Does the Stock Market Distort Investment Incentives? [*] [†]

John Asker
Stern School of Business
New York University
and *NBER*

Joan Farre-Mensa
Department of Economics
New York University

Alexander Ljungqvist
Stern School of Business
New York University
ECGI and *CEPR*

March 13, 2011

[*] We are grateful to Sageworks Inc. for access to their database on private companies, and to Drew White and Tim Keogh of Sageworks for their help and advice regarding their data. Thanks for helpful comments and suggestions go to Mary Billings, Jesse Edgerton, Alex Edmans, Yaniv Grinstein, David Hirshleifer, Hamid Mehran, Bruce Petersen, Joshua Rauh, Michael Schill, and Stanley Zin and to various seminar audiences. We are grateful to Mary Billings for sharing her ERC data with us. Ljungqvist gratefully acknowledges generous financial support from the Ewing M. Kauffman Foundation under the Berkley-Kauffman Grant Program.

[†] Address for correspondence: New York University, Stern School of Business, Suite 9-160, 44 West Fourth Street, New York NY 10012-1126. Phone 212-998-0304. Fax 212-995-4220. e-mail al75@nyu.edu.

Does the Stock Market Distort Investment Incentives?

Abstract

Theory suggests that agency problems resulting from separation of ownership and control can cause stock market listed firms to invest suboptimally. To test whether they do, we evaluate differences in investment behavior between listed and private firms in the U.S. using a rich new data source on private firms. Listed firms invest less and are less responsive to changes in investment opportunities compared to observably similar private 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 models of managerial myopia 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 private firms pre-IPO and like public firms post-IPO. Nor do the results appear to be driven by measurement error, tax effects, or firm life-cycle effects. 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.

Economic theory provides plenty of reasons to expect that stock markets distort corporate investment. While stock markets provide investors with liquidity and opportunities for diversification, and thereby reduce firms' cost of capital (Pagano, Panetta, and Zingales (1998)), listing a firm on a stock market may have two detrimental effects. First, ownership and control must at least partially be separated, as shares are sold to outside investors who are not involved in managing the firm. This may lead to agency problems if managers' interests diverge from those of their investors (Berle and Means (1932), Jensen and Meckling (1976)). Second, liquidity enables outside shareholders to sell their stock at the first sign of trouble rather than actively monitoring management. This, in turn, weakens incentives for effective corporate governance (Bhide (1993)). Private firms, in contrast, are often owner-managed and even when they are not, are both illiquid and typically have highly concentrated ownership, which encourages shareholders to monitor management. Building on these insights, a rich theoretical literature, discussed below, explores how stock market listed firms may fail to make first-best investment decisions.

Against this background, we compare the investment behavior of stock market listed (or 'public') firms in the U.S. to that of observably similar private firms, using a novel panel dataset of private U.S. firms covering around 250,000 firm-years over the period 2002-2007. The data reveal that public firms invest substantially less than do private firms matched on size and industry: On average, public firms invest 4% of total assets a year compared to an average of 9.7% among private firms. Moreover, public firms are considerably less responsive to changes in investment opportunities than are private firms: When we estimate standard investment equations in the tradition of tests of the *Q* theory of investment (Hayashi (1982)), we find that the sensitivity of investment to investment opportunities is 3.5 times greater among private firms than among public firms.

These empirical patterns survive a battery of robustness tests. They do not appear to be driven by lifecycle or tax effects, unobserved differences in R&D spending, our proxy for investment opportunities, or which characteristics we match on. They are also robust to concerns that investment opportunities are measured with error: Both the Arellano-Bond (1991) GMM estimator and a natural experiment (in the form of a plausibly exogenous tax shock to investment opportunities) confirm that private firms are substantially more sensitive to changes in investment opportunities than are public firms. Finally, they do not appear to be driven by unobserved differences between public and private firms. To show this, we hand-collect accounting and investment data for a sample of firms that go public and test how their investment sensitivities change as they transition from private to public, thus differencing away time-invariant firm-level unobservables. We avoid a mechanical relation between going public and changes in investment behavior by focusing on firms that go public *without raising new capital*. The identifying assumption of this test is that these firms 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.

Of course, short of a trial that randomly assigns firms to public or private status, we cannot rule out endogeneity concerns. However, for our findings to be driven by endogeneity, it would need to be the case that firms stay private while the sensitivity of their (optimal) investment to investment opportunities is high, and only go public when this sensitivity becomes low. It is not easy to see why this would be the case, particularly given that our results do not appear to be driven by lifecycle effects.

Our empirical findings suggest the following trade-off. On the one hand, a stock market listing provides access to cheaper capital, all else equal. On the other hand, a listing – somehow – makes firms invest less and be less responsive to changes in investment opportunities. The question is how.

The agency literature provides three prominent explanations. First, Baumol (1959), Jensen (1986), and Stulz (1990) argue that managers have a preference for scale and therefore engage in 'empire building.' Empire builders invest regardless of the state of their investment opportunities, thus explaining the lower investment sensitivity as long as private firms are less prone to be empire builders than public ones. However, the fact that we find lower investment *levels* among public firms seems inconsistent with empire building. Second, Bertrand and Mullainathan (2003) argue the opposite, namely that managers have a preference for the 'quiet life.' When poorly monitored, managers avoid the costly effort involved in making investment decisions, leading to lower investment levels and, presumably, lower investment sensitivities. Third, models of 'managerial myopia' or 'short-termism' argue that a focus on short-term profits may distort investment decisions from the first-best. Short-termism can lead to either overinvestment (Bebchuk and Stole (1993)) or underinvestment (Miller and Rock (1985), Narayanan (1985), Stein (1989), Shleifer and Vishny (1990), von Thadden (1995), and Holmström (1999)). Overinvestment obtains when the manager has superior information. Essentially, the manager overinvests to signal the quality of his investment opportunities. As described more fully below, underinvestment obtains if investors cannot observe the manager's actions.

Both the quiet life argument and models of short-termism that predict underinvestment fit the empirical facts. Clearly, though, agency considerations are not the only difference between public and private firms. An alternative, non-agency, explanation that also fits the facts is that public firms are subject to myriad rules and regulations designed to protect investors which slow down their decision making, resulting in less timely investment decisions. To discriminate among these explanations, we explore how the difference in investment behavior between public and private firms varies with a parameter that reasonably plays a role *only* in short-termism models: The sensitivity of share prices to earnings news. We motivate the importance of this parameter in the context of Stein's (1989) model.

Short-termism models assume that the manager of a public firm derives utility from the firm's current stock price as well as from its long-term value.[1] As a result, the manager has an incentive to 'manipulate' the current stock price. Since the stock price equals the present discounted value of the firm's future cash flows, the manager tries to boost it by manipulating investors' expectations of *future* cash flows. He does so by reporting higher *current* cash flows, in the hope that investors will respond by revising up their expectations of future cash flows. The mechanism in short-termism models is not fraudulent accounting (Kedia and Philippon (2009)) but underinvestment. Stein (1989) lets the manager "borrow" cash flows from the future by "deciding not to invest in assets that have returns greater than r," the firm's cost of capital. In other words, a short-termist manager foregoes positive NPV projects.

Investors do not know the extent of underinvestment, but they understand the manager's incentives. They thus realize that high current cash flows might in fact lead to *lower* future cash flows. In response, they 'discount' the manager's report of current earnings. And yet in equilibrium, the manager will still

[1] 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)).

underinvest. The reason is akin to the prisoners' dilemma: If investors assumed no underinvestment, the manager would inflate current cash flows by cutting investment; and given that investors will, therefore, assume underinvestment, the manager is better off actually underinvesting.

A key parameter in short-termism models is the sensitivity of investors' expectations of future cash flows to current cash flows. When current cash flows are *un*informative about future cash flows, the manager cannot manipulate investors' expectations and therefore has no incentive to underinvest. In this case, we predict no difference in investment behavior between public and private firms. But the more sensitive investors' expectations are to current cash flows, the greater is the incentive to distort investment and hence the greater is the difference between public and private firms' investment sensitivities.

