Comparing the Investment Behavior of Public and Private Firms [*] [†]

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

Joan Farre-Mensa
Harvard Business School

Alexander Ljungqvist
Stern School of Business
New York University
and *NBER*

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[†] 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.

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Abstract

We evaluate differences in investment behavior between stock market listed and privately held 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 matched private firms, especially in industries in which stock prices are particularly sensitive to current earnings. These differences do not appear to be due to unobserved differences between public and private firms, how we measure investment opportunities, lifecycle differences, or our matching criteria. We suggest that the patterns we document are consistent with theoretical models emphasizing the role of managerial myopia.

Key words: Corporate investment; *Q* theory; Private companies; Managerial incentives; Agency costs; Short-termism; Managerial myopia; IPOs.

JEL classification: D22; D92; G31; G32; G34.

This paper compares the investment behavior of stock market listed (or 'public') firms to that of comparable privately held firms, using a novel panel dataset of private U.S. firms covering around 335,000 firm-years over the period 2001-2011. Almost everything we know about investment at the micro level is based on evidence from public firms,[1] which number only a few thousand, yet private firms form a substantial part of the U.S. economy.[2] We estimate that in 2010, private U.S. firms accounted for 52.8% of aggregate non-residential fixed investment, 68.7% of private-sector employment, 58.7% of sales, and 48.9% of aggregate pre-tax profits. Nearly all of the 5.7 million firms in the U.S. are private (only 0.06% are listed), and many are small, but even among the larger ones, private firms predominate: Among those with 500+ employees, for example, private firms accounted for 86.4% in 2010.[3]

Our empirical tests unearth two new patterns. First, nearest-neighbor matching reveals that private firms grow substantially faster than public ones, holding firm size and industry constant. The average investment rate among private firms is nearly twice as high as among public firms, at 6.8% versus 3.7% of total assets per year. Second, private firms' investment decisions are more than four times more responsive to changes in investment opportunities than are those of public firms, based on standard investment regressions in the tradition of tests of the *Q* theory of investment (see Hayashi (1982) or, more recently, Gomes (2001), Cummins, Hassett, and Oliner (2006), Bloom, Bond, and van Reenen (2007), or Bakke and Whited (2010)). Both results are robust to various alternative matching approaches.

The striking difference in investment sensitivities does not appear to be driven by how we measure investment opportunities. When we exploit a plausibly exogenous tax shock to the user cost of capital, which sidesteps the need to directly measure investment opportunities, we find that private firms respond strongly to changes in investment opportunities whereas public firms barely respond at all.

We find similar patterns when we exploit within-firm variation in listing status for a sample of firms that go public without raising new capital. This differences away time-invariant firm-level unobservables.

[1] Most studies of investment dynamics use firm-level data from Compustat and so focus on public firms. The exceptions are studies that use plant-level data from the Census of Manufactures (Caballero et al. (1995) and Cooper and Haltiwanger (2006)).
[2] Private firms should not be confused with venture capital-backed firms. The latter are a subset of the population of private firms, but they are few in number: Of the around 5.7 million private firms operating in the U.S. in 2010, fewer than 3,000 were funded by VCs. VC-backed firms come from a narrow set of industries and are not representative of private firms in general.
[3] The denominators in these estimates are from the National Income and Product Accounts (http://www.bea.gov/national) and the Statistics of U.S. Businesses (http://www.census.gov/econ/susb). The numerators are based on CRSP-Compustat data for U.S. corporations listed on a national exchange (the NYSE, AMEX, or Nasdaq). The sales data are from 2007, the most recent year for which they are available.

The identifying assumption of this test is that these firms go public purely in order to change their ownership structure (that is, the expressed intention is to allow existing owners to 'cash out'). The within-firm results show that IPO 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 observably similar, already-public firms.

What would cause public and private firms to invest so differently? We consider three leading explanations – lifecycle effects, financial constraints, and agency problems – while emphasizing that they are neither exhaustive nor necessarily mutually exclusive.

We find that our results are robust to conditioning on age and the ratio of the firm's retained earnings to its total assets, a popular measure of a firm's lifecycle stage (DeAngelo, DeAngelo, and Stulz (2006)). This suggests that the observed differences in investment behavior are not a result of public firms being systematically older and more mature than private firms. That said, we cannot rule out a more sophisticated version of the lifecycle hypothesis that involves firms going public in response to a decline in their growth opportunities and investment sensitivity, regardless of their age or maturity. However, this hypothesis seems to contradict the evidence that most U.S. firms use IPOs to raise capital (Ljungqvist and Wilhelm (2003)) which their CFOs say is to be used to fund investment (Brau (2012)).

