Does the Stock Market Harm Investment Incentives? * †

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Abstract

Do 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 distinguishing between underinvestment due to rational "short-termism" and overinvestment due to "empire building." Our identification strategy exploits a rich new data source on unlisted U.S. firms which are essentially agency-cost free. Listed firms invest less and are less responsive to changes in investment opportunities compared to observably similar unlisted firms, especially in industries in which stock prices are particularly sensitive to current profits. Listed firms also tend to smooth earnings growth and dividends and avoid reporting losses. These patterns are consistent with short-termism and do not appear to be due to firms endogenously choosing to be public or private: Firms that go public for reasons other than to fund investment invest like unlisted firms pre-IPO and like listed firms post-IPO. Nor do the results appear to be driven by measurement error. Our evidence suggests that the stock market distorts investment, at least for the fast-growing companies in our sample.

Key words: Corporate investment; Q theory; Short-termism; Managerial myopia; Empire building; Managerial incentives; Agency problems; Private companies; IPOs.

JEL classification: D21; G31; G32; G34.

Stock market-listed companies enjoy access to a deep pool of capital. 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 of funding investment 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 (see Berle and Means (1932) or Jensen and Meckling (1976)). As a result, managers may invest capital sub-optimally from the point of view of shareholders. The theory literature is divided on whether managers will overinvest or underinvest.

A preference for scale results in overinvestment, or 'empire building' as Baumol (1959) and Stulz (1990) call it. Underinvestment, on the other hand, occurs if investors cannot tell whether cash flows are high because the firm is doing well or because the manager has foregone positive NPV projects. Models of such 'managerial myopia' or 'short-termism' include Miller and Rock (1985), Stein (1989), Shleifer and Vishny (1990), Bebchuk and Stole (1993), von Thadden (1995), and Holmström (1999).[1]

We examine empirically whether stock market listings distort investment. We do so by comparing the investment decisions of U.S. stock market-listed (or 'public') firms to those of observably similar unlisted (or 'private') U.S. firms. 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. Public firms also tend to smooth earnings and dividends and avoid reporting negative earnings.

Based on extensions to Stein (1989) and Stulz (1990), we show that these patterns are consistent with managerial myopia but not with empire building. Stein assumes that the manager derives utility from the firm's long-term value as well as from its current stock price.[2] As a result, the manager has an incentive to 'manipulate' the stock price. He does so by underspending today which allows him to report higher cash flows. In the resulting signal-jamming equilibrium, investors aren't fooled and the manager gains nothing, but since investors rationally anticipate his behavior the manager has no incentive to deviate from it.

[1] The argument that investors cannot easily understand the sources of fluctuations in reported cash flows is widely made in finance and economics. Duffie (2010), for example, uses it to explain the growth in over-the-counter derivatives.
[2] A focus on the current stock price could either reflect optimal contracting (Garvey, Grant, and King (1999)) or be induced by the costs of arbitraging long-term mispricing (Shleifer and Vishny (1990)).

We adapt Stein's model to derive a necessary and sufficient condition for short-termism. If public-firm managers have short-termist preferences and if this agency cost outweighs the financing benefit of a stock market listing, investment will both be lower and less responsive to investment opportunities among public firms than among private ones – precisely what we find in the data. Adapting the Stulz (1990) model, we show that empire building implies higher investment and greater sensitivity to investment opportunities among public firms – the opposite of what we find in the data.

This 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 findings suggest that the agency costs of a stock market listing outweigh the benefits of a reduced cost of capital, from an investment perspective, at least for the fast-growing entrepreneurial firms in our sample.[3]

Our tests assume that private firms, carefully screened to be observably similar, are a good benchmark for how public firms would behave absent agency problems. This assumption is common in the literature. Ang, Cole, and Lin (2000, p. 83), for example, observe that "When compared to publicly traded firms, [private firms] come closest to the type of [zero-agency-cost] firms depicted in the stylized theoretical model of agency costs developed by Jensen and Meckling (1976)." This will be true as long as private firms have more concentrated ownership and less separation of ownership and control than public firms.

Evidence from the Federal Reserve's 2003 National Survey of Small Business Finances (NSSBF) supports this assumption. As Tables A7 and A8 in the Online Data Appendix shows, 94.1% of the larger private firms in the U.S. have no more than nine shareholders (most have fewer than three), and 83.2% are managed by the controlling shareholder. Thus, there is little separation of ownership and control in private firms.[4] Public-firm CEOs, by contract, typically own little equity and ownership is highly dispersed. In our sample, the average (median) public-firm CEO owns a mere 8.4% (1.6%) of his firm's equity, and the average (median) public firm has 23,772 (1,082) shareholders.

Private firms are not subject to public reporting requirements, so little is known about their investment

[3] There are of course other good reasons to go public. Besides minimizing the costs of funding investment, these include allowing shareholders to diversify or cash out and establishing a market value for the firm. See Brau and Fawcett (2006).

[4] According to Brau and Fawcett's (2006) survey evidence, keeping it that way is the main motivation for staying private in the U.S. Of the 336 survey respondents, 56% listed a "desire to maintain decision-making control" as the most important reason for staying private, followed by a desire "to avoid ownership dilution" (47%). Only 10.5% had seriously considered going public.

behavior. 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. Sageworks contains balance sheet and income statement data and so mirrors Compustat, the standard database for public U.S. firms. Our download spans the period 2002 to 2007, covering 95,370 private firms and 250,507 firm-years.

Matching Sageworks and Compustat firms on observables such as firm size and industry allows us to exploit variation along the extensive (public/private) margin. This identification approach is novel and is possible only thanks to Sageworks. Existing empirical work in this area focuses instead on the intensive margin; examples include Wurgler (2000) and Knyazeva et al. (2007) who relate investment among *public* firms to variation in proxies for agency costs and corporate governance. Our approach is distinct from, but complementary to, that taken in earlier work. By focusing on the extensive margin, we avoid the empirical difficulties faced by intensive-margin studies, such as the concern that investors may demand better governance as a firm's investment needs increase, possibly leading to a spurious relation between governance and investment. By finding evidence of agency costs along different margins, each approach reinforces the conclusions arising from the other. In addition, a focus on the extensive margin amounts to testing if agency costs are positive net of the governance remedies intensive-margin studies focus on.

We estimate standard investment equations in the tradition of tests of the Q theory of investment (Hayashi (1982)). These show that public firms are substantially less responsive to changes in investment opportunities than are private firms. Since we use matched panels of observably similar firms, there is a clear identification concern: The observed differences in investment may be driven by *un*observed differences between public and private firms. We address this concern in a separate test that exploits *within*-firm changes in investment as a firm transitions from private to public. To avoid a mechanical relation between going public and changes in investment behavior, we focus on firms that go public without raising capital. These are firms that go public purely in order to change their ownership structure. The within-firm patterns mirror those in our matched-sample tests. Firms are significantly more sensitive to investment opportunities in the five years before they go public than after. Indeed, once they are public, their investment sensitivity becomes indistinguishable from that of other already-public firms.

Our IPO experiment cannot completely remove endogeneity concerns. Unfortunately, the obvious instruments do not work in our data. In principle, one could exploit discontinuities around stock exchange listing standards to identify the effects of a stock market listing on investment. But in practice, most U.S. listing standards can be satisfied simply by going public, and the remaining standards – concerning profitability – are set so low that they would not be a binding constraint for most of our private firms.[5] Alternatively, one could use the 2002 Sarbanes-Oxley Act as an exogenous shock to the cost of being public, because compliance with its Section 404 effectively acts as a tax on being a small publicly traded firm. However, 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.[6]

Ultimately, short of a trial that randomly assigns firms to public or private status, we cannot rule out endogeneity concerns. 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.

Our paper makes three main contributions. First, we provide rare direct evidence of an important cost of a stock market listing by documenting that the public firms in our sample appear to invest suboptimally and that they do so in a manner consistent with myopia. We are not the first to show evidence of short-termism, but extant studies tend to have a narrower focus. Their upshot is that *some* public-firm managers *sometimes* act myopically, usually by taking costly actions to avoid negative earnings surprises that would otherwise lead to short-term stock price declines (Skinner and Sloan (2002)).[7] Our results are broader. We show that, for the public firms in our large sample, investment is distorted by agency costs *on average*.

Second, our results contribute to the agency literature by documenting that short-termism distorts the

[5] See http://www.nasdaq.com/about/nasdaq_listing_req_fees.pdf.
[6] Modeling delistings instead is problematic. Exploiting discontinuities around delisting thresholds has poor external validity: Firms forced to delist are usually in trouble and so are not representative of private firms in general (Bakke and Whited (2010)). Internal validity may also be poor, since delisting need not lead to more concentrated ownership.
[7] 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. Baber, Fairfield, and Haggard (1991) find that firms cut R&D spending to avoid reporting losses, and 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) finds that firms boost sales by discounting to meet earnings forecasts. Bhojraj and Libby (2005) provide related evidence from laboratory experiments. Sheen (2009) analyzes hand-collected investment data for public and private firms in the chemical industry, finding results similar to ours.

investment decisions of small fast-growing public firms. This adds to existing survey evidence of widespread short-termism in the U.S. Poterba and Summers (1995) find that managers of U.S. stock-market listed firms prefer investment projects with shorter time horizons in the belief that stock market investors fail to properly value long-term projects. Similarly, Graham, Harvey, and Rajgopal (2005, p. 3) report 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]." This is not to say that effective corporate governance cannot reduce public-firm managers' focus on short-term objectives. Tirole (2001, 2006) argues that large shareholders can actively monitor managers and fire them if necessary, while Edmans (2010) shows that large shareholders can reduce managerial myopia by trading on private information such that stock prices will reflect fundamental value rather than current cash flows. But it is an empirical question whether these mechanisms are sufficiently effective for the average entrepreneurial firm on the stock market. Our evidence suggests that they are not.