To test these two cross-sectional predictions, we compute 'earnings response coefficients,' or ERC (Ball and Brown (1968)), for each industry-year. For industries whose ERC = 0, we find no significant difference in investment sensitivities between public and private firms. This pattern is consistent with the first prediction, i.e., the absence of distortions for public firms whose current cash flows are uninformative about their future cash flows. As ERC increases, we find that public firms' investment sensitivity decreases significantly while that of private firms remains unchanged. In other words, the difference in investment sensitivities between public and private firms increases in ERC, and this increase is driven by a change in public-firm behavior. This pattern is consistent with the second prediction.

These cross-sectional patterns are consistent with short-termism models. Moreover, it is hard to see how they would support alternative explanations. A preference for a quiet life may indeed result in lower investment sensitivities among public firms whose managers are poorly monitored, but it is unclear why such a preference should be absent for firms whose current cash flows are uninformative about their future cash flows, nor why the preference would become stronger the greater an impact current cash flows have on investors' expectations. Similarly, while we do not doubt that public firms are subject to more onerous rules and regulations and so may be less nimble than private firms, we can see no obvious reason why the effect of these rules and regulations would correlate with the informativeness of current earnings. We thus conclude that the data favor a short-termism explanation for the observed differences in investment behavior between public and private firms.

Our findings have implications for the distribution of public firms across industries. If investment distortions increase in the informativeness of current earnings, we expect fewer firms to be stock market listed in high-ERC industries than in low-ERC industries, all else equal. We test this prediction by regressing the share of public firms in an industry on the industry's ERC, controlling for the industry's R&D intensity and capital intensity as other likely drivers of the listing decision. The results confirm that there are significantly fewer stock market listed firms in high-ERC industries. To illustrate, a one-standard deviation increase in ERC is associated with a decrease of around a third in the share of public firms. These patterns are consistent with the interpretation that firms and investors view agency-induced investment distortions as a cost of being publicly listed.[2]

Finally, we document that public firms differ from private firms not only in their investment behavior but also in their financial decisions. Most importantly, public firms tend to smooth dividend payouts over time while private firms have more volatile payout policies. One plausible interpretation of this pattern is that public firms treat investment as the residual after setting dividends, whereas private firms treat dividends as the residual after funding their investment plans out of their cash flows.

Our empirical strategy implicitly assumes that private firms, carefully screened to be observably similar, provide a good benchmark for how public firms would behave absent agency problems.[3] This assumption is common in the literature.[4] Since agency problems ultimately stem from a conflict of interest between managers and shareholders and insufficient incentives to monitor, the assumption will hold as long as private firms have less separation of ownership and control and more concentrated ownership than public firms. Evidence from the Federal Reserve's 2003 Survey of Small Business Finances (SSBF) suggests that they do. As Tables A7 and A8 in our Online Data Appendix show, 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 indeed little separation of

---

[2] Clearly, some firms will choose to be public despite these distortions, perhaps to allow founding shareholders to diversify their investments or cash out entirely. See Brau and Fawcett (2006) for survey evidence of why firms go public.

[3] The fact that public firms have access to cheaper capital than private firms, all else equal, biases our results against finding that short-termism induces public firms to underinvest.

[4] 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)."

ownership and control in private firms.[5] Public-firm CEOs, by contrast, 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.

Our paper makes two 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 short-termism. 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. Ten years on, Graham, Harvey, and Rajgopal (2005, p. 3) report the startling survey finding that "the majority of managers would avoid initiating a positive NPV 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) argues that large shareholders have an incentive to actively monitor managers and fire them if necessary, while Edmans' (2010) model shows that large shareholders can reduce managerial myopia by trading on private information so that stock prices will reflect fundamental value rather than current cash flows. But it is an empirical question whether these mechanisms are sufficiently effective on average. Our evidence suggests that, on net, they are not.

Second, 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.

The paper proceeds as follows. Section 1 discusses related literature. Section 2 introduces a rich new database of private firms in the U.S. created by Sageworks Inc. (The Online Data Appendix describes it in detail.) Section 3 establishes our main empirical result, that public firms are less responsive to changes in

[5] 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.

investment opportunities than private firms. Section 4 tests whether this pattern is due to short-termism. Section 5 shows that public firms tend to smooth their earnings and dividend payouts and avoid reporting small accounting losses. Section 6 concludes.

1. Related Literature

The literature on agency costs is vast. We depart from it by exploiting variation along the extensive (public/private) margin. Existing empirical work in this area focuses instead on the intensive margin. Examples include Wurgler (2000), Knyazeva et al. (2007), Bøhren, Cooper, and Priestley (2009), and Gopalan et al. (2010), who relate investment among *public* firms to variation in proxies for agency costs and corporate governance, and Fang, Tian, and Tice (2010), who examine whether public firms with more liquid shares (and thus more footloose investors) are less innovative. Our approach is distinct from, but complementary to, this body of 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.

There is a small but growing empirical 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 the notion that a listing reduces a firm's cost of capital. Also using British data, Michaely and Roberts (2007) show that private firms smooth dividends less than public firms while Brav (2009) shows that private firms rely mostly on debt financing. 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 hypothesis that private firms are subject to fewer agency problems than public firms. Edgerton (2010) shows that public firms overuse corporate jets compared to observably similar private firms, consistent with agency problems. Sheen

(2009) analyzes hand-collected investment data for public and private firms in the chemical industry, finding results similar to ours.

Bharat, Dittmar, and Sivadasan (2010) analyze within-firm changes in productivity in a sample of firms that are taken private, for example by a private equity fund. For this group of firms, productivity does not change after the change in ownership, relative to similar still-listed firms, and firms close down plants after being taken private. This is consistent with the idea that private equity buyers target firms in shrinking industries that are in need of consolidation and disinvestment.

Finally, there is an active accounting literature that documents that *some* public-firm managers *sometimes* act myopically, in the sense of taking costly actions to avoid negative earnings surprises. Bhojraj et al. (2009) show that firms that barely beat analysts' earnings forecasts myopically cut discretionary spending. This avoids the short-run stock price hit associated with missing earnings forecasts (Skinner and Sloan (2002)) but over longer horizons leads to underperformance. Roychowdhury (2006) finds that firms discount product prices to boost sales and thereby meet short-term earnings forecasts. Bhojraj and Libby (2005) provide related evidence from laboratory experiments. 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. Our results are broader. We show that investment is distorted for the *average* public firm in our large sample.

2. Sample and Data

Private firms are not subject to public reporting requirements, so little is known about their investment 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. We provide a comprehensive overview of Sageworks, along with detailed summary statistics, in the Online Data Appendix.

Sageworks is similar to Compustat, the standard database for public U.S. firms. Like Compustat, Sageworks contains accounting data from income statements and balance sheets along with basic demographic information such as NAICS industry codes and geographic location, except that Sageworks exclusively covers private firms. Unlike in Compustat, firm names are masked though each firm has a unique identifier allowing us to construct a panel. 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 such transitions 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 that 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. The accounting firms Sageworks co-operates with include most national mid-market firms (those below the 'Big Four') as well as hundreds of regional players, but only a few of the many thousand local accountants who service the smallest firms in the U.S. 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 SSBF.[6] This selection may be problematic depending on the research question but it is innocuous for our purposes since very small firms have no realistic chance of being public.

Sageworks started 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 covering 95,370 firms and 250,507 firm-years.

Sageworks is free of survivorship bias. If a firm goes public, dies, or switches to an accounting firm that doesn't co-operate with Sageworks, its data time series will end but all of its historical data remains in the database.

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

[6] Unlike Sageworks, the SSBF is not a panel: While it has been conducted every few years, the cohorts of firms surveyed are different every time.