Alternatively, as a consequence of not having access to the stock market, private firms may simply be more financially constrained than public firms. However, the fact that private firms invest more than public ones, holding size and industry constant, does not obviously suggest that private firms are more financially constrained.

The third potential explanation we examine is agency. While a stock market listing provides access to a deep pool of low cost capital, this can have two detrimental effects. First, ownership and control must be at least partially 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 makes it easy for shareholders to sell their stock at the first sign of trouble rather than actively monitoring management – a practice sometimes called the 'Wall Street walk.' This weakens incentives for effective corporate governance (Bhide (1993)).

Private firms, in contrast, are often owner-managed and even when not, are both illiquid and typically have highly concentrated ownership, which encourages their owners to monitor management more closely. Indeed, analysis of the Federal Reserve's 2003 Survey of Small Business Finances (SSBF) shows that 94.1% of the larger private firms in the survey have fewer than ten shareholders (most have fewer than three), and 83.2% are managed by the controlling shareholder.[4] According to another survey, by Brau and Fawcett (2006), keeping it that way is the main motivation for staying private in the U.S. As a result, agency problems are likely to be greater among public firms than among private ones.

There are three strands of the agency literature that argue public firm's investment might be distorted due to agency problems. First, Baumol (1959), Jensen (1986), and Stulz (1990) argue that managers have a preference for scale which they satisfy by 'empire building.' Empire builders invest regardless of the state of their investment opportunities. This could explain the lower investment sensitivity we observe among public firms.

Second, Bertrand and Mullainathan (2003) argue the opposite: Managers may have a preference for the 'quiet life.' When poorly monitored, managers may 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 when a public-firm manager derives utility from both the firm's current stock price and its long-term value. Short-termism can lead to either too much or too little investment. Overinvestment results when the manager has better information about the high quality of his investment opportunities, which he signals by overinvesting (e.g., Bebchuk and Stole (1993)). Under-investment results if investors have incomplete information about how much the firm *should* invest to maximize its long-term value and how much it actually *does* invest (see Miller and Rock (1985), Narayanan (1985), Stein (1989), Shleifer and Vishny (1990), von Thadden (1995), and Holmström (1999)). Essentially, by underinvesting the manager tries to create the impression that the firm's profitability is greater than it really is, hoping this will boost today's share price (Stein (1989)).

The fact that we find *lower* investment levels among public firms seems inconsistent with empire

[4] Contrast this with the fact that the average (median) public-firm CEO in our sample owns a mere 3.1% (0.66%) of his firm's equity, and the average (median) public firm has 35,550 (1,210) shareholders.

building. The quiet life argument and those short-termism models that predict underinvestment, on the other hand, fit the empirical facts we document. To shed further light on what drives the observed investment difference between public and private firms, we explore how it varies with a parameter that plays a central role in short-termism models: The sensitivity of share prices to earnings news. As we explain in Section 4, under short-termism a public-firm manager has no incentive to underinvest if current earnings are uninformative about future earnings, in which case we expect no difference in investment behavior. But the more sensitive share prices are to earnings news, the greater the incentive to distort investment and hence the greater the difference in public and private firms' investment sensitivities.

These predictions can be tested using what the accounting literature calls 'earnings response coefficients' or ERC (Ball and Brown (1968)). For industries in which share prices are unresponsive to earnings news (ERC = 0), we find no significant difference in investment sensitivities between public and private firms. As ERC increases, 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. These cross-sectional patterns are consistent with the notion that public firms invest myopically.

Our paper makes three contributions. First, we document economically important differences in the investment behavior of private and public firms. Because few private firms have an obligation to disclose their financials, relatively little is known about how private firms invest. A potential caveat is that our analysis focuses on the segments of public and private firms that overlap in size and industry, so we essentially compare large private firms to smaller public firms. To what extent do our results extend to larger public firms? We show that the low investment sensitivity among smaller public firms is typical of the investment behavior of all but the largest decile of public firms, which are substantially more sensitive to investment opportunities than the public firms in the other nine deciles.

Second, we provide rare direct evidence of an important potential cost of a stock market listing by documenting that the investment of public firms in our sample appears to be distorted relative to that of comparable private firms. Calling it a distortion assumes that private firms, carefully screened to be observably similar, provide a good benchmark for how public firms would behave were their ownership

and control more closely aligned. The data support this assumption.