Third, we contribute to the empirical investment literature. Various papers have blamed the relatively poor empirical performance of neoclassical *Q* theory on adjustment costs, measurement error, financing frictions, etc. (see Bond and Van Reenen (2007) for a review). We find that *Q* theory works relatively well for private firms, which suggests that agency costs – assumed away in *Q* theory – may help to explain why public firms do not invest in accordance with theory.

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 firm 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. Edgerton (2010), finally, shows that public firms overuse corporate jets compared to observably similar private firms, again consistent with agency problems.

1. Modeling Investment Behavior

To guide our empirical tests, we model the effects of the two principal investment-related agency problems: Short-termism and empire building. Despite its simplicity, the model has all the features we need to discipline our empirical analysis. Proofs and technical derivations can be found in Appendix A.

1.1. Short-termism

We formalize the effect of short-termism on investment by adapting Stein (1989). Stein captures short-termism by letting the manager "borrow" cash flows from the future by underspending today relative to the optimum value-maximizing spending choice. Underspending is costly and so is detrimental to the firm's long-term value and yet, as Stein shows, it may be in the manager's interest. One example of borrowing that Stein mentions is "deciding not to invest in assets that have returns greater than *r*," the firm's one-period discount rate. In other words, a short-termist firm may forego positive NPV projects.

Consider a firm, either public or private, whose manager chooses investment, i_t. The firm's reported free cash flows[8] 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 to be common knowledge. We assume a standard functional form for the production function, $f(i) = i^{\alpha}$, with $0 < \alpha < 1$.[9] 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 the manager nor investors observe v_t and u_t, but everyone knows their distributions.

At the beginning of period *t*, the manager chooses investment without knowing e_t^n. He solves

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

if managing a private firm, while a public-firm manager solves

[8] Free cash flow equals cash flow available for distribution to shareholders and lenders, i.e., EBITDA (earnings before interest, taxes, depreciation, and amortization) less investment (capital expenditures plus change in net working capital) less taxes.
[9] 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.

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

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 includes previous cash flows, $\{e_{t-j} : j > 0\}$, and past investments, $\{i_{t-j} : j > 0\}$.

Private firms may face a higher cost of capital as their shares are relatively less liquid than those of publicly traded firms.[10] 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 the rate used by public firms, $(1+r)$.

For public firms, the second term in the utility function captures the idea that each period, the manager 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 (Shleifer and Vishny (1990), Stein (1988)).[11] For private firms, which are unlisted, our maintained assumption is that no such distortion exists as private firms have more concentrated ownership and are overwhelmingly owner-managed.

A public firm's stock price at time *t* equals the market's expected present value of the firm's future cash flows: $P_t = E_t^I \left(\sum_{j=1}^{\infty} e_{t+j} / (1+r)^j \right)$, where E_t^I denotes the expectation at time *t* conditional on the information available to investors. Investors' information set includes current and past cash flows, $\{e_{t-j} : j \geq 0\}$, and past investment, $\{i_{t-j} : j > 0\}$, but not current investment i_t.[12]

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

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

[11] Shleifer and Vishny (1990) argue that arbitrage costs increase in the time until an asset pays off, and so arbitrageurs rationally have short horizons leaving long-term assets more mispriced. Managers then avoid long-term assets as undervaluation could lead to their being fired. Holden and Lundstrum (2009) provide supporting evidence.

[12] We assume that current investment is unobservable only for convenience. As Stein (1989) shows, what has to be unobservable for the model to yield underinvestment is the amount of cash flows the manager "borrows" from the future, that is, the amount of underspending relative to optimal investment. While current investment is noisily observable in reality, optimal investment is not, and so investors cannot be sure whether current cash flows are high due to underspending or because the firm is doing well.

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

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

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 Holmström (1982) and Stein (1989).

Equation (1) can be interpreted as follows. Past investment is observable to everyone, so investors can back out the stochastic element of cash flows e_{t-k}^n in all past periods. But they do not observe current investment and so can only *infer* the current cash flow shock, $E_t^I\left[e_t^n\right]$. This enables a public-firm manager 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 hence investors' future cash flow expectations, $E_t^I\left[e_{t+j}\right]$. Specifically, we have that $E_t^I\left[e_t^n\right]=e_t-Qf\left(i_{t-1}\right)+\bar{i}$. Thus, the manager manipulates $E_t^I\left[e_t^n\right]$ by reducing current investment, i_t, below the value-maximizing level and thereby increasing current cash flows $e_t=Qf\left(i_{t-1}\right)-i_t+e_t^n$.

Obviously, for such a signal-jamming equilibrium to exist, investors' beliefs about current investment, $\bar{i}$, must be consistent with the firm's actual investment, i_t. Thus, in the resulting perfect Bayesian Nash equilibrium, investors are not fooled by the manager's behavior, and yet the perceived ability to manipulate the stock price causes a public-firm manager 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 by cutting investment; and given that investors will, therefore, assume inflation, the manager is better off actually inflating cash flows.

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-termist incentives.

1.1.1 Investment Choices

A public-firm manager chooses investment i_t^{Public} to maximize the following expression:

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

(The objective functions are written out in full in the proof of Result S1, which can be found in Appendix A.) Given the concavity of the maximand, i_t^{Public} is implicitly defined by the following necessary and sufficient first-order condition:

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

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

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

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

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

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 firms is* <u>*lower*</u> *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 more 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.[13]

However, this does not imply that going public would be a poor choice for *every* firm. The next result explores what factors make it more costly for a firm to be public, by exploring under what conditions we should expect agency costs 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, γ_0 captures the extent to which investors base their expectations of the firm's future cash flows on the inferred level of its current cash flow shock. Firms with high γ_0 are firms for which the permanent component of stochastic cash flows tends to evolve rapidly (that is, $\sigma_u^2 >> \sigma_v^2$), thereby making past cash flows less useful in forecasting future ones. Conversely, in firms with low γ_0, current stochastic cash flows are subject to a lot of transitory noise (i.e., $\sigma_v^2 >> \sigma_u^2$), so the whole series of past cash flows is important in forming accurate predictions about future cash flows; see Stein (1989) for details. Thus, 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 investment sensitivities between public and private firms. The opposite should be true for firms in industries with high γ_0.

[13] Setting $\delta = 0$ in condition (4) allows us to directly 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.

1.2. Empire Building

We follow Stein (2003) and model empire building by assuming that a public-firm manager with empire-building tendencies chooses investment $i_t^{Public,\ Empire}$ to maximize 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 investment, as in Stulz (1990). The following necessary and sufficient first-order condition implicitly defines investment $i_t^{Public,\ Empire}$:

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

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 higher 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.*

1.3 Null and Alternative Hypotheses

The purpose of modeling investment behavior formally is to give structure to our interpretation of the empirical results. The null hypothesis of our tests is that public and private firms do not differ in their investment behavior. Conceptually, empirical support for the null would permit four interpretations: 1) Neither public nor private firms suffer from agency problems affecting their investment decisions. 2) Both types of firms suffer from agency problems to the same extent. 3) Public firms enjoy a funding advantage that just offsets their greater agency costs. Or 4) agency problems among public firms are controlled effectively through governance mechanisms, such as monitoring by the board of directors or large shareholders or the threat of hostile takeovers. These four interpretations are observationally equivalent in our tests, so we do not take a stand on which might be the most empirically relevant.

If the data reject the null, the model yields two alternative hypotheses: Public firms either underinvest or overinvest, which may reflect either short-termism or empire building. As long as private firms are largely agency-cost-free, we can distinguish empirically between the two agency problems, based on differences in investment levels and investment sensitivities between public and private firms.

Results S1 and S2 are necessary and sufficient conditions for short-termism to exist and for it to outweigh the funding advantage of a stock market listing. In contrast, results E1 and E2, which make the opposite predictions compared to S1 and S2,[14] are only necessary conditions for empire building to exist. They are not sufficient because public firms could invest more and have higher investment sensitivities simply due to their funding advantage over private firms. Thus, lack of empirical support for E1 and E2 would allow us to rule out empire building, but the opposite is not true: While support for E1 and E2 would be consistent with empire building, it would be equally consistent with both no agency problems at all and with short-termism. To see why, note that support for E1 and E2 would imply that condition (4) must be violated and so $\delta > \pi \frac{1+r}{r^2}\gamma_0$. This inequality is satisfied if there is no short-termism $(\pi = 0)$ as well as if there is short-termism $(\pi > 0)$ and public firms enjoy a sufficiently large funding advantage over private firms $(\delta >> 0)$. This ambiguity underscores the need for a formal model.[15]

Result S3 applies exclusively to short-termism and not to empire building. It gets *directly* at the mechanism Stein (1989) models, namely that a concern for the current stock price induces managers to sacrifice long-term value by reducing investment. Clearly, this mechanism is only plausible in situations where stock prices are in fact sensitive to current earnings, so empirical support for Result S3 would increase our confidence in a finding that public-firm managers behave myopically.

2. Sample and Data

Our dataset combines data on public firms obtained from standard sources such as Compustat and CRSP with data on private firms obtained from a new database vendor, Sageworks Inc. Sageworks is

[14] This means we have a signed test. While we report standard two-sided tests, *p*-values are hence conservative by a factor of 2.
[15] This makes the results of a related paper hard to interpret. Using Amadeus data, Mortal and Reisel (2009) find support for our Result E2 among European firms. This could indicate short-termism, empire building, or the absence of either agency problem. Aside from interpretation problems, an important caveat is that the Amadeus database suffers from survivorship bias because the historical data of dead firms are eliminated from the database. See Popov and Roosenboom (2009) for further details.

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. The main drawback of anonymity for our purposes 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 firms from other sources.

