in its first sample year to up to five public companies in the same industry (three-digit SIC) with the closest total assets to the IPO firm in the year of the match. In three cases, this algorithm yields no eligible matches, so we broaden the industry criterion to two-digit SIC. On average, we have 3.7 matches per IPO firm. The variables are defined as follows. Investment (no R&D) is capital expenditures (Compustat item *capx*) scaled by beginning-of-year total assets (Compustat item *at*); Investment (with R&D) is defined analogously, adding capital expenditures and R&D expenditures (Compustat items *capx + xrd*). Investment opp. (sales growth) is defined as the annual percentage increase in sales (Compustat item *sale*). ROA is operating income before depreciation (Compustat item *oibdp*) and cash holdings is beginning-of-year cash and short-term investments (Compustat item *che*), both scaled by beginning-of-year total assets. Total assets is in $ millions of 2000 purchasing power, deflated using the annual GDP deflator. All variables are winsorized 0.5% in each tail to reduce the impact of outliers, and we trim winsorized sales growth at 2. Data for cash holdings is missing for 41 pre-IPO firm-years. Panel A reports means and standard deviations (in italics underneath the means), while Panel B reports medians. The last three columns report pairwise differences in means or medians between the relevant samples, with ***, **, and * indicating a difference that is significant in a *t*-test (for means) or a Pearson χ^2 test (for medians) at the 1%, 5%, and 10% level, respectively.

Table 6. Continued.

	IPO companies			Differences in means/medians		
	Pre IPO	*Post IPO*	Matched controls	*Post – Pre*	*Pre – Matched*	*Post – Matched*
Panel A: Means						
Investment (no R&D)	0.078	0.071	0.081	-0.008	-0.002	-0.010***
	0.100	*0.082*	*0.088*			
Investment (with R&D)	0.108	0.097	0.114	-0.011	-0.006	-0.016***
	0.142	*0.101*	*0.104*			
Inv. opp. (sales growth)	0.153	0.124	0.120	-0.029**	0.033***	0.004
	0.227	*0.210*	*0.247*			
ROA	0.191	0.193	0.159	0.002	0.031***	0.034***
	0.184	*0.142*	*0.129*			
Cash holdings	0.064	0.140	0.142	0.076***	-0.077***	-0.001
	0.105	*0.160*	*0.167*			
Total assets ($m)	1,603.1	1,877.5	2,097.6	274.4	-494.5**	-220.1
	3,580.3	*4,957.1*	*5,268.2*			
Panel B: Medians						
Investment (no R&D)	0.051	0.047	0.054	-0.004***	-0.003	-0.007***
Investment (with R&D)	0.064	0.069	0.088	0.004	-0.024***	-0.019***
Inv. opp. (sales growth)	0.113	0.100	0.093	-0.013	0.021***	0.008
ROA	0.166	0.169	0.156	0.003	0.010	0.013**
Cash holdings	0.020	0.082	0.070	0.062***	-0.049***	0.012
Total assets ($m)	327.5	544.2	403.2	216.7***	-75.8**	140.9***
No. of observations	397	566	3,538			
No. of firms	90	90	329			

Table 7. Changes in sensitivity to investment opportunities around IPOs.

In this table, we estimate changes in the sensitivity of investment spending to investment opportunities around the IPOs of companies that go public for the sole purpose of allowing some of their existing shareholders to cash out. We use sales growth as a measure of investment opportunities, given that this is the only measure available for pre-IPO observations. As in previous tables, we exploit within-firm variation by including firm fixed effects. Following parts of the empirical investment literature, we include ROA. This is sometimes interpreted as a possible proxy for financing constraints. Columns 1 and 2 report own-difference results for the IPO sample, where we interact investment opportunities and ROA with an indicator variable that equals one if the observation is post-IPO. Columns 3 and 4 report difference-in-difference results based on combining data from the IPO sample with data from the matched control sample. This allows us to interact investment opportunities and ROA with separate indicators for pre- and post-IPO. Uncrossed variables capture the effect of investment opportunities and ROA on the investment decisions of the matched control public companies, while the interaction terms test whether IPO companies have investment behavior that is significantly different from that of their matched controls either before or after going public, respectively. We also allow for a level difference in investment spending between IPO and matched companies by including a post-IPO indicator. (Note that the presence of firm fixed effects rules out simultaneous inclusion of a pre-IPO indicator.) All variables are defined in Table 6. Each regression includes an intercept and year fixed-effects (not reported for brevity). Heteroskedasticity-consistent standard errors are shown in italics underneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