Third, our analysis suggests that agency problems in public firms, and in particular short-termism, are an important driver of these differences (though we emphasize that other forces such as life-cycle effects or financial constraints might also play a role in explaining our results). This finding adds to existing survey evidence of widespread short-termism in the U.S. Poterba and Summers (1995) find that public-firm managers 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' (2009) model shows that the presence of large shareholders can reduce managerial myopia. But it is an empirical question whether these mechanisms are sufficiently effective on average. Our evidence suggests that, at least on the dimension of investment, this may not be the case.

The paper proceeds as follows. Section 1 briefly reviews related literature. Section 2 introduces a rich new database of private U.S. firms created by Sageworks Inc. Section 3 establishes our main empirical results, that public firms invest less and are less responsive to changes in investment opportunities than private firms. Section 4 investigates possible explanations for these findings. Section 5 concludes.

1. Related Literature

There is a small but growing empirical literature contrasting public and private firms. Using data for the population of British firms, Saunders and Steffen (2009) show that private firms face higher borrowing costs than do public firms; Michaely and Roberts (2012) show that private firms smooth dividends less than public firms; and Brav (2009) shows that private firms rely mostly on debt financing.

Before Sageworks became available, studies of private U.S. firms relied on limited samples. Gao, Lemmon, and Li (2010) compare CEO compensation in public and private firms in the CapitalIQ database, 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 survey evidence showing that private firms are subject to fewer agency problems than public firms, as well as with Edgerton's (2012) finding that public firms overuse corporate jets compared to observably similar private firms.

We are aware of two recent papers comparing the investment behavior of public and private firms in the U.S. Sheen (2009) analyzes hand-collected investment data for public and private firms in the chemical industry, finding results similar to ours. Gilje and Taillard (2012), on the other hand, find that public firms in the natural gas industry are more responsive to changes in natural gas prices than private firms. Our multi-industry study is able to reconcile these seemingly contradictory findings by empirically showing that the exposure to agency-driven investment distortions differs across industries.

The empirical literature on the effects of agency costs on investment, surveyed in Stein (2003), is vast. We depart from it by exploiting variation along the extensive (public/private) margin. Existing work in this area focuses instead on the intensive margin. For example, Wurgler (2000), Knyazeva et al. (2007), Franzoni (2009), Bøhren et al. (2009), and Gopalan et al. (2010) relate investment among *public* firms to variation in corporate governance, while Fang, Tian, and Tice (2010) 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.

Finally, the accounting literature documents that *some* public-firm managers *sometimes* act myopically, in the specific 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. 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.

2. Sample and Data

According to the Census, there were 5,734,538 firms in the U.S. in 2010.[5] The vast majority of these are privately held (in 2010, there were only 3,716 U.S. firms with a listing on a U.S. exchange) and even among the very largest private firms, most express no desire to go public.[6] Unless they have issued public bonds or have more than 500 shareholders (2,000 shareholders since April 2012), private firms are not subject to mandatory disclosure requirements, so little is known about how they invest. Our study is only possible 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 the data, along with detailed summary statistics, in the Online Data Appendix.[7]

In structure, Sageworks resembles Compustat, a standard database covering 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 which input data for *all* their unlisted corporate clients directly into Sageworks' database. Selection thus operates at the level of the accounting firm and not of their clients. Sageworks co-operates with most of the largest national accounting firms as well as 100s of regional players, but with proportionately fewer of the many thousand local accountants who service the smallest firms in the U.S. As a result, the main selection effect is that Sageworks' coverage is biased towards large private firms. Figure 1 illustrates this by comparing Sageworks firms to the universe of U.S.

[5] This figure does not include the self-employed. (Source: http://www.census.gov/econ/susb)
[6] In Brau and Fawcett's (2006) survey of large private firms, only 10.5% had considered going public.
[7] The data appendix is available at http://papers.ssrn.com/sol3/papers.cfm?abstract_id=1659926.

firms, as captured by the National Establishment Time Series (NETS) database.[8] Much of the mass of Sageworks firms is to the right of NETS firms in terms of log sales. This selection may be problematic for some research questions but it is innocuous for us given that our goal is to compare the investment behavior of public firms to that of observably similar private firms. Sageworks started in 2000 with fiscal year 2001 being the first panel year. We have data through fiscal year 2011 and use 2001 to construct lags, giving a ten-year, unbalanced panel.[9] Figure 2 shows how the database has grown over time.