Sageworks obtains data not from the private firms themselves, which could raise selection concerns, but from a large number of accounting firms which input data for *all* their (unlisted) corporate clients directly into Sageworks' database. Selection thus operates at the level of the accounting firm and not of the private firms themselves. The accounting firms Sageworks co-operates with include most national mid-market accounting firms (those below the 'Big Four') and hundreds of regional players, but few of the many thousand local accountants who service the smallest firms in the economy. As a result, the main selection effect is that firms in Sageworks are substantially larger than the small private businesses covered in the only other large-scale private-firm dataset, the NSSBF. This selection may be problematic depending on the application but is innocuous for our purposes since the smallest firms in the economy have no realistic chance of going public.

Sageworks started the database in 2000 with fiscal year 2001 being the first panel year. The growth of the database over time is detailed in Table A2 in the Online Data Appendix. We have data through fiscal year 2007 and use 2001 to construct lags, giving a six-year panel with more than 250,000 firm-years.

Sageworks is free of survivorship bias, as no records are ever deleted. Of course, if a firm goes public, dies, or switches to an accounting firm that doesn't co-operate with Sageworks, its data time series in Sageworks will end, but its historical data will not be removed.

2.1 Sample Construction

Full details of our sample construction can be found in Table A1 in the Online Data Appendix, along with further summary statistics describing the Sageworks database. To construct our sample of private firms, we exclude from Sageworks 10,104 Canadian firms as well as 3,930 firms with data quality problems (i.e., those violating basic accounting identities and firms with missing or negative total assets).

To be part of the public-firm sample, a firm has to be recorded in both Compustat and CRSP during our sample period; be incorporated in the U.S. and listed on a major U.S. exchange (NYSE, AMEX, or Nasdaq); 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 financial firms (SIC 6), regulated utilities (SIC 49), and government entities (SIC 9) from both the public and private samples. Since our empirical models exploit within-firm variation, we exclude firms with fewer than two years of complete data. Both the public-firm and private-firm samples cover the period from 2002 through 2007. The public-firm sample consists of 3,926 firms and 19,203 firm-years; the private-firm sample contains 32,204 firms and 88,568 firm-years.

2.2 Matching

The ideal experiment compares the investment behavior of two otherwise identical firms that differ only in their listing status. To get close to this ideal, we need to find pairs of public and private firms that are observably similar to each other. Matching is a convenient way to do so. Our preferred match is based on size and industry, the two dimensions in which the samples differ the most and which, economically, likely affect investment. Not surprisingly, Compustat firms are much larger than Sageworks firms. The top graph in Figure 1 shows the distribution of total assets in log 2000 dollars for each group of firms. The distributions overlap only to a limited extent. Table 1 shows that the mean (median) public firm has total assets of $1,364.4 million ($246.2 million), compared to $7.1 million ($1.3 million) for private firms. The industry distributions too are different; see Tables A3 and A15 in the Online Data Appendix.

Other variables also differ in the two samples, but size is by far the most important observable difference in our data. This can easily be seen in a probit model conditioned on a kitchen sink of firm characteristics and a full set of year effects. We find that one standard deviation increases in the explanatory variables have the following effects on the probability that a sample firm is public: Log total real assets: +10.6 percentage points; cash holdings: +0.52 percentage points; return on assets (ROA): -0.46 percentage points; leverage: -0.17 percentage points; and sales growth: +0.09 percentage points. The unconditional probability is 17.8%, so size is the only covariate that moves the needle at all. The pseudo-R^2 of this model is 84.1%, so there is little variation left unexplained by differences in unobserved characteristics. We thus match on size and industry, though we will consider alternatives for robustness.

In the language of the matching literature surveyed in Imbens and Wooldridge (2009), we use a nearest-neighbor match adapted to a panel setting. Starting in fiscal year 2002, for each public firm, we find the private firm that is closest in size and that operates in the same four-digit NAICS industry, requiring that the ratio of their total assets (*TA*) is less than 2 (i.e., $\max(TA_{public}, TA_{private}) / \min(TA_{public}, TA_{private}) < 2$). If no match can be found, we discard the observation and look for a new match for that firm in the following year. Once a match is formed, it is kept in subsequent years to ensure the panel structure of the data remains intact. (If a matching firm exits the panel, a new match is spliced in.) The resulting 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.[16]

How good is the match? The bottom graph in Figure 1 shows the distribution of log real assets for matched public and private firms. The overlap is near perfect and the sample moments are very close. Average total assets, for example, are $144.7 million and $120.0 million for public and private firms, respectively, and the difference is not statistically significant. The distribution of the ratio of matched pairs' total assets (our matching criterion) is centered on 1, with 461 of the 1,666 public firms matched to larger private firms and the remaining 1,205 firms matched to smaller ones. Thus, matched firms are of similar size and so have a plausible choice between being public or private. In fact, empirically, they share almost the same propensity to be public: The propensity scores (i.e., predicted probabilities) associated with our nearest-neighbor match average 0.205 for public firms and 0.193 for the private ones.

[16] Our standard errors are clustered in the usual way but do not adjust for variation introduced by the nearest-neighbor matching procedure. No such adjustment yet exists for matched panels, though subsampling is potentially a viable solution (see Abadie and Imbens (2008)). Subsampling is sensitive to the size of the subsample used and econometric theory is silent on the optimal size. For robustness, we have computed standard errors using a standard subsampling procedure (see Politis, Romano and Wolf (1999)) for different sizes, looking for one that seems robust within a reasonable local interval and that gives results similar to standard asymptotic estimates wherever a specification involves no additional variation that is unaccounted for. A subsample size of 70% satisfies these criteria. The resulting subsampled standard errors, which account for variance introduced by all first-stage procedures, support all inference presented in the paper, with the following exceptions: Table 2, column 4, the coefficient on "Investment opp. x public" has a *p*-value of 12%; Table 4, column 3, "ROA" has a *p*-value of 21%; and Table 8, Panel A, "Sales growth x ERC", "Sales growth x public x ERC", and "ERC" have *p*-values of 13%, 21%, and 23%, respectively. As noted earlier, these are two-sided tests and hence are conservative for our purposes.

2.3 Investment

Firms can grow their assets by either building new capacity or buying another firm's existing assets. These are reflected in capital expenditures (CAPEX) and mergers and acquisitions (M&A), respectively. Many studies of investment model CAPEX, but there are reasons to expect systematic differences in the relative importance of M&A and CAPEX for public and private firms. Unlike public firms, private firms usually cannot pay for their acquisitions with stock so their overall investment is likely to involve relatively more CAPEX than that of public firms. Sageworks data do not allow us to distinguish between CAPEX and M&A, so we cannot test this conjecture. But to avoid biases when we compare public and private firms' overall investment behavior, we will measure investment in a way that captures both CAPEX and M&A. This can be done by modeling investment as the annual increase in fixed assets.

Our main investment measure is *gross investment*, defined as the annual increase in gross fixed assets scaled by beginning-of-year total assets.[17] We also model *net investment*, defined analogously using net fixed assets. The 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 firm's investment decisions. (For detailed definitions of these and all other variables, see Appendix B.)

Table 1 shows that private firms invest significantly *more* than public firms on average, 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 firms. In the matched sample, the average differences are 5.6 and 7.2 percentage points in favor of private firms, respectively. The median differences are smaller, at -0.1 and 0.9 percentage points, largely because neither the median public firm nor the median private firm invests very much.

2.4 Measures of Investment Opportunities

The empirical investment literature proxies 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

[17] Another form of investment, R&D, does not change fixed assets and so is not captured by gross investment. We cannot model investment in R&D explicitly as R&D appears on a firm's statement of cash flows, which is not available in Sageworks.

(measured as the annual percentage increase in sales) 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 values for private firms, Q has to be imputed. Campello and Graham (2007) suggest imputing it by regressing Q, where available, on four variables thought to be informative about a firm's marginal product of capital (sales growth, ROA, net income before extraordinary items, and book leverage). The resulting regression coefficients are then used to generate 'predicted Q' for each public and each private firm. Cummins, Hassett, and Oliner (2006) suggest estimating a firm's intrinsic value not from market values but from financial analysts' earnings forecasts. Because analysts do not cover private firms, we impute their 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 problematic. It can only be estimated for public firms that are covered by one or more financial analysts, raising selection concerns.[18] Also, the fact that we are using medians means we are using 'a proxy for a proxy.' The likely net effect of this measurement error problem is to dampen any effect that analysts' Q has on investment decisions.

2.5 Other variables

Table 1 shows that private firms are significantly more profitable, hold significantly less cash, and have significantly more debt, even after we match on size and industry. While interesting in their own right, we will show that these differences in observable characteristics do not drive our empirical results.

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

3.1 Baseline Models

Neoclassical investment models predict that corporate investment is solely a function of investment opportunities. In Table 2, we estimate standard investment regressions of gross investment on investment opportunities.[19] 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, correlate positively with investment. A significant ROA effect is often interpreted as a

[18] See Carpenter and Guariglia (2007) for a critique of Cummins et al.'s (2006) approach and findings.
[19] As we will show later, we obtain similar results using net investment instead.

sign of financing constraints (Fazzari, Hubbard, and Petersen (1988)), though some disagree (Kaplan and Zingales (1997, 2000), Cleary (1999), or Erickson and Whited (2000)). While we are agnostic about the debate surrounding its interpretation, we follow the literature by including ROA. Finally, we remove unobserved time-invariant heterogeneity by using firm fixed effects and include 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 estimate 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.[20] Thus, the data reject the null of no differences in investment behavior between public and private firms, favoring short-termism (Result S2) over empire building (Result E2). We also find that investment is sensitive to ROA, significantly less so among public firms. This is consistent with the interpretation that public firms are less financially constrained.

The fixed-effects specification in column 1 cannot accommodate a public-firm indicator in levels (as opposed to one interacted with sales growth or ROA) since public and private status are fixed in the sample (as 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 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. This suggests that agency costs could be a major reason why neoclassical theory has been shown to work poorly in the empirical investment literature.

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.