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

To ensure an apples-to-apples comparison, we would ideally compare 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 unexplained variation left that could be due to unobserved characteristics. We thus match on size and industry, though we will consider alternatives for robustness.

Matching on size means that our matched sample consists of *small* public and *large* private firms.

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,[7] 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.[8]

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 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 somewhat larger private firms and the remaining 1,205 firms matched to somewhat 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.

[7] As we will show, our results are robust to using finer industry classifications, such as NAICS5 or NAICS6.

[8] We cluster standard errors in the usual way but do not adjust for variation introduced by the 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 support all inference presented in the paper, with two exceptions: In Table 2, column 4, the coefficient on "Investment opp. x public" has a *p*-value of 12%; and in Table 7, column 3, the coefficient on "ROA" has a *p*-value of 21%.

2.3 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 on a stock exchange, 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 (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), Billett, King, and Mauer (2007), and Acharya, Almeida, and Campello (2007).

For robustness purposes, we also explore two *Q* measures. The simplest measure constructs an 'industry *Q*' from public-firm data and then applies that to all firms, public and private. We measure industry *Q* for each four-digit NAICS industry and year as the size-weighted average *Q* of all public firms in that industry. Alternatively, we can impute *Q* at the firm level. Campello and Graham (2007) suggest regressing *Q*, for public firms, 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.

2.4 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.[9] 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 A.)

2.5 Differences in Investment Levels

Table 1 shows that private firms invest significantly more than public firms on average. The differences are substantial. 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.) Assuming that private firms are less prone to agency problems on account of their more concentrated ownership structures, these results suggest that public firms are *not* empire builders on average.[10]

3. How Do Public and Private Firms Respond to Changes in 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.[11] 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 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

[9] 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 Sageworks does not break out R&D spending. We will, however, report evidence showing it is highly unlikely that our results are driven by this data limitation.

[10] Table 1 also 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.

[11] As we will show later, we obtain similar results using net investment instead. Also, as we will show, our results are robust to including R&D spending, which is left out of gross investment.

unobserved time-invariant heterogeneity by using firm fixed effects, and we 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, 3.5 times greater than the 0.136 – 0.097 = 0.039 coefficient estimate for public firms. The difference between these estimates is statistically significant at the 1% level. 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 private and public 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 estimated 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.

Columns 4 and 5 show that our results are robust to matching on finer industry codes. Using five-digit NAICS (rather than NAICS4 as we have done so far) has no effect at all on the point estimates (see column 4). Using six-digit NAICS naturally reduces the sample size considerably but again suggests that public firms have significantly lower investment sensitivities than private ones. (In fact, the difference is 50% larger when we use NAICS6 in our match.) Our findings thus do not appear to be driven by an overly noisy industry match, so we continue to match on NAICS4 in the remainder of the paper.

Our findings are also robust to using our two measures of Q to proxy for investment opportunities; see columns 6 and 7. 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. Complete results using the two Q measures are available on request.

3.2 Alternative Specifications

Table 3 considers ten alternative specifications. We begin by investigating an important potential confound: Age. Jovanovic and Rousseau (2010) 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, such lifecycle effects 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) SSBF 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 appear to 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.

Sageworks does not report R&D spending. There is substantial evidence linking IPOs to subsequent increases in R&D, suggesting that the stock market is an important source of funding for R&D projects (Kim and Weisbach (2008), Brown, Fazzari, and Petersen (2009)). Omitting R&D from the dependent variable would then bias our results if public firms' R&D spending was more sensitive to changes in investment opportunities than private firms', sufficiently so to outweigh the lower sensitivity of public firms' fixed investment spending. While the absence of R&D data in Sageworks prevents us from testing this story directly, we can assess its plausibility indirectly. To do so, we test whether including R&D spending in the dependent variable increases the estimated investment sensitivity of public firms. Column 4 shows that it does not. The point estimate is 0.042 when we include R&D, marginally greater than the point estimate of 0.038 when we omit R&D (cf. column 2 of Table 2). On its own, R&D is thus far from sufficient to close the gap in investment sensitivities between public and private firms. Closing the gap would require that private firms' unobserved R&D spending correlated in a strong and *negative* way with investment opportunities. While this is possible, there is no compelling reason to think it is likely.

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 5 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 6 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 7, 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 8 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 9 matches on sales growth while column 10 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 9, propensity scores average 0.251 for matched public firms and 0.246 for matched private firms; in column 10, mean propensity scores are 0.256 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 opportunities, 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.

Across all ten alternative specifications considered in Table 3, we find the same result: Public firms are significantly and substantially less responsive to changes in investment opportunities. Before we consider possible explanations, we ask if measurement error might spuriously generate this pattern.

3.3 Differential Measurement Error

Measurement error in proxies for investment opportunities can lead to attenuation bias so that the estimated investment sensitivity is too low. For this 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 those 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 4 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.[12] 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 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.

3.4 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 thus a plausibly exogenous shock to investment opportunities. If private firms' investment decisions are indeed more sensitive to investment opportunities, we expect private firms also 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

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

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.[13] 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 B for details.[14] 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 Table 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 industry-and-size-matched sample for this test because it contains only 13 'treated' private firms.

Table 5 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

[13] 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.

[14] 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 (i.e., a tax on sales rather than on profits). Similarly, in 2007, Texas replaced a 4.5% corporate income tax with a 1% tax on gross receipts.

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 5 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.

3.5 Do Private Firms Have Fewer Agency Problems?

Our empirical strategy assumes that private firms suffer from fewer investment-distorting agency problems compared to public firms. Since such agency problems ultimately are the result of a separation of ownership and control and insufficient incentives for monitoring, this assumption will hold only to the extent that private firms have relatively more concentrated ownership structures than do public firms. We know that the public firms in our sample have highly dispersed ownership (see for example Table A7 in the Online Data Appendix), and we know from the Federal Reserve's SSBF survey that private firms in the U.S. almost invariably have highly concentrated ownership (see Table A8 in the Online Data Appendix.) But Sageworks provides no ownership data, so we cannot test the assumption directly.

Sageworks does, however, report each firm's legal form, and four of these correlate strongly 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 SSBF, 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 SSBF, 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.

This leaves two legal forms that can *theoretically* involve any degree of separation: C Corps can have any number of shareholders while S Corps can have up to 100. Absent data on their actual ownership, we test indirectly for differences in ownership structure between C and S Corps on the one hand and those legal forms that imply high ownership concentration on the other. Specifically, if private C and S Corps have dispersed ownership, and hence agency problems, on average, we expect to find that their investment behavior is systematically different from that of the other legal forms.

Table 6 tests this by allowing 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 five models tells the same story: There is no significant variation in investment sensitivities *within* the sample of private firms, in contrast to the tremendous variation we found *between* public and private firms. Since a non-trivial fraction of private firms in our sample are by definition free of agency costs, this is consistent with private firms suffering from fewer investment-distorting agency problems, as our empirical strategy assumes.

3.6 Within-firm Changes in Investment Behavior Around IPOs

So far, our tests have compared the behavior of public and private firms using matched panels of observably similar firms. While we are the first to have access to comprehensive financial data on a large sample of private firms in the U.S., making such tests possible for the first time, there is a clear identification concern: The observed differences in investment could be driven by *un*observed differences between public and private firms. 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. Gertner, Powers, and Scharfstein (2002), for example, employ this identification strategy, showing that divisions increase their sensitivity to investment opportunities from before to after they are spun off as stand-alone firms by their parent companies.

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 strategy reduces identification concerns, but it cannot eliminate them. 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 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.[15] Alternatively, one could use the 2002 Sarbanes-Oxley Act as an exogenous shock

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

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 the Act, and while some of its provisions were phased in for small firms, they did not come into force until after the end of our sample period.[16] As a result, 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 C 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 and income statements. On average, we have 4.4 pre-IPO years of accounting data.