2.1 Sample Construction

Sageworks contains panel data for 239,327 private firms. To construct our private-firm sample, we exclude 14,346 Canadian firms, 647 firms located in U.S. territories such as Guam, 530 firms without known location, 3,110 non-profits, 32,686 firms whose data are incomplete or violate basic accounting identities, and 617 firms with missing or negative total assets. As is customary, we further exclude 25,572 financial firms (the NAICS equivalent to SIC 6) and 1,577 regulated utilities (SIC 49). Finally, we keep only firms with at least three consecutive annual observations so that we can construct lags and still have at least two panel years of complete data, as our empirical models exploit within-firm variation. This leaves 99,040 private firms and 307,803 firm-years over the period from 2002 to 2011.

To be part of our public-firm sample, a firm has to be recorded in both Compustat and CRSP during our sample period; be incorporated in the U.S. and listed on a major U.S. exchange (NYSE, AMEX, or Nasdaq); have valid stock prices in CRSP in three consecutive years; 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); and be neither a financial firm nor a regulated utilities (SIC 49). These filters leave us with 4,360 public firms and 29,718 firm-years in 2002-2011.

2.2 Matching Procedure

2.2.1 Objective

In an ideal world, we would compare the investment behavior of two versions of the same (originally

[8] NETS contains data on employment, estimated sales, location, industry, and founding year for approximately 18 million firms in the U.S. The underlying data come from Dun & Bradstreet, a credit reference agency. NETS does not contain data on investment and so cannot be used as a substitute for Sageworks in our empirical tests.

[9] 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.

private) firm, one which goes public and another which remains private. To get close to this ideal, we look for pairs of public and private firms that are observably similar to each other on dimensions, denoted Z_{it}, that are likely to affect investment. To ensure common support, we use a matching procedure to control for Z_{it}. This has the advantage of avoiding having to impose a specific functional form on the way Z_{it} affects investment. Given the markedly different distributions of some of the Z_{it} variables among public and private firms, controlling for the effect of these variables on investment in a linear regression setting appears a hopeless task.

2.2.2 Choice of Matching Variables

To implement our matching estimator, we need to select the elements of Z_{it} that we are to match on. Our goal is to find variables that affect investment and that, importantly, are not themselves directly affected by the firm's listing status. Matching on variables that are affected by a firm's listing status would mask the effect of listing status on investment (see Heckman, LaLonde, and Smith (1999)).[10]

A large body of work, surveyed in Jorgenson (1971), documents cross-industry variation in investment, while, more recently, Gala and Julio (2011) report evidence of an inverse relation between firm size and investment. Building on this stream of literature, our preferred match is based on size and industry.[11] These are two dimensions that, economically, have a large effect on investment and whose ranges and distributions are very different for the public and private firms in our sample.[12] That said, we consider various alternative sets of matching variables for robustness. Note that matching on size means that our matched sample consists of *small* public and *large* private firms.

2.2.3 Matching Algorithm

In the language of the matching literature surveyed in Imbens and Wooldridge (2009), we use a caliper-based nearest-neighbor match adapted to a panel setting. Starting in fiscal year 2002, for each public firm, we find the private firm that is closest in size and that operates in the same four-digit NAICS industry, requiring that the ratio of their total assets (TA) is less than 2 (i.e., $\max(TA_{public}, TA_{private})$ /

[10] For this reason, our baseline match avoids matching on "endogenous" variables such as cash holdings or leverage. (Recall that Brav (2009) shows that private firms rely more on debt financing than public ones.)
[11] As we will discuss shortly, data constraints prevent us from matching on age.
[12] See Figure 3 (discussed below) for a comparison of the size distribution of the public firms in Compustat and the private firms in Sageworks, and Table A15 in the Online Data Appendix for a comparison of their industry distributions.

$\min(TA_{public}, TA_{private}) < 2$).[13] If no match can be found, we discard the observation and look for a match 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. In particular, this will allow us to estimate the within-firm sensitivity of investment to investment opportunities. We match with replacement, though our results are not sensitive to this.[14] If a matched private firm exits the panel, a new match is spliced in.

The resulting matched sample contains 11,372 public-firm-years and an equal number of private-firm-years. Due to matching with replacement, the sample contains 2,595 public firms and 1,476 private firms.