	Dependent variable: Investment / lagged total assets			
	Own difference		Diff-in-diff with matched controls	
	without R&D (1)	with R&D (2)	without R&D (3)	with R&D (4)
Investment opportunities (sales growth)	0.074***	0.111***	0.013*	0.027***
	0.025	*0.031*	*0.007*	*0.008*
Investment opp. x pre-IPO			0.066**	0.092***
			0.028	*0.035*
Investment opp. x post-IPO	-0.058*	-0.080*	0.003	0.006
	0.032	*0.041*	*0.020*	*0.027*
ROA	0.053	0.095	0.139***	0.140***
	0.063	*0.074*	*0.018*	*0.027*
ROA x pre-IPO			-0.093	-0.052
			0.067	*0.080*
ROA x post-IPO	0.059	0.057	-0.019	0.019
	0.053	*0.062*	*0.038*	*0.046*
Post-IPO	0.001	-0.004	-0.004	-0.006
	0.010	*0.012*	*0.009*	*0.012*
R^2 (within)	19.4 %	21.1 %	13.9 %	14.3 %
Wald test: all coefficients = 0 (F)	6.7***	7.3***	16.6***	14.8***
No. observations	963	963	4,501	4,501
No. firms	90	90	419	419

Table 8. Short-termism at work: Interacting investment sensitivity with stock price sensitivity to earnings.

In this table, we test the prediction that the difference in investment sensitivity between private and public firms documented in Tables 2 to 4 is driven by public firms whose stock prices are highly sensitive to earnings announcements. Result 3 predicts that managers have an incentive to make myopic investment decisions to boost current earnings (and thus their stock price) only to the extent that their stock price is sensitive to current earnings. We follow the accounting literature and use the earnings response coefficient (ERC) to capture a firm's stock price sensitivity to earnings. Thus, a triple interaction of investment opportunities (as captured by sales growth), an indicator for public firms, and lagged ERC should be significantly negative, i.e., the difference in investment sensitivity between private and public companies should increase in ERC. We estimate ERCs at the industry-year level as the slope coefficient of a regression of one plus a firm's stock return during the fiscal year on a constant and the firm's earnings per share (EPS). Stock returns are computed as the annually compounded monthly buy-and-hold return (including dividends; CRSP variable *ret*). Following Kothari (1992), EPS (before extraordinary items) is Compustat variable *epspx* scaled by beginning-of-year stock price (CRSP variable *prc*). We run annual regressions using the full sample of public companies (after trimming 1% of returns and EPS) and allow the slope coefficient (ERC) to vary at the industry-level. The estimated ERCs (one for each industry and year) are winsorized 0.5% in each tail. We use the Fama and French (1997) classification of 30 industry groups, available from Kenneth French's webpage. Results are robust to using Fama-French 38 or 49 industries instead. Private firms are grouped into Fama-French industries based on their NAICS codes, which we map to SIC codes using the U.S. Census Bureau's NAICS-SIC bridge, available at http://www.census.gov/epcd/naics02/index.html. Panel A shows triple-difference estimation results using our matched sample of private and public firms, exploiting within-firm variation; the regression includes an intercept and year fixed-effects (not reported). All variables other than ERC are defined in Table 1. Panel B shows the effect of sales growth on investment, as estimated in Panel A, for private and public firms at the 25th and 75th percentile of the ERC distribution within the matched sample (0.217 and 0.929, respectively). Heteroskedasticity-consistent standard errors clustered at the firm level are shown in italics. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

Panel A: Triple-difference estimation results

		Sales growth x …								
	Sales Growth	… public	… ERC	… public x ERC	public x ERC	ERC	ROA	ROA x public	R^2 (within) / Test: all coef. = 0	No. obs. / No. firms
Dep. var.: Gross investment / lagged assets	0.040	-0.014	0.125***	-0.105**	0.056**	-0.044**	0.204***	-0.166***	31.7 %	9,950
	0.031	*0.032*	*0.046*	*0.047*	*0.022*	*0.020*	*0.022*	*0.032*	24.6***	3,719