Columns 1 and 2 of Table 7 test if investment sensitivities change around a firm's IPO. 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. To allow for this, columns 3 and 4 report difference-in-difference results, combining data from the IPO sample with data for size-and-industry matched public-firm controls. 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 have significantly greater investment sensitivities; but once they are public, their sensitivities are not only significantly lower but they become indistinguishable from those of observably similar, public firms.

[16] 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 is also poor: Delisting is really a change in trading venue and so need not lead to more concentrated ownership.

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, despite their relatively easier access to capital. What drives these differences? As we discussed in the introduction, there are several agency models that predict suboptimal investment behavior among public firms. Since we find that public firms invest less than private firms, we focus on those that predict underinvestment: A preference for a quiet life and certain types of short-termism models. In the next section, we report cross-sectional evidence that is more nearly consistent with short-termism than with a preference for a quiet life.

4. Validating Short-termism

Models of short-termism in the Stein (1989) mould predict that the extent of a public firm's investment distortions depends on the sensitivity of its share price to its current cash flow. This parameter, which Stein calls α_0, captures the extent to which investors base their expectations of the firm's future cash flows on its current cash flow. Firms with high α_0 are firms whose current cash flows are highly informative about their future cash flows. Conversely, in firms with low α_0, current cash flows are subject to a lot of transitory noise and so less useful in forming accurate predictions about future cash flows. Thus, the higher is α_0, the more informative are current cash flows for pricing the stock and so the greater is the manager's incentive to manipulate investors' expectations by underinvesting.

Short-termism thus has two cross-sectional implications. First, we expect *no* distortion for a public firm whose current cash flow is uninformative about future cash flows, that is, whose $\alpha_0 = 0$. In this case, the manager cannot manipulate investors' cash flow expectations, so there is no point making myopic investment decisions. Second, as α_0 increases, distortions increase. Thus, we expect the difference in investment sensitivities between public and private firms to be zero for $\alpha_0 = 0$ and to increase in α_0. We view these predictions as unique to short-termism as the α_0 parameter plays no role in alterative stories.

4.1 Cross-industry Variation in Investment Behavior

To test these predictions, we follow the accounting literature and measure α_0 using earnings response coefficients, or ERC (Ball and Brown (1968), Beaver (1968)). As our sample contains unlisted firms, we cannot estimate ERC at the firm level. Instead, we estimate a set of industry ERCs for each year from 2001 to 2006.[17] Industries are defined using Fama and French's (1997) 30 industry groups, which contain enough firms to estimate ERCs relatively precisely. (For details of the estimation, see Appendix A.) We also report somewhat noisier results using their finer 48 industries, which contain fewer firms. We then include a full set of interaction terms involving lagged industry ERCs in our baseline investment equation.

Panel A of Table 8 reports the results. In row one, for ERC = 0, we find no significant difference in investment sensitivities between public and private firms: The point estimate for private firms is 0.099 (p<0.001) while that for public firms is an insignificant 0.033 lower (p=0.355). This pattern is consistent with an absence of distortions for $\alpha_0 = 0$, as predicted. As ERC increases, public firms' investment sensitivity decreases significantly (p=0.033) while that of private firms increases somewhat, though not significantly so (p=0.178). In other words, the difference in investment sensitivities between public and private firms increases in ERC, and this increase is driven by a change in public-firm behavior. This pattern is consistent with short-termism. Finally, note that ERC affects investment only through its interaction with investment sensitivities; it has no direct effect on investment for either public or private firms. This suggests that ERC does not capture some omitted dimension of investment opportunities.

Panel B illustrates the economic magnitudes, showing implied investment sensitivities, from Panel A, for private and public firms at the 25th and 75th percentile of the ERC distribution. In low-ERC industries, the investment sensitivity is 0.062 higher for private firms, using Fama-French 30 industries. In high-ERC industries, the difference more than doubles, to 0.141. The difference in differences of 0.079 is highly significant. It is driven by a significant decline in sensitivity among public firms, from 0.054 at the 25th percentile to 0.019 at the 75th percentile. Results look very similar using Fama-French 48 industries.

4.2 Interpretation

The findings in Table 8 are consistent with the interpretation that it is short-termism rather than some

[17] We are grateful to Mary Billings for providing the necessary data.

other friction that accounts for the observed differences in investment behavior between public and private firms in our sample. The effect of α_0 on public (but not private) firms' investment sensitivity fits Stein's (1989) model of short-termism but is not predicted by any other agency model that we are aware of. Nor is it predicted by non-agency stories, such as the idea that public firms are less responsive to changes in investment opportunities simply because they are weighed down by rules and regulations designed to protect minority shareholders.

But perhaps it is the private firms that invest suboptimally. Maybe private firms respond more strongly to investment opportunities not because they are relatively agency-cost free, but because they are capital-inefficient, inexperienced at making investment decisions, or closet empire-builders. In light of the results in this section, these alternative interpretations do not seem plausible. They cannot explain why public and private firms exhibit similar investment sensitivities when $\alpha_0 = 0$ nor why *public* firms' investment sensitivities decrease in α_0.

4.3 Cross-industry Variation in the Share of Public Firms

These findings have implications for the distribution of public firms across industries. If investment distortions increase in the informativeness of current earnings, and if investors understand this, we expect fewer firms to be public in high-α_0 industries than in low-α_0 industries, all else equal. We test this prediction by regressing the share of public firms in an industry on the industry's ERC, controlling for its R&D intensity and capital intensity as other likely drivers of the listing decision. Apart from ERC, our variables are constructed from Census data which restricts us to estimating a single cross-section, for 2007. (See Appendix A for further details.) To ensure we have enough power, we model the share of public firms per four-digit NAICS industry, of which there are 283, rather than in the 30 or 48 Fama-French industry groups.

In column 1 of Panel C, we measure the share of public firms in an industry as the ratio of the number of public firms (from Compustat) to the total number of firms (from the Census). The controls behave as expected: Significantly more firms are public in industries with greater R&D or capital intensity. The results for ERC also go in the right direction – there are significantly fewer public firms in industries with higher ERC – but the effect appears quite small economically: A one-standard deviation increase in ERC is associated with a 0.1 percentage point decrease in the share of public firms. This reflects the very large number of small private firms in the economy; in the average industry, 994 in 1,000 firms are private.

A better way to measure the share of public firms is to exclude small firms from the denominator.[18] Following Chod and Lyandes (2010), who advocate this approach, columns 2 and 3 focus on the share of public firms among firms with at least 100 or 500 employees. The results remain unchanged – there are significantly fewer public firms in high-ERC industries – but the economic magnitudes are larger. In column 2, a one standard deviation increase in ERC leads to a 1.5 percentage point decrease in the share of public firms – a 32.6% decrease from the 4.6% unconditional mean share of public firms. The corresponding effect in column 3 is a 3.3 percentage point decrease from the 9.9% mean. These results are consistent with the interpretation that investors view short-termism as a cost of being publicly listed.

5. Income Smoothing, Payout Policy, and Accounting Losses

On balance, our evidence favors models of short-termism. 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). Alternatively, if the market uses today's dividend to form its expectations of future profits, the manager may sacrifice investment to keep the dividend high and thereby signal that the firm's profitability remains sound, as in Miller and Rock's (1985) short-termism model. 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

[18] Computing the public-firm share using revenue instead is tricky. Compustat revenue includes foreign sales while the revenue data reported by the Census does not. The resulting ratio is not well behaved: 30 out of 283 industries have ratios greater than 1.