3.2 Alternative Specifications

Table 3 considers nine alternative specifications. We begin by investigating an important potential

[20] While we report significance levels using two-sided tests throughout, recall that these are conservative by a factor of two as our two alternative hypotheses predict opposite signs for the variable of interest, making one-sided tests appropriate.

confound: Age. Jovanovic and Rousseau (2010), for example, argue that younger firms face a relatively lower cost of adopting new technologies and so are more sensitive to changes in investment opportunities. If private firms were systematically younger than public firms, this could confound our results. We cannot control for age directly since neither Compustat nor Sageworks contains data on founding years. But the following indirect tests suggest age is not a serious confound.

How old are public firms? According to Ljungqvist and Wilhelm (2003), the average (median) firm in the U.S. goes public aged 10 (6). In our matched sample, the average (median) Compustat firm has been public for 11.9 (9.3) years in 2004. This suggests that the average (median) public firm in our matched sample is around 21.9 (15.3) years old in 2004. For comparison, the average (median) NSSBF firm is 18.2 (15) years old in 2004 (see the Online Data Appendix). Based on these back-of-the-envelope calculations, it is thus not obvious that private firms are systematically younger than public ones, though of course the lack of age data in Sageworks means we cannot be sure.

Columns 1 and 2 of Table 3 investigate the potential age confound more formally. The spirit of the test is simple. If Sageworks firms are systematically younger than matched Compustat firms *and* if this confound drives our results, then we should find no difference in investment sensitivities once we restrict the Compustat sample to 'young' firms.

We code a firm as 'young' if its age (i.e., time-since-IPO) in its first panel year is less than the median in that calendar year, and as 'old' otherwise. (Note that this will not produce two equal-sized subsamples unless the mortality rates of young and old firms are exactly the same, but it keeps the panel structure intact by ensuring that each firm can only be in one of the two subsamples.) We then match public firms in each subsample to private firms based on industry and size, as before. As Table 3 shows, we find a significantly lower investment sensitivity for public firms relative to private firms in *both* subsamples. In fact, the point estimates are nearly identical in size: -0.095 for old firms and -0.096 for young firms.

Another way to look at the possible age confound is to test if investment sensitivity does change with age as the lifecycle story supposes. We can clearly only do so within the sample of public firms. Column 3 restricts the sample accordingly and interacts investment opportunities with the log of one plus the firm's time-since-IPO. The coefficient estimate is -0.002 (p=0.863) so investment sensitivity does not vary with firm age. Overall, these tests suggest that it is unlikely that public and private firms invest differently simply because they are at different points in their lifecycles.

Our private-firm sample pools sole proprietorships, limited liability companies (LLCs), partnerships and limited liability partnerships (LLPs) as well as firms incorporated under Subchapters C or S of the Internal Revenue Code. (See Table A5 in the Online Data Appendix for further details.) These legal forms are taxed differently and it is possible that taxes affect investment. Virtually all public firms are C Corps, so column 4 restricts the private firms accordingly. This leaves the coefficient of interest unchanged, with an estimate of -0.085 ($p<0.001$) for the difference in private and public firms' investment sensitivity.

Column 5 excludes firms that use cash basis (rather than accrual) accounting. This eliminates a small number of matches but leaves the coefficient of interest barely changed at -0.092 ($p<0.001$). In column 6, we model net rather than gross investment and again find that private firms are more sensitive to changes in investment opportunities than are public firms ($p<0.001$).

The last three columns test if observable differences between public and private firms that remain after we match on size and industry can account for the observed difference in investment behavior. For example, perhaps firms with greater cash holdings or lower leverage can more easily take advantage of improvements in investment opportunities. Omitting cash holdings and leverage would then bias our results, though as Table 1 shows, the effect likely goes in the other direction: Private firms actually hold less cash and are more leveraged than public firms. Column 7 adds cash holdings and leverage as additional regressors and also controls directly for firm size. While each of these additional controls is statistically significant, their inclusion does not alter the finding that public firms are significantly less responsive to changes in investment opportunities. The coefficient is -0.058 with a p-value of 0.001.

An alternative way to condition on additional observables is to include them as matching criteria. In addition to size and industry, column 8 matches on sales growth while column 9 matches on the 'kitchen sink': Industry, total assets, sales growth, ROA, cash holdings, and book leverage. As before, we adapt a nearest-neighbor propensity score match to our panel setting. The resulting matches are quite tight. In column 8, propensity scores average 25.1% for matched public firms and 24.6% for matched private firms; in column 9, mean propensity scores are 25.6% for both groups. This implies that our public and private firms are observably quite similar. In both columns, public-firm investment continues to be substantially less sensitive to changes in investment behavior, with point estimates of -0.061 ($p<0.001$) and -0.048 ($p=0.032$). Thus, our core empirical result is unlikely to be an artifact of our matching choices.

3.3 Are Private Firms Good Proxies for Zero-Agency-Cost Firms?

Our empirical strategy assumes that private firms are subject to little (if any) separation of ownership and control and that consequently their investment decisions suffer from fewer (or even no) distortionary agency costs, compared to public firms. In the introduction, we provide statistics from the Federal Reserve's NSSBF survey supporting this assumption. (Further details can be found in Table A8 in the Online Data Appendix.) But, as Figure 1 shows, NSSBF firms are substantially smaller than those in Sageworks so maybe these statistics are misleading.

Sageworks provides no ownership data. However, four legal forms strongly correlate with ownership concentration. Sole proprietorships are by definition owner-managed. For tax purposes and to gain limited liability, many sole traders choose LLC status; according to the 2003 NSSBF, 67.3% of LLCs are owner-managed. And both partnerships and LLPs give each partner the statutory right to participate in management and so are usually managed by a committee comprising all partners; in the NSSBF, around 90% of each are owner-managed. In each of these four legal forms (comprising 12% of the private firms in our Sageworks sample), there is essentially no separation of ownership and control and hence little possibility of agency problems distorting investment. The other main legal forms open to private firms – C and S Corps – can *theoretically* involve any degree of separation: C Corps can have an unlimited number of shareholders while S Corps can have up to 100.

Table 4 allows investment sensitivities among private firms to vary by legal form. Column 1 includes a set of interaction terms for each legal form, capturing differences in investment sensitivities relative to C Corps, in the full private-firm sample. The interaction terms are statistically insignificant individually and jointly. Thus, investment sensitivities among private firms are no different for sample C and S Corps, which *potentially* have dispersed ownership structures, and the other legal forms, which *almost surely* have concentrated ownership structures. Columns 2 and 3 focus on sole proprietorships, which are agency-cost free by definition. In column 2, we compare these to all other private firms, while column 3 matches each by size and industry to a private firm that is not a sole proprietorship, using our earlier matching algorithm. Columns 4 and 5 widen the definition of agency cost-free firms by comparing sole proprietorships, LLCs, partnerships, and LLPs as a group to C and S Corps, using the entire sample (column 4) or a size and industry-matched sample (column 5). Each of these specifications tells the same story: We find no significant variation in investment sensitivities within our sample of private firms, in contrast to the tremendous variation we found between public and private sample firms. Given that a non-trivial fraction of the private firms in our sample are by definition free of agency costs, this is consistent with private firms being good proxies for zero-agency-cost firms, as our empirical strategy assumes.

3.4 Differential Measurement Error

Measurement error in standard proxies for investment opportunities can lead to attenuation bias so that the estimated investment sensitivity is too low. For measurement error to drive our finding of a lower investment sensitivity among public firms, it would have to be the case that their investment opportunities are measured with relatively more error than the investment opportunities of private firms. While this seems somewhat implausible, we investigate this possibility using a standard correction for measurement error due to Arellano and Bond (1991). This is a GMM estimator in first-differences which uses lagged regressors as potentially valid instruments under mild assumptions about serial correlation in the latent variable and the innovations of the model.

Table 5 reports the results. For ease of comparison, column 1 reproduces the baseline estimates from Table 2. Columns 2 and 3 report Arellano-Bond models with regressors dated t-5 to t-3 as instruments.[21] Column 4 is a system GMM model that 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)). The specification in column 5 is dynamic and so includes first lags of all variables. Here, only variables dated t-5 and t-4 can be used as instruments, which greatly affects identification as our six-year panel is probably too short. Each specification includes year effects.

In all four GMM specifications, we find that private firms' investment behavior is more sensitive to changes in investment opportunities than that of public firms. The difference in investment sensitivity is

[21] Variables dated t-2 are mechanically correlated with lagged sales growth and so cannot be included in the instrument set.

in fact *larger* than in the baseline model shown in column 1, at around -0.2 versus -0.097. Thus, there appears to be more measurement error for private firms than for public firms, which makes intuitive sense. The difference in investment sensitivity is significant in three of the four models. The exception is the dynamic specification in column 5, perhaps because of the paucity of suitably lagged instruments in our short panel. All four models pass 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.

3.5 Natural Experiment: State Corporate Income Tax Changes

We can sidestep the need to proxy for investment opportunities altogether by exploiting a natural experiment. In the U.S., C Corps are subject to both federal and state corporate income tax. A cut in a state's tax rate reduces the user cost of capital for firms operating in that state, boosting investment, and vice versa for tax increases. Unexpected changes in state corporate income taxes are a thus a plausibly exogenous shock to investment opportunities. 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 firms headquartered in a state that passed a tax cut (tax increase) that became effective during the fiscal year in question, and zero otherwise.[22] Using data from the Tax Foundation, we identify 13 tax cuts and four tax increases in a total of 10 states (DC, IN, KY, MD, ND, NJ, NY, TN, VT, and WV) over our sample period; see Appendix C for details.[23] For example, ND cut its corporate income tax rate from 10.5% to 7% beginning in the 2004 fiscal year.