Panel B: Implied investment sensitivity to sales growth

	Low ERC (25th percentile)		High ERC (75th percentile)	
	coeff.	std. error	coeff.	std. error
Private firms	0.067***	*0.022*	0.157***	*0.017*
Public firms	0.031***	*0.008*	0.045***	*0.011*
Difference	0.037	*0.024*	0.112***	*0.021*

Table 9. Income Smoothing and Dividend Policy

In this table, we test the conjecture that if short-termism is a feature of stock markets, public firms will have smoother profit growth and/or smoother dividend payout policies than do private ones. The unit of observation in the regressions is a firm rather than a firm-year and the sample used is our matched sample. In columns 1 and 2, the dependent variables are the within-firm time-series standard deviations of the real annual growth in net income before extraordinary items (Compustat item *ib* or its Sageworks equivalent) and in operating income after depreciation (Compustat items *oibdp* – *dp* or their Sageworks equivalents), respectively. The covariate of interest is an indicator variable set equal to one for public companies. We control for firm size since, all else equal, larger firms have more volatile profit growth and dividend payout levels. We measure firm size as the within-firm time-series mean of total assets. We also control for whether a firm has had negative income during its time in our sample, in order to account for the fact that the income of such a firm might be more volatile. In column 3, the dependent variable is the within-firm time-series standard deviation of the payouts paid by each firm to its shareholders (Compustat item *dvc* or its Sageworks equivalent). Here, we control for whether the firm does not pay dividends during its time in our sample, in order to account for the fact that such a firm will have smooth payouts by construction. Intercepts are not reported. Heteroskedasticity-consistent standard errors are shown in italics underneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

	Dependent variable: Standard deviation of:		
	Growth in net income bef. extra. items (1)	Growth in oper. income after deprec. (2)	Payouts (3)
=1 if public company	-2.626***	-2.626***	-0.411***
	0.943	*0.664*	*0.157*
Mean *ln*(total assets)	9.342***	6.878***	0.773***
	0.607	*0.422*	*0.194*
=1 if negative income	9.244***	4.967***	
	0.886	*0.564*	
=1 if zero payouts			-3.890***
			0.569
Adjusted R^2	32.0%	38.0%	12.8%
Wald test: all coefficients = 0 (*F*)	130.3***	126.8***	18.5***
No. observations (firms)	2,286	2,286	2,286

Table 10. Earnings management to avoid reporting losses

In this table, we indirectly test the hypothesis that short-termism induces public firms to make sub-optimal investment decisions in an effort to avoid reporting accounting losses. We ask whether public firms are more likely to report earnings just above zero than are private firms. We focus on two intervals around zero reported net income scaled by total assets, namely (-0.10, 0.10) and (-0.05, 0.05). Net income is defined as in Table 9. We then compare the fraction of public firms reporting positive income rather than losses to the corresponding fraction of private firms. Panels A and B present tests of the null hypothesis that the fractions are equal, in each of the two intervals. Panel C reports placebo tests, which test for differences in the fractions of public and private firms reporting earnings above six arbitrary thresholds away from zero, namely -0.3, -0.2, -0.1, 0.1, 0.2, and 0.3. The *Z*-statistics test the null hypothesis that the populations of public and private firm-years with reported earnings around the threshold (zero or placebo) have the same proportion of observations above the threshold, assuming independent sampling. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

Panel A: Net income / assets in (-0.10, 0.10) interval

	Public firm-years		Private firm-years		Difference in	
	# observations	fraction	# observations	fraction	fractions	*Z*-statistic
Net income > 0	8,690	0.753	27,934	0.704	0.049	10.268***
Net income < 0	2,847	0.247	11,733	0.296		
Total	11,537		39,667			

Panel B: Net income / assets in (-0.05, 0.05) interval

	Public firm-years		Private firm-years		Difference in	
	# observations	fraction	# observations	fraction	fractions	*Z*-statistic
Net income > 0	4,567	0.710	16,506	0.677	0.033	5.074***
Net income < 0	1,868	0.290	7,888	0.323		
Total	6,435		24,394			

Panel C: Placebo tests

	Difference in fraction of (net income / assets) in upper half of interval between public and private firms						
	(-0.35, -0.25)	(-0.25, -0.15)	(-0.15, -0.05)	(-0.05, 0.05)	(0.05, 0.15)	(0.15, 0.25)	(0.25, 0.35)
Z-statistic	-2.690***	-2.513**	-3.455***	5.074***	-8.984***	-7.148***	-2.712***