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 test asks whether short-termism induces public firms to make sub-optimal investment decisions in an effort to avoid reporting accounting losses, as the accounting literature claims (e.g., Baber, Fairfield, and Haggard (1991)). If so, we expect a greater fraction of public firms than of private firms to report earnings just above zero. We measure earnings as net income scaled by total assets and focus on two intervals around zero, namely (-0.10, 0.10) and (-0.05, 0.05). The results, reported in Panels A and B of Table 10, indicate that public firms are more likely to report small positive earnings than are private firms, and the differences are both economically and statistically significant. Panel C reports placebo tests, which test for differences in the fractions of public and private firms reporting earnings above six arbitrary thresholds away from zero, namely -0.3, -0.2, -0.1, 0.1, 0.2, and 0.3. Interestingly, at each of the placebo points in the earnings distribution, a significantly *smaller* fraction of public firms report earnings above the threshold compared to private firms – contrary to what happens at the zero earnings threshold. This is consistent with public firms 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 or underinvestment 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 approach this debate from an empirical angle, by comparing the investment behavior of observably similar public and private firms. Our results show that relative to private firms, public firms invest less and in a way that is significantly less responsive to changes in investment opportunities, especially in industries in which stock prices are quite sensitive to current cash flows. 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, lifecycle differences, or our matching criteria.

Our findings are most 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. We find no evidence that looks consistent with empire building or other agency problems that lead to overinvestment.

We also show that public firms tend to smooth their earnings growth and their payouts to shareholders and are reluctant to report negative earnings. These patterns may 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.

We are careful not to claim causality. 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. In fact, the opposite seems much more likely, implying that our results may underestimate the extent of investment distortions among public firms.

References

Abadie, Alberto, and Guido W. Imbens, 2008, On the failure of the bootstrap for matching estimators, Econometrica 76, 1537-1557.

Acharya, Viral V., Heitor Almeida, and Murillo Campello, 2007, Is cash negative debt? A hedging perspective on corporate financial policies, Journal of Financial Intermediation 16, 515-554.

Ang, James S., Rebel A. Cole, and James Wuh Lin, 2000, Agency costs and ownership structure, Journal of Finance 55, 81-106.

Arellano, Manuel, and Stephen Bond, 1991, Some tests of specification for panel data: Monte Carlo evidence and an application to employment equations, Review of Economic Studies 58, 277-297.

Asker, John, Joan Farre-Mensa, and Alexander Ljungqvist, 2010, What Do Private Firms Look like? Data Appendix to "Does the Stock Market Harm Investment Incentives?", Unpublished working paper, New York University.

Baber, William R., Patricia M. Fairfield, and James A. Haggard, 1991, The effect of concern about reported income on discretionary spending decisions: The case of research and development, Accounting Review 66, 818-829.

Bakke, Tor-Erik, and Toni Whited, 2010, Threshold events and corporate policies, Unpublished working paper, University of Rochester.

Ball, Ray, and Philip Brown, 1968, An empirical evaluation of accounting income numbers, Journal of Accounting Research 6, 159-78.

Baumol, William, 1959, Business Behavior, Value, and Growth (New York, Macmillan).

Beaver, William H., 1968, The information content of annual earnings announcements, Journal of Accounting Research 6, 67-92.

Bebchuk, Lucian A., and Lars A. Stole, 1993, Do short-term objectives lead to under- or overinvestment in long-term projects?, Journal of Finance 48, 719-729.

Berle, Adolf A., and Gardiner C. Means, 1932, The Modern Corporation and Private Property (New York, Macmillan).

Bertrand, Marianne, and Sendhil Mullainathan, 2003, Enjoying the quiet life? Corporate governance and managerial preferences, Journal of Political Economy 111, 1043-1075.

Bharat, Sreedhar, Amy Dittmar, and Jagadeesh Sivadasan, 2010, Does capital market myopia affect plant productivity? Edvidence from "going private" transactions, Unpublished working paper, University of Michigan.

Bhide, Amar, 1993, The hidden cost of stock market liquidity, Journal of Financial Economics 34, 34-51.

Bhojraj, Sanjeev, Paul Hribar, Marc Picconi, and John McInnis, 2009, Making sense of cents: An examination of firms that marginally miss or beat analyst forecasts, Journal of Finance 64, 2361-2388.

Bhojraj, Sanjeev, and Robert Libby, 2005, Capital market pressure, disclosure frequency-induced earnings/cash flow conflict, and managerial myopia, Accounting Review 80, 1-20.

Billett, Matthew T., Tao-Hsien Dolly King, and David C. Mauer, 2007, Growth opportunities and the choice of leverage, debt maturity and covenants, Journal of Finance 62, 697-730.

Blundell, Richard, and Stephen Bond, 1998, Initial conditions and moment restrictions in dynamic panel data models, Journal of Econometrics 87, 115-143.

Bøhren, Øyvind, Ilan Cooper, and Richard Priestley, 2009, Real investment, economic efficiency, and managerial entrenchment, Unpublished working paper, Tel Aviv University.

Bond, Stephen, and John van Reenen, 2007, Microeconometric models of investment and employment, in: J.J. Heckman and E.E. Leamer (eds.), Handbook of Econometrics Vol. 6, chapter 65 (Amsterdam, Elsevier).

Brau, James C., and Stanley E. Fawcett, 2006, Initial public offerings: An analysis of theory and practice, Journal of Finance 59, 399-436.

Brav, Omer, 2009, Access to capital, capital structure, and the funding of the firm, Journal of Finance 64, 263-208.

Brown, James R., Steven M. Fazzari, and Bruce Petersen, 2009, Financing innovation and growth: Cash flow, external equity, and the 1990s R&D boom, Journal of Finance 64, 151-185.

Bushee, Brian J., 1998, The influence of institutional investors on myopic R&D investment behavior, Accounting Review 73, 305-333.

Campello, Murillo, and John Graham, 2007, Do stock prices influence corporate decisions? Evidence from the technology bubble, NBER Working Paper no. 13640.

Chod, Jiri, and Evgeny Lyandes, 2010, Strategic IPOs and product market competition, Journal of Financial Economics, forthcoming.

Cleary, Sean, 1999, The relationship between firm investment and financial status, Journal of Finance 54, 673-692.

Dechow, Patricia M., and Richard G. Sloan, 1991, Executive incentives and the horizon problem: An empirical investigation, Journal of Accounting and Economics 14, 51-89.

Easton, Peter D., and Mark E. Zmijewski, 1989, Cross-sectional variation in the stock market response to accounting earnings announcements, Journal of Accounting and Economics 11, 117-141.

Edgerton, Jesse, 2010, Agency problems in public firms: Evidence from corporate jets in leveraged buyouts, Unpublished working paper, Federal Reserve Board.

Edmans, Alex, 2010, Blockholder trading, market efficiency, and managerial myopia, Journal of Finance, forthcoming.

Erickson, Timothy, and Toni Whited, 2000, Measurement error and the relationship between investment and Q, Journal of Political Economy 108, 1027-1057.

Fama, Eugene F., and Kenneth R. French, 1997, Industry costs of equity, Journal of Financial Economics 43, 153-193.

Fang, Vivian W., Xuan Tian, and Sheri Tice, 2010, Does stock liquidity enhance or impede firm innovation?, Unpublished working paper, Tulane University.

Fazzari, Steven, R. Glenn Hubbard, and Bruce Petersen, 1988, Financing constraints and corporate investment, Brookings Papers on Economic Activity, 141-195.

Gao, Huasheng, Michael Lemmon, and Kai Li, 2010, A comparison of CEO pay in public and private U.S. firms, Unpublished working paper, University of British Columbia.

Garvey, Gerald T., Simon Grant, and Stephen P. King, 1999, Myopic corporate behavior with optimal management incentives, Journal of Industrial Economics 47, 231-50.

Gertner, Robert, Eric Powers, and David Scharfstein, 2001, Learning about internal capital markets from corporate spin-offs, Journal of Finance 57, 2479-2506.

Gopalan, Radhakrishnan, Todd Milbourn, Fenghua Song, and Anjan V. Thakor, 2010, The optimal duration of executive compensation: Theory and evidence, Unpublished working paper, Washington University in St. Louis.

Graham, John R., Campbell R. Harvey, and Shiva Rajgopal, 2005, The economic implications of corporate financial reporting, Journal of Accounting and Economics 40, 3-73.