Since only C Corps are subject to the same state corporate income tax regime as public firms, we exclude sole proprietorships, LLCs, partnerships, LLPs, and S Corps from the private-firm sample. This reduces the number of private-firm-years from 88,568 to 33,072 but in light of the results in Section 3.3 is not restrictive. In total, 934 public and 998 private sample firms are affected by a tax cut, while 257 public firms and 320 private ones are affected by a tax increase. Unfortunately, we cannot use the

[22] 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.
[23] We ignore two tax changes whose net effect on firms is unclear. In 2005, Ohio phased out corporate income tax while phasing in a gross receipts tax. Similarly, in 2007, Texas replaced a 4.5% corporate income tax with a 1% tax on gross receipts.

industry-and-size-matched sample for this test because it contains only 13 'treated' private firms.

Table 6 reports the results. Column 1 shows that private firms – but not public ones – significantly increase investment spend in response to tax cuts and lower it in response to tax increases. The point estimates are quite large. All else equal, private firms on average increase investment by 1.8% of assets 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 31% increase in investment spend (0.018/0.058). For public firms, the effect of a tax change is essentially zero (0.018 – 0.019 = -0.001).

Column 2 investigates 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 or two years or that underwent a tax change one or two years ago. None of the indicators is statistically significant, suggesting that firms a) do not anticipate future tax changes in their investments, b) adjust their investment spend as soon as a tax change comes into effect, and c) keep their investment at the new level for at least the next two years.

Column 3 reports an indirect 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 indicator in the non-C Corp sample is 0.003 with a standard error of 0.006.

A possible confound in Table 6 is size. If larger firms more often operate in multiple states, their investment decisions will be less sensitive to a tax change in their home state. The reason is that states levy taxes on all corporate activities within their jurisdiction; 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 at least somewhat, we exclude large public firms. The lowest size cutoff that produces a sufficient number of public firms affected by tax changes is the 15th percentile, corresponding to total real assets of no more than $27.2 million. This group of public firms includes 157 affected by a tax cut and 45 affected by a tax increase. Their total real assets average $14.4 million, which is very close to the private C Corp average of $12.9 million. As column 4 shows, restricting the sample in this way has virtually no effect on the point estimates of the tax change variables.

3.6 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 firms in the U.S., we cannot rule out that private and public firms 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. We could then remove unobserved time-invariant heterogeneity using firm fixed effects. But going public is, of course, not a natural experiment: Most firms go public for reasons that correlate with their investment behavior – most obviously a desire to fund a planned increase in investment (see Brau and Fawcett (2006)). To mitigate this problem, we focus on firms going public *without raising capital*. These firms experience increased ownership dispersion and a separation of ownership and control, possibly leading to agency problems, but can reasonably be assumed not to have gone public to fund investment. This reduces identification concerns, but since it cannot eliminate such concerns, we 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 operations or investment plans, or to repay debt. Suitable IPOs are identified from Thomson Reuters' SDC database. Appendix D lists their names, dates, and circumstances. We collect post-IPO accounting data from Compustat and hand-collect pre-IPO accounting data from IPO prospectuses and 10-K filings available in the SEC-Edgar and Thomson Research databases. Since this sample does not 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 7 tests whether investment sensitivities change around the IPO, within a given firm. Columns 1 and 2 report own-difference results for the IPO sample. The variable of interest interacts investment opportunities with an indicator equal to one if an observation is post-IPO. Whether we measure investment as CAPEX (column 1) or the sum of CAPEX and R&D (column 2), we find that it is significantly sensitive to investment opportunities before a firm goes public and then becomes significantly less sensitive after the IPO. Thus, firms appear to 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 change 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 a control sample of size-and-industry matched public firms. While we cannot rule out that treated and control firms differ in unobserved ways, the results continue to tell the same story: Before they go public, IPO firms are significantly more sensitive to investment opportunities; and once they are public, their investment sensitivity is not only significantly lower but in fact indistinguishable from that of other, observably similar, public firms.

3.7 Discussion

The results of the four separate identification strategies reported in this section – within-firm, Arellano-Bond, the tax experiment, and the IPO approach – all paint the same picture: On average, stock market-listed firms are significantly and substantially less responsive to changes in their investment opportunities. This is contrary to the null that public and private firms invest in the same way and so suggests that public firms are subject to agency problems that cannot be controlled effectively through the usual governance mechanisms. The patterns we document are consistent with Result S2 and contrary to Result E2. Thus, public firms behave in ways that suggest they suffer from managerial myopia rather than engaging in empire building, at least in our sample of fast-growing, entrepreneurial firms.

4. Validating Short-termism: Cross-industry Variation in Myopic Investment Behavior

Result S3 states that 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 myopia among public firms. 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, the difference in investment sensitivity between private and public firms should increase in ERC.

Since our sample contains unlisted firms, we implement this test using ERC estimates that are estimated at the industry-year level. Each year between 2001 and 2006, we regress one plus a firm's stock return over the fiscal year on a constant and the firm's earnings per share. We use the full sample of public firms and allow the slope coefficient to vary by industry (see Dechow, Hutton, and Sloan (1999) for a similar approach), using Fama and French's (1997) classification of 30 industry groups (results are robust to using their 38- or 49-industry groupings instead). The slope coefficients (one for each industry and year) provide an estimate of each industry's ERC at the beginning of the year, which we then interact with investment sensitivities in our empirical investment models.

Panel A of Table 8 reports the triple-difference estimation results using our matched sample of private and public firms. The triple interaction is negative and significant, as Result S3 predicts, suggesting that the difference in investment sensitivities 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 firms are similarly responsive to changes in investment opportunities as their private-firm 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 expected investment sensitivities to increase in ERC for public but not for private firms. While the difference between the two behaves as predicted, the levels do not: Public firms' investment sensitivity increases only marginally as we increase ERC from the 25^{th} to the 75^{th} 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 have good investment opportunities that are not fully captured by our proxy for investment opportunities.[24] Absent short-termism, we would then expect both public and private firms to exhibit greater investment sensitivity as ERC increases. The fact that only private firms do so is, therefore, consistent with short-termism.

Regardless of what the confound is, we stress that it can be differenced out using the triple-difference structure. The fact that the difference in investment sensitivities between private and public firms increases in ERC thus supports Result S3. That in turn reinforces our conclusion that public firms exhibit short-termist tendencies, for S3 should hold neither under the null nor under empire building. Moreover, it contradicts reasonable challenges to our identification strategy, such as claims that private firms respond more strongly to investment opportunities not because they are agency-cost free, but because they are capital-inefficient, inexperienced at making investment decisions, or closet empire-builders. None of these claims can reasonably generate a correlation between γ_0 and investment sensitivities.

Our evidence thus suggests that the agency costs of a separation of ownership and control begin to outweigh the funding cost advantage of a stock market listing as ERC (i.e., γ_0) increases across industries. If shareholders understand this, we expect fewer firms to be public in high-ERC industries than in low-ERC industries. Panel C tests this prediction by regressing the share of public firms (by sales in column 1 or by number of firms in columns 2 and 3) in each Fama-French industry in 2007 on the industry's ERC and some controls. The coefficients estimated for ERC are reliably negative in all three specifications, as predicted, and the economic effects are large: A one-standard deviation increase in ERC is associated with a reduction in the fraction of public firms (relative to the respective unconditional mean) of 24.1% in column 1, 29.2% in column 2, and 23.7% in column 3. These results are consistent with the interpretation that investors view short-termism as a cost of being publicly listed.

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[24] 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, Payout Policy, and Accounting Losses

Our evidence clearly favors 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).[25] 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 firm's profitability remains sound, as in Miller and Rock (1985).[26] In this section, we test whether public firms 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 net income before extraordinary items or operating income after depreciation. The unit of observation in this test is a firm rather than a firm-year. Similarly, we use time-series variation in the payouts paid by each firm to its shareholders to measure the smoothness of its payout policy.

Table 9 reports the results for our matched sample. The covariate of interest is an indicator variable equal to one for public firms. We control for firm size since, all else equal, larger firms have more volatile profit growth and payout levels. In the two profit growth regressions, we also control for whether a firm reported losses 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 payout regression, we control for whether the firm does not pay dividends during its time in our sample, since 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 significant coefficient. Thus, public firms appear to have both smoother profit growth and smoother 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

[25] This differs from earnings management through accounting 'tricks.' These have no effect on the firm's cash flows.
[26] 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 test this mechanism.

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 actively taking measures to avoid reporting negative earnings.

6. Conclusions

Our aim in this paper is to examine whether the stock market distorts investment decisions. 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. This 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) and Stulz (1990) 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, which the data fail to reject, is that the agency problems that public firms are subject to are essentially absent among private ones, which, however, may face a higher funding cost.

Matching Sageworks data for private firms to Compustat data for stock-market listed firms on size and industry, we identify matched panels of (large) private firms and (small) public firms and then estimate standard investment equations. Our null hypothesis is that public and private firms do not differ in their investment behavior. This null would hold if neither public nor private firms suffered from agency problems affecting their investment decisions; if both suffered from agency problems to the same extent; or if only public firms suffered from agency problems but these problems were either offset by their funding advantage or mitigated by effective governance mechanisms such as an active board of directors.

Our results show that compared to private firms, public firms invest 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 differences do not appear to be due to firms endogenously choosing to be public or private: Investment sensitivities among private firms that go public for reasons other than to fund investment are significantly higher pre-IPO and converge on those of observably similar public firms post-IPO. Nor do the results appear to be driven by measurement error or lifecycle differences.

Our findings are inconsistent with the null. They are also contrary to what one would expect if the dominant agency problem in the stock market were empire building. Instead, they are consistent with the interpretation that public firms' investment decisions are distorted by managerial short-termism arising from agency costs associated with a separation of ownership and control. This distortion appears to be large enough to outweigh the benefit of cheaper funding via the stock market so that, on average, public firms in our sample invest suboptimally relative to observably similar private firms. Managers and investors appear to realize this, in the sense that there are fewer public firms in industries in which the distortion is expected to be particularly severe.