Hayashi, Fumio, 1982, Tobin's *Q* and average *Q*: A neoclassical interpretation, Econometrica 50, 213-224.

Holmström, Bengt, 1999, Managerial incentive problems: A dynamic perspective, Review of Economic Studies 66, 169-182.

Holmström, Bengt, 1982, Managerial incentive problems: A dynamic perspective, in: Essays in Economics and Management in Honor of Lars Wahlbeck (Helsinki, Swedish School of Economics).

Imbens, Guido W., and Jeffrey M. Wooldridge, 2009, Recent developments in the econometrics of program evaluation, Journal of Economic Literature 47, 5-86.

Jensen, Michael, and William H. Meckling, 1976, Theory of the firm: Managerial behavior, agency costs and ownership structure, Journal of Financial Economics 3, 305-360.

Jensen, Michael, 1986, Agency cost of free cash flow, corporate finance, and takeovers, American Economic Review 76, 323-32.

Jovanovic, Boyan, and Peter L. Rousseau, 2010, Extensive and intensive investment over the business cycle, Unpublished working paper, NYU.

Kaplan, Steven, and Luigi Zingales, 1997, Do financing constraints explain why investment is correlated with cash flow?, Quarterly Journal of Economics 112, 169-215.

Kaplan, Steven, and Luigi Zingales, 2000, Investment-cash flow sensitivities are not valid measures of financing constraints, Quarterly Journal of Economics 115, 707-712.

Kedia, Simi, and Thomas Philippon, 2009, The economics of fraudulent accounting, Review of Financial Studies 22, 2169-2199.

Kim, Woojin, and Michael S. Weisbach, 2008, Motivations for public equity offers: An international perspective, Journal of Financial Economics 87, 281-307.

Knyazeva, Anzhela, Diana Knyazeva, Randall Morck, and Bernard Yeung, 2007, Comovement in investment, Unpublished working paper, University of Rochester.

Lehn, Kenneth, and Annette Poulsen, 1989, Free cash flow and stockholder gains in going private transactions, Journal of Finance 44, 771-787.

Ljungqvist, Alexander, and William J. Wilhelm, 2003, IPO pricing in the dot-com bubble, Journal of Finance 58, 723-752.

Martin, Kenneth J., 1996, The method of payment in corporate acquisitions, investment opportunities, and management ownership, Journal of Finance 51, 1227-1246.

Michaely, Roni, and Michael R. Roberts, 2007, Corporate dividend policies: Lessons from private firms, Unpublished working paper, Cornell University.

Miller, Merton, and Kevin Rock, 1985, Dividend policy under asymmetric information, Journal of Finance 40, 1031-1051.

Narayanan, M.P., 1985, Managerial incentives for short-term results, Journal of Finance 40, 1469-1484.

Pagano, Marco, Fabio Panetta, and Luigi Zingales, 1998, Why do companies go public? An empirical analysis, Journal of Finance 53, 27-64.

Politis, Dimitris, Joseph Romano, and Michael Wolf, 1999, Subsampling (New York: Springer).

Poterba, James, and Lawrence H. Summers, 1995, A CEO survey of U.S. companies' time horizons and hurdle rates, Sloan Management Review 37, 43-53.

Roychowdhury, Sugata, 2006, Earnings management through real activities manipulation, Journal of Accounting and Economics 42, 335-370.

Rozeff, Michael S., 1982, Growth, beta and agency costs as determinants of dividend payout ratios, Journal of Financial Research 5, 249-259.

Saunders, Anthony, and Sascha Steffen, 2009, The costs of being private: Evidence from the loan market, Unpublished working paper, New York University.

Sheen, Albert, 2009, Do public and private firms behave differently? An examination of investment in the chemical industry, Unpublished working paper, UCLA.

Shin, Hyun-Han, and René M. Stulz, 1998, Are internal capital markets efficient?, Quarterly Journal of Economics 113, 531-552.

Shleifer, Andrei, and Robert W. Vishny, 1990, Equilibrium short horizons of investors and firms, American Economic Review 80, 148-153.

Shleifer, Andrei, and Robert W. Vishny, 1997, A survey of corporate governance, Journal of Finance 52, 737-783.

Skinner, Douglas J., and Richard G. Sloan, 2002, Earnings surprises, growth expectations, and stock returns or don't let an earnings torpedo sink your portfolio, Review of Accounting Studies 7, 289-312.

Stein, Jeremy C., 1988, Takeover threats and managerial myopia, Journal of Political Economy 96, 61-80.

Stein, Jeremy C., 1989, Efficient capital markets, inefficient firms: A model of myopic corporate behavior, Quarterly Journal of Economics 104, 655-669.

Stulz, René M., 1990, Managerial discretion and optimal financing policies, Journal of Financial Economics 26, 3-27.

Tirole, Jean, 2001, Corporate governance, Econometrica 69, 1-35.

Von Thadden, Ernst-Ludwig, 1995, Long-term contracts, short-term investment, and monitoring, Review of Economic Studies 62, 557-575.

Whited, Toni M., 2006, External finance constraints and the intertemporal pattern of intermittent investment, Journal of Financial Economics 81, 467-502.

Wurgler, Jeffrey, 2000, Financial markets and the allocation of capital, Journal of Financial Economics 58, 187-214.

Appendix A. 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 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 3-digit NAICS industries). We then use the regression coefficients to generate predicted *Q* for each firm, both public and private ones.

Industry Q is the lagged size-weighted mean of Tobin's *Q* (Compustat items *prcc_f×cshpri + pstkl + dltt + dlc – txditc* divided by beginning-of-year total assets, *at*), estimated separately for each four-digit NAICS industry and each year. We use Compustat total assets (*at*) as weights in computing the size-weighted means.

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.

ERC is the earnings response coefficient. Following Easton and Zmijewski (1989), we estimate *ERC* separately for each industry *j* and fiscal year *t*=2001 to 2006 by regressing abnormal returns SAR_{ijt} on a constant and on unexpected earnings UE_{ijt} using all firms *i* in industry *j*. *ERC* for industry *j* in year *t* is the coefficient estimated for UE_{ijt}. SAR_{ijt} is firm *i*'s size-adjusted abnormal return in the five-day window centered on the day the firm announced annual earnings. UE_{ijt} is firm *i*'s earnings surprise, measured as actual earnings per share less analyst consensus (i.e., the median outstanding earnings forecast from I/B/E/S data). We are grateful to Mary Billings for sharing these data with us. We use the Fama and French (1997) classification of either 30 or 48 industry groups, available from Kenneth French's webpage. Once we have an ERC estimate for each Fama-French industry and year, we assign each private firm to a Fama-French industry based on its NAICS code. (We map NAICS codes to SIC codes using the U.S. Census Bureau's NAICS-SIC bridge, available at http://www.census.gov/epcd/naics02/index.html.)

Industry R&D intensity equals the ratio of total R&D spending and total revenue in each four-digit NAICS industry, measured in 2007 and covering both public and private firms. Data on corporate R&D spending by NAICS industry come from the National Science Foundation; see Table 2 at http://www.nsf.gov/statistics/infbrief/nsf09316/. Data on industry revenue come from "Statistics of U.S. Businesses" provided by the Census Bureau. There is no Census data for NAICS codes 111 (Crop production), 112 (Animal production), and 4821 (Rail transportation).

Industry capital intensity equals one minus the ratio of labor costs and total revenue in each four-digit NAICS industry, measured in 2007 and covering both public and private firms. Labor costs are measured as data item "annual payroll" in the Core Business Statistics of the Economic Census. Data on industry revenue come from "Statistics of U.S. Businesses" provided by the Census Bureau.

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.

Appendix B. List of State Corporate Income Tax Changes

This table lists the state corporate income tax changes that we use for the analysis in Table 5. We limit our attention to state corporate income tax changes that occurred during our sample period (2002-2007) and that 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 C. 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 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 unit of observation in the bottom graph is a firm-year, to illustrate the closeness of the matched panels.