Finally, we show that public firms also tend to smooth their earnings growth and their payouts to shareholders and are reluctant to report negative earnings. These patterns might suggest 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 funding their investment plans out of their cash flows.

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Appendix A. Proofs

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

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

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

For public firms, the objective function is

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

The C terms collect future cash flows that do not contain any terms that are a function of current investment, and $\overline{f\left(i\right)}=E_t^I\left[f\left(i_{t+j-1}\right)\right]$ for all $j>0$. The first-order conditions characterizing the optimal levels of investment for private and public firms are easily derived.

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

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

Analogously, for public firms we have that

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

Therefore, we have the following:

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

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 that

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

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-building 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 $B(i) = \theta Qf(i)$, 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

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

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

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

Appendix B. Variable Definitions

Total assets is Compustat item *at* or its Sageworks equivalent. It is reported 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 the annual increase in net fixed assets (Compustat item *ppent* or its Sageworks equivalent).

Investment (with R&D) is capital expenditures plus R&D expenditures (Compustat items *capx + xrd*) scaled by beginning-of-year total assets (Compustat item *at*).

Investment (no R&D) is capital expenditures (Compustat item *capx*) scaled by beginning-of-year total assets (Compustat item *at*).

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, *at*) on the firm's sales growth, return on assets (ROA, defined as operating income before depreciation scaled by beginning-of-year total assets), 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 Thomson Financial's I/B/E/S database, 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*).

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), scaled by beginning-of-year total assets.

Book leverage is beginning-of-year long-term and short-term debt (Compustat items *dltt + dlc* or their Sageworks equivalents), scaled by beginning-of-year total assets.

Net income before extraordinary items is Compustat item *ib* or its Sageworks equivalent.

Operating income after depreciation is Compustat items *oibdp – dp* or their Sageworks equivalents.

Payouts paid by a firm to its shareholders is Compustat item *dvc* or its Sageworks equivalent.

All variables (except predicted *Q* and analysts' *Q*) are winsorized 0.5% in each tail to reduce the impact of outliers.

Appendix C. List of State Corporate Income Tax Changes

This table lists the state corporate income tax changes that we use for the analysis in Table 6. We limit our attention to state corporate income tax changes that occurred during our sample period (2002-2007) and that can unambiguously be categorized as either a tax increase or a tax decrease. Thus, we exclude two tax changes, in Ohio and Texas, whose net effects on investment incentives are unclear. These are listed below under "Ambiguous tax changes". 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	Brief description of tax change
Tax increases:		
DC	2004	Corporate income tax rate increased from 9.5% to 9.975%
MD	2007	Corporate income tax rate increased from 7% to 8.25%
NJ	2003	Introduction of an Alternative Minimum Assessment tax based on gross receipts, which applies if it exceeds the corporate franchise tax
TN	2004	Corporate income tax rate increased from 6% to 6.5%
Tax cuts:		
DC	2002	Corporate income tax rate cut from 9.975% to 9.5%
IN	2004	Corporate tax rate increased from 3.4% to 8.5% 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
KY	2005	Corporate income tax rate cut from 8.25% to 7%
KY	2007	Corporate income tax rate cut from 7% to 6%
ND	2004	Corporate income tax rate cut from 10.5% to 7%
ND	2007	Corporate income tax rate cut from 7% to 6.5%
NJ	2002	Corporate income tax rate cut from 9% to 8.5%
NY	2002	Corporate income tax rate cut from 8% to 7.5%
NY	2005	Corporate income tax rate cut for small businesses from 6.85% to 6.5%
NY	2007	Corporate income tax rate cut from 7.5% to 7.1%
VT	2006	Corporate income tax rate cut from 9.75% to 8.9%
VT	2007	Corporate income tax rate cut from 8.9% to 8.5%
WV	2007	Corporate income tax rate cut from 9% to 8.75%
Ambiguous tax changes (excluded from the analysis):		
OH	2005	Phase out corporate tax, phase in gross receipts tax, over period 2005 to 2010
TX	2007	Replace 4.5% tax on net taxable earned surplus with a 1% gross receipts tax

Appendix D. List of IPO firms

The sample used in Table 7 consists of 90 U.S. firms that went public on the NYSE, AMEX, or Nasdaq exchanges between 1990 and 2007 for the sole purpose of allowing existing shareholders to cash out, as opposed to raising equity to fund the firm'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 firm). 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 6), regulated utilities (SIC 49), government entities (SIC 9), and firms with CRSP share codes greater than 11 (such as mutual funds).

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

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

Figure 1. Size Distribution of Public and Private Sample Firms.

The top graph shows the size distribution of the public and private firms in our full samples of Compustat and Sageworks firms along with the size distribution of private U.S. firms in the Federal Reserve's 2003 National Survey of Small Business Finances. The NSSBF is a survey of 4,240 small U.S. businesses which were interviewed between June and December 2004. The Federal Reserve supplies sampling weights to construct a nationally representative sample, and the top graph uses the resulting weighted sample. (We exclude 72 NSSBF firms with zero total assets and three with negative total assets.) The bottom graph shows the size distribution of the public and private firms in our matched sample. The graphs present, for each set of firms, Epanechnikov kernel densities of the natural logarithm of total assets in $ millions of 2000 purchasing power. The width of the kernel density window around each point is set to 0.4. The unit of observation in the top graph is a firm (the NSSBF is a single cross-section; for public and private firms, we use the firm's first panel year). The unit of observation in the bottom graph is a firm-year, to illustrate the closeness of the matched panels.

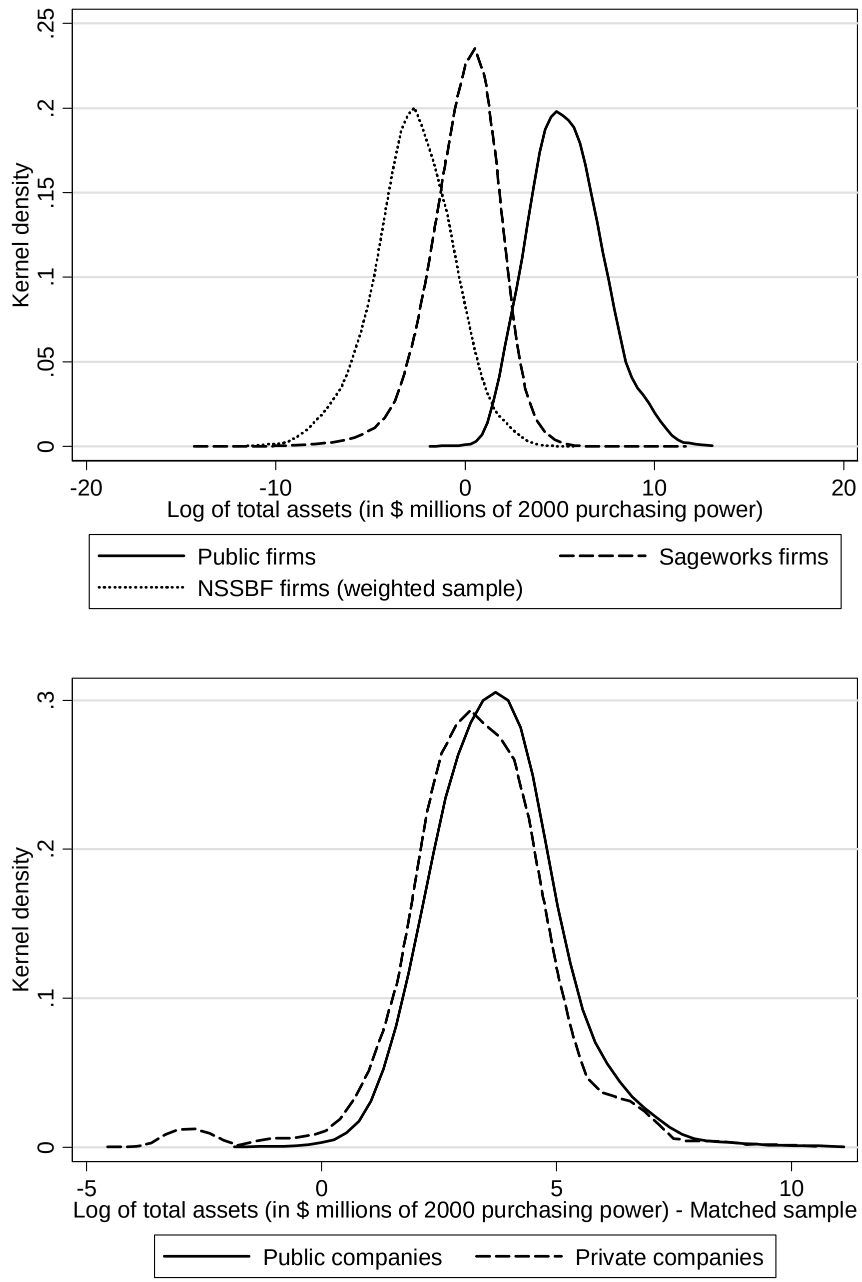

Table 1. Descriptive Statistics.

This table presents descriptive statistics for the full samples of public and private firms and for a size-and-industry matched sample over the period from 2002 to 2007. See Sections 2.1 for a description of how we construct the full samples from Compustat and Sageworks data and Section 2.2 for details of the matching procedure. The table reports means, medians, and standard deviations of the key variables used in our empirical analysis as well as pairwise differences in means and medians, 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% and 5% level, respectively. For variable definitions and details of their construction, see Appendix B.