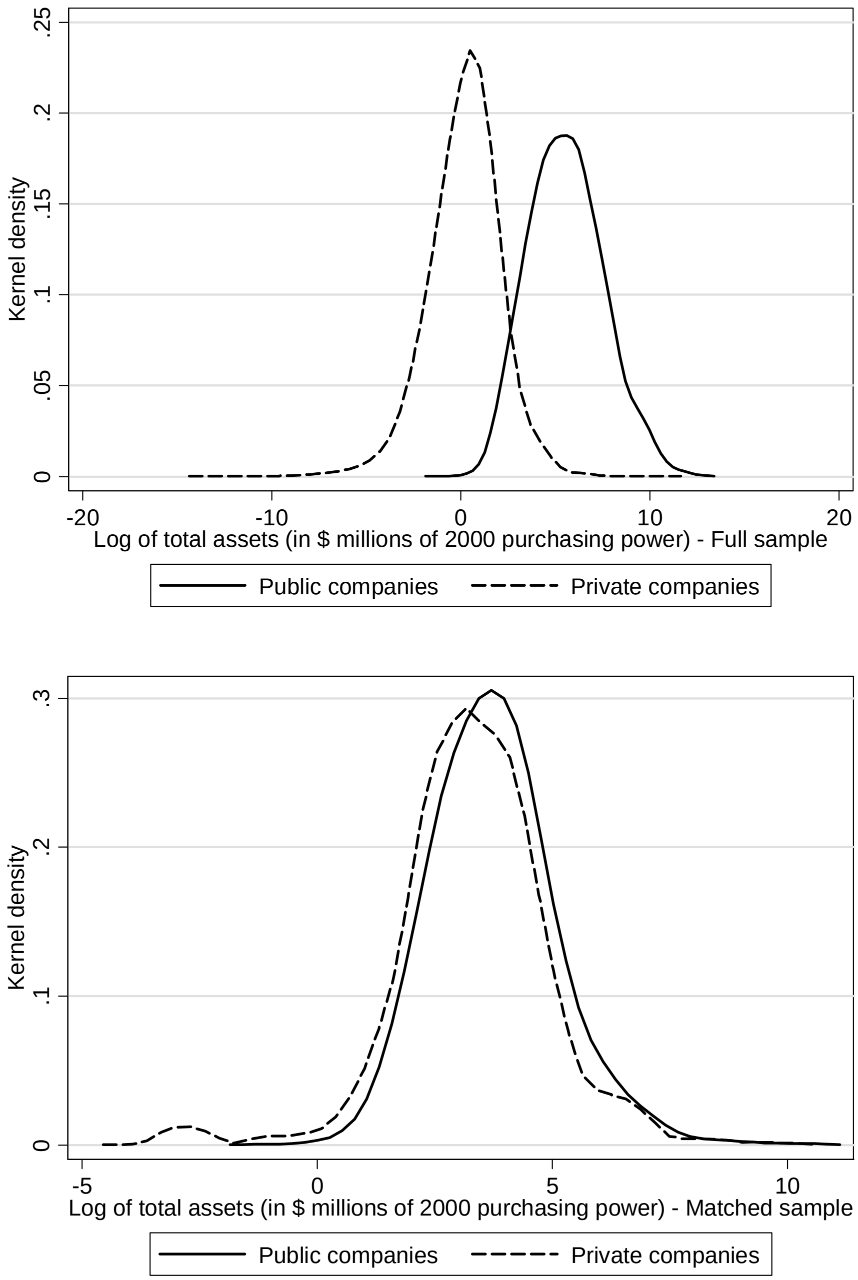

Table 1. Descriptive Statistics.

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

		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	
Industry *Q*	mean	1.747	1.398	0.349***	1.838	1.838	0.000
	median	1.579	1.235	0.344***	1.753	1.753	0.000
	st.dev.	0.840	0.613		0.740	0.740	
Predicted *Q*	mean	1.817	1.473	0.344***	2.119	1.964	0.155***
	median	1.778	1.385	0.393***	2.047	1.889	0.158***
	st.dev.	0.663	1.082		0.774	1.229	
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.

This table exploits within-firm variation to analyze differences in the sensitivity of investment spending to investment opportunities between public and private firms. 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 7 in Table 3. We use three different measures of investment opportunities: Sales growth, our preferred measure (columns 1 through 5); industry Q (column 6); and predicted Q (column 7). For variable definitions and details of their construction, see Appendix A. Note that we lose a small number of observations for four firms in column 7 due to missing leverage data, which is used in the construction of predicted Q. Following parts of the empirical investment literature, all specifications include ROA, 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 in these regressions. All regressions use a size-and-industry matched sample. Throughout the paper, we match on four-digit NAICS industries, except in columns 4 and 5 which investigate robustness to finer industry classifications. See Section 2.2 for further details of how the matched samples are constructed. All columns except 2 and 3 include both public and private firms, so we interact investment opportunities and ROA with a dummy equal to one if the firm is publicly traded. Each regression includes firm fixed effects 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 7, 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. All continuous variables are winsorized 0.5% in each tail to reduce the impact of outliers, except industry Q (which is a size-weighted average and so already downweights outliers) and predicted Q (which is itself constructed from winsorized data).

	Dependent variable: Gross investment / lagged total assets						
Measure of investment opportunities:	Sales growth					Industry Q	Predicted Q
Sample	All matched-sample firms (NAICS4)	Matched public firms	Matched private firms	All matched-sample firms (NAICS5)	All matched-sample firms (NAICS6)	All matched-sample firms (NAICS4)	All matched-sample firms (NAICS4)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Investment opportunities	0.136***	0.038***	0.134***	0.135***	0.186***	0.148***	0.383***
	0.013	*0.009*	*0.012*	*0.016*	*0.055*	*0.055*	*0.030*
Investment opp. x public	-0.097***			-0.099***	-0.143**	-0.147***	-0.226***
	0.015			*0.018*	*0.057*	*0.053*	*0.030*
ROA	0.173***	0.038	0.172***	0.171***	0.232***	0.112	0.519***
	0.014	*0.023*	*0.013*	*0.018*	*0.065*	*0.077*	*0.034*
ROA x public	-0.135***			-0.146***	-0.254***	-0.063	-0.342***
	0.027			*0.029*	*0.077*	*0.080*	*0.042*
R^2 (within)	29.6 %	5.5 %	42.5 %	26.5 %	17.4 %	15.1 %	28.1 %
Wald test: all coeff. = 0 (F)	32.1***	5.6***	36.4***	19.9***	2.9***	3.0***	15.1***
No. observations	9,950	4,975	4,975	8,640	2,924	9,950	9,931
No. firms	2,286	1,666	620	2,049	781	2,286	2,282

Table 3. Alternative Specifications.

As in Table 2, we use sales growth to proxy for investment opportunities and exploit within-firm variation using OLS 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 includes public firms' R&D spending in the dependent variable. Column 5 restricts the sample to C Corps in order to hold tax regime constant between public and private firms. Column 6 restricts the sample to firms using accrual-basis rather than cash accounting. In column 7, we change the dependent variable from gross to net investment (i.e., the change in net fixed assets over beginning-of-year total assets). In column 8, we 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 9 and 10, we use different matching criteria to generate the estimation sample. Column 9 matches on sales growth in addition to total assets and industry while column 10 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 A. 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. All continuous variables are winsorized 0.5% in each tail to reduce the impact of outliers.