		Full sample			Matched sample		
		Public firms	Private firms	Differences in means or medians	Public firms	Private firms	Differences in means or medians
Firm size							
Total assets ($m)	mean	1,364.4	7.1	1,357.3***	144.7	120.0	24.7
	median	246.2	1.3	245.0***	40.3	28.0	12.3***
	st.dev.	2,958.1	190.2		692.8	675.5	
Investment spending							
Gross investment	mean	0.045	0.076	-0.031***	0.040	0.097	-0.056***
	median	0.023	0.017	0.005***	0.017	0.016	0.001
	st.dev.	0.154	0.261		0.191	0.304	
Net investment	mean	0.022	0.033	-0.011***	0.022	0.094	-0.072***
	median	0.002	0.000	0.002***	0.000	0.009	-0.009***
	st.dev.	0.123	0.205		0.150	0.302	
Investment opportunities							
Sales growth	mean	0.183	0.177	0.006	0.256	0.327	-0.071***
	median	0.087	0.070	0.016***	0.091	0.111	-0.020***
	st.dev.	0.674	0.652		0.925	1.075	
Predicted Q	mean	1.817	1.473	0.344***	2.119	1.964	0.155***
	median	1.778	1.385	0.392***	2.047	1.889	0.158***
	st.dev.	0.663	1.082		0.774	1.229	
Analysts' Q	mean	1.136	1.004	0.133***	1.188	1.188	0.000
	median	1.103	0.941	0.162***	1.164	1.164	0.000
	st.dev.	0.478	0.447		0.396	0.396	
Firm characteristics							
ROA	mean	0.065	0.075	-0.010**	-0.060	0.084	-0.144***
	median	0.111	0.095	0.016***	0.051	0.123	-0.072***
	st.dev.	0.286	1.069		0.437	0.986	
Cash holdings	mean	0.225	0.152	0.073***	0.304	0.151	0.152***
	median	0.131	0.073	0.058***	0.228	0.074	0.154***
	st.dev.	0.239	0.202		0.267	0.200	
Book leverage	mean	0.199	0.311	-0.111***	0.149	0.218	-0.069***
	median	0.145	0.157	-0.012***	0.055	0.132	-0.077***
	st.dev.	0.230	0.455		0.250	0.264	
No. of observations		19,203	88,568		4,975	4,975	
No. of firms		3,926	32,204		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 scaled by beginning-of-year total assets). We obtain similar results using net investment (the scaled increase in net fixed assets); see column 5 in 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 for four firms 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 for which financial analysts make earnings forecasts according to Thomson Financial's I/B/E/S database (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, all specifications include ROA. This is sometimes interpreted as a possible proxy for financing constraints. All regressions include firm fixed effects. Since the sample contains no firms that transition from public to private status or vice versa, inclusion of firm fixed effects implies that we cannot identify differences in investment *levels* between public and private firms. All regressions use the matched sample; see Section 2.2 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. For variable definitions and details of their construction, see Appendix B. Each regression includes a firm-specific intercept and year effects; their coefficients 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 firm pairs as resampling clusters and perform 500 replications. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively. Since our two alternative hypotheses predict different signs for the interaction term involving investment opportunities and public firms, these *p*-values are conservative by a factor of 2.

	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.

As in Table 2, we proxy for investment opportunities using sales growth and exploit within-firm variation using least squares with firm and year fixed effects. Columns 1-3 investigate lifecycle stories of investment. Column 1 restricts the sample of public firms to 'old' firms (those whose time-since-IPO in their first year in our panel exceeds the median time-since-IPO of all public firms in the same calendar year), while column 2 restricts the sample of public firms to 'young' firms. Age for private firms is not available in Sageworks, so we continue to match on size and industry but not on age. Column 3 excludes private firms altogether and tests if the investment sensitivity of public firms depends on their age by interacting investment opportunities with log time since IPO. Column 4 restricts the sample to C Corps in order to hold tax regime constant between public and private firms. Column 5 restricts the sample to firms using accrual-basis rather than cash accounting. In column 6, 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 7, we test whether the results presented in Table 2, column 1 are robust to observable heterogeneity in cash holdings, book leverage, and firm size. In columns 8 and 9, we use different matching criteria to generate the estimation sample. Column 8 matches on sales growth in addition to total assets and industry while column 9 matches on the 'kitchen sink': Industry, total assets, sales growth, ROA, cash holdings, and book leverage. In both columns, we use a nearest-neighbor propensity score match with a 5% caliper. For variable definitions and details of their construction, see Appendix B. Each regression includes a firm-specific intercept and year 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. Since our two alternative hypotheses predict different signs for the interaction term involving investment opportunities and public firms, these *p*-values are conservative by a factor of 2.

	Dependent variable: Investment / lagged total assets								
	Lifecycle effects							Alternative matches	
	Old firms	Young firms	Matched public firms only	Only C Corps	Only accrual basis accounting	Net rather than gross investment	Additional controls	Industry, size, and sales growth	Kitchen sink
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Investment opportunities	0.149***	0.131***	0.042*	0.121***	0.131***	0.210***	0.092***	0.098***	0.081***
	0.016	*0.013*	*0.023*	*0.013*	*0.021*	*0.016*	*0.020*	*0.020*	*0.022*
… x public	-0.095***	-0.096***		-0.085***	-0.092***	-0.175***	-0.058***	-0.061***	-0.048**
	0.028	*0.016*		*0.017*	*0.022*	*0.017*	*0.022*	*0.021*	*0.023*
… x *ln*(1 + years since IPO)			-0.002						
			0.011						
ROA	0.189***	0.169***	-0.134*	0.159***	0.166***	-0.006	0.174***	0.124***	0.102***
	0.018	*0.015*	*0.077*	*0.015*	*0.025*	*0.019*	*0.012*	*0.025*	*0.021*
… x public	-0.121**	-0.137***		-0.114***	-0.128***	0.007	-0.118***	-0.076**	-0.041
	0.052	*0.030*		*0.032*	*0.034*	*0.028*	*0.030*	*0.036*	*0.034*
… x *ln*(1 + years since IPO)			0.076**						
			0.032						
Cash holdings							0.116*		
							0.065		
Book leverage							-0.157**		
							0.062		
Size (*ln*(total assets))							-0.055***		
							0.017		
R^2 (within)	26.9 %	31.1 %	6.2 %	34.0 %	19.3 %	50.0 %	32.4 %	18.1 %	11.7 %
Wald test: all coeff. = 0 (*F*)	26.6***	24.7***	5.0***	15.1***	11.0***	27.3***	80.1***	7.8***	8.6***
No. observations	4,028	5,922	4,975	8,154	9,822	9,950	9,931	14,546	14,826
No. firms	984	1,498	1,666	1,913	2,250	2,286	2,282	4,213	4,427

Table 4. Investment Sensitivities by Legal Form.

Our aim in this table is to validate our working assumption that private firms are subject to less (or even no) separation of ownership and control and that consequently their investment decisions suffer from fewer (or even no) distortionary agency costs. The key to this validation test is the insight that some legal forms strongly correlate with high ownership concentration. Sole proprietorships are by definition owner-managed. For tax purposes and to gain limited liability, many sole traders choose LLC (limited liability company) status. And both partnerships and limited liability partnerships (LLPs) give each partner the statutory right to participate in management and are typically managed by a committee consisting of all partners. In each of these legal forms, there is essentially no separation of ownership and control. The other two legal forms open to private firms – C Corps and S Corps – can *theoretically* involve any degree of separation: C Corps can have an unlimited number of shareholders while S Corps can have up to 100. While prior empirical evidence suggests that private firms incorporated under subcharters C and S of the Internal Revenue Code in fact have highly concentrated ownership, we test explicitly for differences in investment sensitivities between C and S Corps and the other types of private firms in our sample. If the private C and S Corps in our sample were to suffer from agency costs due to dispersed ownership, contrary to our working assumption, their investment behavior should be systematically different from that of the other private sample firms. Column 1 includes all private sample firms and allows investment sensitivities to vary by legal form. The null is that the investment sensitivities do not differ by legal form, which we test with a Wald test. The uninteracted effect in column 1 captures the investment sensitivity of C Corps (together with 702 firms of unknown legal origin; dropping these has no bearing on the results). Columns 2 and 3 focus on sole proprietorships. In column 2, we compare the investment behavior of sole proprietorships to that of all other private firms, while in column 3 we match each sole proprietorship by size and industry to a private firm that is not a sole proprietorship, using the same matching algorithm described in Table 1. In columns 4 and 5, we group sole proprietorships with LLCs, partnerships, and LLPs and compare this group to C and S Corps, using either the entire sample (column 4) or a size and industry-matched sample (column 5). Each regression includes a firm-specific intercept and year effects (not reported) and is estimated using least-squares. 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				
		Sole proprietorships		Sole prop. + LLC + partnership + LLP	
	All private firms	vs. all other private firms	matched to similar private firms	vs. all other private firms	matched to similar private firms
	(1)	(2)	(3)	(4)	(5)
Investment opportunities	0.057***	0.054***	0.106***	0.054***	0.073***
	0.007	*0.004*	*0.036*	*0.004*	*0.018*
x sole proprietorship	-0.017	-0.020	-0.065		
	0.041	*0.043*	*0.057*		
x LLC	-0.003				
	0.013				
x partnership	-0.013				
	0.016				
x LLP	-0.035				
	0.024				
x S Corp	-0.003				
	0.009				
x (sole prop.+LLC+partnership+LLP)				-0.005	-0.026
				0.010	*0.019*
ROA	0.034***	0.033***	0.078**	0.034***	0.050***
	0.005	*0.005*	*0.032*	*0.005*	*0.018*
x sole proprietorship		0.023	-0.024		
		0.028	*0.043*		
x (sole prop.+LLC+partnership+LLP)				-0.005	-0.023
				0.015	*0.023*
R^2 (within)	3.2 %	3.2 %	6.0 %	3.2 %	3.7 %
Wald test: all coeff. = 0 (*F*)	29.4***	39.2***	4.6***	39.0***	10.6***
F test: inv. opp. interaction coefficients = 0	0.54	n.a.	n.a.	n.a.	n.a.
No. observations	88,568	88,568	2,530	88,568	19,244
No. firms	32,204	32,204	1,168	32,204	8,058

Table 5. GMM Estimates of Public and Private Firms' Investment Sensitivities.

This table explores the robustness of the Table 2 results to 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 1 in Table 2 as a baseline. Columns 2 to 5 report the GMM results. In columns 2 and 3, we estimate two static GMM models. The first uses investment and sales growth dated t-5 to t-3 and year effects as instruments while the second as ROA dated t-5 to t-3. 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. Column 4 shows 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 firms and so to identify differences in investment *levels* between public and private firms. The specification in column 5 is dynamic and thus includes first lags of all variables; however, for brevity, we suppress the coefficient estimates for all lags except for lagged investment. In the dynamic specification, only variables dated t-5 and t-4 can be used as instruments, which greatly affects identification as our panel is relatively short. For variable definitions and details of their construction, see Appendix B. Each regression includes an intercept and year effects (not reported). For the GMM models in columns 2 to 5, we report 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. Since our two alternative hypotheses predict different signs for the interaction term involving investment opportunities and public firms, these p-values are conservative by a factor of 2.

	Dependent variable: Gross investment / lagged total assets				
	Within-groups (fixed effects) (1)	First diff. GMM, static (2)	First diff. GMM, static (3)	System GMM, static (4)	First diff. GMM, dynamic (5)
Investment opportunities	0.136***	0.182*	0.181**	0.220*	0.294
	0.013	*0.098*	*0.082*	*0.113*	*0.355*
Investment opp. x public	-0.097***	-0.244**	-0.187**	-0.219*	-0.292
	0.015	*0.118*	*0.091*	*0.132*	*0.357*
ROA	0.173***	0.171	0.205	0.059	0.163
	0.014	*0.383*	*0.307*	*0.304*	*0.318*
ROA x public	-0.135***	-0.108	-0.229	0.119	-0.160
	0.027	*0.343*	*0.305*	*0.314*	*0.332*
Public				0.031	
				0.053	
Investment lagged					-0.118
					0.302
Instrument set		*Inv. (3-5) Sales growth (3-5)*	*Inv. (3-5) Sales growth (3-5) ROA (3-5)*	*Inv. (3-5) Sales growth (3-5) Public Levels eq.*	*Inv. (4-5) Sales growth (4-5) ROA (4-5)*
Hansen test of overidentifying restrictions (p)		0.894	0.877	0.485	0.822
Arellano-Bond test: AR(3) (p)		0.913	0.933	0.552	0.965
No. observations	9,950	7,474	7,474	9,950	5,055
No. firms	2,286	2,217	2,217	2,286	1,773

Table 6. Public and Private Firms' Reactions To State Corporate Income Tax Changes.

In this table, we use changes in state corporate income tax rates as a plausibly exogenous shock to investment opportunities. Appendix C lists 17 tax changes in ten states that occurred over our sample period. The sample of private firms is limited to C Corps because only C Corps are subject to the same tax regime as public firms (in contrast to sole proprietorships, LLCs, partnerships, LLPs, or S Corps). However, in column 3, we focus on the private non-C Corps to validate the identification strategy. The main variable of interest in column 1 is tax change. This is an indicator variable set equal to 1 (-1) for firms headquartered in a state that passed a tax cut (tax increase) that became effective during the fiscal year in question. In column 2, we test whether tax changes were unexpected by allowing for pre- and post-trends in the tax change effect. Specifically, we add indicator variables that identify firms 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 column 4, we limit the sample of public firms to those with total real assets in the bottom 15% of the public-firm distribution (specifically, those with real assets below $27.2 million). Each regression includes a firm-specific intercept and year effects (not reported) and is estimated using least-squares. 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. Since our two alternative hypotheses predict different signs for the interaction term involving tax changes and public firms, these p-values are conservative by a factor of 2.

	Dependent variable: Gross investment / lagged total assets			
	Full sample of public firms and private C Corps		Private non-C Corps	Bottom 15% of public firms, all private C Corps
	(1)	(2)	(3)	(4)
Tax change (decrease = 1, increase = -1)	0.018***	0.021**	0.003	0.018***
	0.007	*0.009*	*0.006*	*0.007*
Tax change x public	-0.019***	-0.019***		-0.022**
	0.007	*0.007*		*0.010*
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**		-0.027**
	0.009	*0.009*		*0.012*
ROA	0.028***	0.028***	0.036***	0.028***
	0.009	*0.009*	*0.006*	*0.009*
ROA x public	0.027	0.027		-0.020
	0.028	*0.028*		*0.039*
Tax change (t-2)		0.004		
		0.008		
Tax change (t-1)		0.005		
		0.008		
Tax change (t+1)		0.001		
		0.007		
Tax change (t+2)		0.001		
		0.006		
R^2 (within)	3.3 %	3.3 %	3.4 %	3.1 %
Wald test: all coefficients = 0 (F)	13.1***	9.7***	34.8***	8.9***
No. observations	52,275	52,275	55,496	35,785
No. firms	15,682	15,682	20,448	12,345

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 firms 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. 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 a matched control sample of public firms. To be eligible for matching, a public firm must be in both Compustat and CRSP; be incorporated in the U.S. and listed on the NYSE, AMEX, or Nasdaq exchanges; have valid stock price data in CRSP; and have a CRSP share code no greater than 11. Each IPO firm is matched in its first sample year to up to five public firms 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 difference-in-difference tests allow 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 firms, while the interaction terms test whether IPO firms 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 firms by including a post-IPO indicator. (Note that the presence of firm fixed effects rules out simultaneous inclusion of a pre-IPO indicator.) For variable definitions and details of their construction, see Appendix B. Each regression includes a firm-specific intercept and year effects (not reported for brevity) and is estimated using least-squares. 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. Since our two alternative hypotheses predict different signs for the interaction terms involving investment opportunities, pre-IPO, and post-IPO, these *p*-values are conservative by a factor of 2.

	Dependent variable: Investment / lagged total assets			
	Own difference		Diff-in-diff with matched controls	
	investment (no R&D)	investment (with R&D)	investment (no R&D)	investment (with R&D)
	(1)	(2)	(3)	(4)
Investment opportunities	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, 3, and 5 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 firms 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 firms (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 a firm-specific intercept and year effects (not reported) and is estimated using least-squares. 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 25^{th} and 75^{th} percentile of the ERC distribution within the matched sample (0.217 and 0.929, respectively). In Panel C, we test the follow-on prediction that public firms account for a smaller share of activity in an industry the higher is the industry's ERC. The unit of observation is a Fama-French industry. Excluding finance and utilities leaves 28 observations. Estimation in column 1 uses OLS. The dependent variable in column 1 is the share of revenue produced by public firms in the industry. The denominator (total revenue in the industry by both public and private firms) comes from the Statistics of U.S. Businesses provided by the Census Bureau. Data on revenue ("total receipts") are available only in years ending in 2 and 7; we use data for 2007. Ideally, the numerator should capture revenue generated in a given industry in establishments owned by public firms located in the U.S. However, this information is not readily available in the Census. Instead, we sum the revenues reported in Compustat-CRSP during fiscal year 2007 for all firms located in the U.S. that belong to a particular Fama-French industry. We require that the firms are located in the U.S. (excluding Puerto Rico and the U.S. Virgin Islands), are listed on a major exchange, have a price quote in CRSP, have a CRSP share code greater than 11, and report positive sales. The numerator will be estimated with error (in fact, it will be overestimated) since unlike the Census data, Compustat's revenue measure includes revenue from products or services the company produced abroad (e.g. in China). Thus, the ratio modelled in column 1 can be greater than one. In columns 2 and 3, the dependent variable is instead the ratio of the number of public firms in an industry, rather than their revenue. This ignores size and so can produce meaningless results (e.g., if a public firm produces 99% of the output in an industry but there are thousands of tiny private firms, the industry would still be classified as being dominated by private firms). Chod and Lyandes (2010) suggest limiting the denominator (the total number of firms) to firms of a certain size (e.g., in terms of employment). We follow Chod and Lyandes and report results for firms with at least 500 employees (column 2) or at least 100 employees (column 3). The dependent variables here are well behaved fractions, so we estimate standard fractional logits (results are similar using OLS). Heteroskedasticity-consistent standard errors (clustered at the firm level in Panels A and B) are shown in italics. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively. Since our hypothesis predicts a negative sign for the interaction term involving investment opportunities, public firms, and ERC, these *p*-values are conservative by a factor of 2 in Panels A and B.

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*

Panel C: Public firms' industry shares and ERC

	Fraction of firms in a Fama-French industry that are public, by …		
	sales	no. of firms	
		with 500 or more employees	with 100 or more employees
	(1)	(2)	(3)
ERC	-0.355**	-0.567**	-0.416*
	0.145	*0.277*	*0.254*
median size	0.461***	0.505*	0.310
	0.153	*0.298*	*0.275*
median leverage	-3.965*	-2.147	-0.301
	2.156	*4.640*	*5.060*
median market-to-book	0.405	1.327*	1.134
	0.346	*0.805*	*0.749*
R^2	26.6%	22.1%	17.1%
No. of industries	28	28	28

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 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. For variable definitions and details of their construction, see Appendix B. 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 and in operating income after depreciation, respectively. The covariate of interest is an indicator variable set equal to one for public firms. We control for firm size since, all else equal, larger firms have more volatile profit growth and payout levels. We measure firm size as the within-firm time-series mean of total assets. We also control for whether a firm reported losses 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. 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 before extraordinary items	Growth in operating income after depreciation	Payouts
	(1)	(2)	(3)
=1 if public firm	-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). (For variable definitions and details of their construction, see Appendix B.) 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	27,928	0.704	0.049	10.270***
Net income < 0	2,847	0.247	11,731	0.296		
Total	11,537		39,659			

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	16,502	0.677	0.033	5.073***
Net income < 0	1,868	0.290	7,886	0.323		
Total	6,435		24,388			

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***	5.073***	-8.981***	-7.148***	-2.709***