	Dependent variable: Investment / lagged total assets									
	Lifecycle effects								Alternative matches	
	Old firms	Young firms	Matched public firms only	R&D effects, 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)	(10)
Investment opportunities	0.149***	0.131***	0.042*	0.042***	0.121***	0.131***	0.210***	0.092***	0.098***	0.081***
	0.016	*0.013*	*0.023*	*0.010*	*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*	-0.123**	0.159***	0.166***	-0.006	0.174***	0.124***	0.102***
	0.018	*0.015*	*0.077*	*0.037*	*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 %	6.5 %	34.0 %	19.3 %	50.0 %	32.4 %	18.1 %	11.7 %
Wald test: all coeff. = 0 (F)	26.6***	24.7***	5.0***	5.7***	15.1***	11.0***	27.3***	80.1***	7.8***	8.6***
No. observations	4,028	5,922	4,975	4,975	8,154	9,822	9,950	9,931	14,546	14,826
No. firms	984	1,498	1,666	1,666	1,913	2,250	2,286	2,282	4,213	4,427

Table 4. 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 adds ROA dated *t*-5 to *t*-3 to the instrument set. (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 in the estimated equation; however, for brevity, the table suppresses 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 A. 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. 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. All continuous variables are winsorized 0.5% in each tail to reduce the impact of outliers.

	Dependent variable: Gross investment / lagged total assets				
	Within-groups (fixed effects)	First diff. GMM, static	First diff. GMM, static	System GMM, static	First diff. GMM, dynamic
	(1)	(2)	(3)	(4)	(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 5. 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 B 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. All continuous variables are winsorized 0.5% in each tail to reduce the impact of outliers.

	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 6. Investment Sensitivities by Legal Form.

This table tests whether private firms in our sample are likely to be free of agency problems that could distort their investment decisions. Agency problems ultimately stem from a separation of ownership and control and from dispersed ownership. Since Sageworks does not report ownership information, we use legal form as a proxy for ownership concentration instead. Sole proprietorships, LLCs (limited liability companies), partnerships, and limited liability partnerships (LLPs) in the U.S. are overwhelmingly owner-managed and have highly concentrated ownership (see Table A8 in the Online Data Appendix). The other two legal forms open to private firms – C Corps and S Corps – can *theoretically* have dispersed ownership. These account for the bulk of our sample firms. We test for differences in investment sensitivities between C and S Corps on the one hand and the other types of private firms in our sample on the other. If the private C and S Corps in our sample were to have dispersed ownership and thus suffer from agency problems, their investment behavior should be systematically different from that of the other types of 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 which, by definition, have a single owner. 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. All continuous variables are winsorized 0.5% in each tail to reduce the impact of outliers.

	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 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 A. 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. All continuous variables are winsorized 0.5% in each tail to reduce the impact of outliers.

	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. Cross-industry Variation in Short-termism.

Short-termism models predict that the difference in investment sensitivities between public and private firms is zero for $\alpha_0 = 0$ and then increases in α_0, where α_0 measures how sensitive a public firm's stock price is to its current cash flows. We follow the accounting literature and use the earnings response coefficient (ERC) to capture a firm's stock price sensitivity and include a full set of interaction terms involving ERC in our baseline investment equation from Table 2. For details of how we construct ERC, see Appendix A. Panel A shows the results, using our matched sample of private and public firms. We report results for two separate measures of ERC, estimated at the Fama-French (1997) 30 industry level (row 1) and at the Fama-French 48 industry level (row 2). As before, the dependent variable is gross investment over lagged assets and the regression includes firm fixed effects and year effects (not reported) and is estimated using least-squares. Panel B illustrates the effect of ERC on investment sensitivities, as estimated in Panel A, for private and public firms at the 25th and 75th percentile of the ERC distribution (0.077 and 0.289, respectively, when using Fama-French 30 industries and 0.085 and 0.323, respectively, when using Fama-French 48 industries). 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 four-digit NAICS industry. We exclude government entities (NAICS 92XX, 5211, 4911, and 8131), pension funds (5251), REITs and other investment trusts (5259), and private households (8141). Data availability restricts us to a single cross-section, for 2007. The independent variables besides the industry's ERC are the industry's R&D intensity and its capital intensity. For details of their construction, see Appendix A. In column 1 of Panel C, the dependent variable is the ratio of the number of public firms in an industry. Specifically, we divide the number of firms in Compustat during fiscal year 2007 that belong to a particular NAICS4 industry by the total number of firms (public + private) active in that industry according to the 2007 "Statistics of U.S. Businesses" provided by the Census Bureau. We require that Compustat 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 no greater than 11, and report positive sales. This variable ignores differences in firm size and so can produce meaningless results. For example, 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 their approach and report results for firms with at least 100 employees (column 2) or at least 500 employees (column 3). Since we model fractions, Panel C is estimated using standard fractional logits (results are similar using OLS). Heteroskedasticity-consistent standard errors are shown in italics. In Panels A and B, they are clustered at the firm level. In Panel C, where the unit of observation is a four-digit NAICS industry, they are clustered at the Fama-French-30 industry level, the level at which ERC is measured. This is more conservative than clustering at the NAICS4 level. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively. In Panel C, we report marginal effects in brackets underneath the standard errors. They are computed for a one-standard deviation increase in the relevant variable, holding the other covariates constant, and should be compared to the sample mean of the dependent variable, reported in the last row of Panel C. The number of firm-years in Panels A and B is 9,950 and the number of firms is 2,286. The number of NAICS4 industries in Panel C is 283. In Panel A, all continuous variables are winsorized 0.5% in each tail to reduce the impact of outliers.

Panel A: Interaction results

Row	Industry definition	Sales growth	Sales growth x public	Sales growth x ERC	Sales growth x ERC x public	ERC	ERC x public	ROA	ROA x public	R^2 (within)	*F*-test: all coef. = 0
1	Fama-French 30 industries	0.099***	-0.033	0.208	-0.373**	0.017	-0.010	0.168***	-0.139***	30.6%	13.1***
		0.031	*0.036*	*0.154*	*0.174*	*0.058*	*0.057*	*0.019*	*0.028*		
2	Fama-French 48 industries	0.106***	-0.048	0.189	-0.298*	0.082	-0.064	0.167***	-0.135***	30.6%	14.1***
		0.028	*0.032*	*0.157*	*0.166*	*0.053*	*0.056*	*0.019*	*0.028*		

Panel B: Implied investment sensitivities

	Low ERC (25th percentile)		High ERC (75th percentile)		Difference	
	coeff.	std. error	coeff.	std. error	coeff.	std. error
Fama-French 30 industries:						
Private firms	0.115***	*0.021*	0.160***	*0.023*	0.044	*0.033*
Public firms	0.054***	*0.013*	0.019*	*0.010*	-0.035**	*0.017*
Difference	0.062***	*0.025*	0.141***	*0.025*	0.079**	*0.037*
Fama-French 48 industries:						
Private firms	0.122***	*0.018*	0.167***	*0.030*	0.045	*0.037*
Public firms	0.049***	*0.012*	0.023**	*0.009*	-0.026*	*0.013*
Difference	0.073***	*0.021*	0.144***	*0.031*	0.071*	*0.039*

Panel C: Public firms' industry shares and ERC

	Fraction of firms in a four-digit NAICS industry that are public, measured by the number of firms		
	all firms (1)	with 100 or more employees (2)	with 500 or more employees (3)
ERC	-4.408**	-5.271***	-5.110***
	1.865	*1.661*	*1.960*
	[-0.001]	[-0.015]	[-0.033]
industry R&D intensity	15.916***	20.625***	26.348***
	1.611	*2.846*	*4.361*
	[0.001]	[0.017]	[0.048]
industry capital intensity	6.800***	4.607***	4.334***
	1.858	*1.236*	*1.156*
	[0.003]	[0.018]	[0.038]
R^2	62.1 %	40.6 %	30.3 %
Sample mean of dep. var.	0.006	0.046	0.099

Table 9. Income Smoothing and Dividend Policy.

This table tests whether public firms have smoother profit growth and smoother payout policies than private firms. 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 A. 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.

This table tests 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 A.) 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 fractions	*Z*-statistic
	# observations	fraction	# observations	fraction		
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 fractions	*Z*-statistic
	# observations	fraction	# observations	fraction		
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***