2.2.4 Match Quality

Not surprisingly, before matching, the public firms in Compustat are larger than the privately held firms in Sageworks. The top graph in Figure 3 shows the distribution of total assets in log 2005 dollars for each dataset. The distributions overlap only to a limited extent. Table 1 shows that the mean (median) public firm has total assets of $2,869.4 million ($392.2 million), compared to $13.5 million ($1.2 million) for private firms.

As the bottom graph in Figure 3 shows, matching produces size distributions that are nearly identical. To test this formally, we report two standard statistical measures of match quality. The first is Rosenbaum and Rubin's (1985) *SDIFFF* test, which relies on standardized differences to evaluate the extent to which size differs across our matched public and private firms (industry, by construction, cannot differ). The test measures the scaled difference in means of total assets (our matching variable) between matched public and private firms. While critical values have not yet been derived, Rosenbaum and Rubin suggest that a value of 20 is 'large', which would warrant concern about the extent to which the matched groups are balanced. The value of *SDIFF(size)* is 1.44, suggesting that our matched sample is balanced. The second test of match quality is the Hotelling T^2 test. The data cannot reject the null that the means of total assets are equal for the two matched groups (p=0.276).

2.3 Measures of Investment Opportunities

The investment literature proxies for a firm's investment opportunities using either Tobin's *Q* or sales

[13] As we will show, our results are robust to using finer industry classifications, such as NAICS5 or NAICS6.
[14] As Smith and Todd (2005) point out, matching with replacement involves a trade-off between bias and efficiency. Bias is reduced as higher quality matches are generated, but efficiency is reduced as fewer distinct observations are used.

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 thus favor sales growth, which can be constructed at the firm level for any firm, whether public or private. Sales growth has been widely used as a measure of investment opportunities. See, for example, Rozeff (1982), Lehn and Poulsen (1989), Martin (1996), Shin and Stulz (1998), Whited (2006), Billett, King, and Mauer (2007), Bloom, Bond, and van Reenen (2007), and Acharya, Almeida, and Campello (2007).

For robustness purposes, we also explore two *Q* measures. The first 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, return on assets (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 Measures of 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 focus on CAPEX, but there is good reason 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 (see Maksimovic, Phillips, and Yang (2012) for evidence consistent with this hypothesis). Sageworks data do not allow us to distinguish between CAPEX and M&A, so we cannot directly test this in our sample. 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 *gross investment*, defined as the annual increase in gross fixed assets scaled by beginning-of-year total assets. For robustness, we will also model *net investment*, defined analogously using net fixed assets. The difference between the two is depreciation. To the extent that depreciation schedules can be somewhat arbitrary, gross investment better captures the

firm's investment decisions.[15] (For detailed definitions of these and all other variables, see Appendix A.)

2.5 Other Firm Characteristics

Table 1 shows that private firms have higher profits, less cash, more debt, and more retained earnings, even after we match on size and industry. It is important to note that we should *not* expect public and private firms to look identical on these dimensions *even if* our matching approach allowed us to replicate the ideal experiment in which the firms' listing status were randomly assigned. The reason is that these characteristics are likely endogenous, either to listing status (e.g., dependence on debt) or to a firm's investment behavior (e.g., profits). Regardless, as we will show, the observed differences in profits, cash, debt, and retained earnings do not drive our results.

3. Differences in Public and Private Firm Investment Behavior

We begin by documenting sizable differences in investment levels between private and public firms, using our size-and-industry matched sample and a range of alternative samples matched on a wider choice of characteristics. We then show that public and private firms differ in their responses to changes in their investment opportunities using standard investment regressions in the style of the *Q*-theory literature. These differences in investment behavior are robust to a range of potential confounds, including concerns that age or lifecycle effects may contaminate inference; differences in tax treatment and accounting choices; and differences in observable characteristics such as size, cash holdings, or debt.

A standard concern in investment regressions is that investment opportunities are measured with error. To address this, we use a natural experiment that exogenously varies the user cost of capital and so sidesteps the need to directly measure investment opportunities. This test confirms that public firms have much lower investment sensitivities. Finally, we report a test that uses within-firm variation in listing status, using hand-collected data for firms that switch from private to public status without raising new capital (a particular form of IPO).

3.1 Differences in Investment Levels: Baseline Results and Alternative Matches

Table 2 compares public and private firms' investment levels. It shows that private firms invest

[15] 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.

significantly more than public firms on average. The differences are substantial. Row 1 shows that in the full samples, private firms increase their gross fixed assets by an average of 7.5% of total assets a year, compared to 4.1% for public firms. Matching on size and industry, as shown in row 2, does not close the gap: Private firms continue to out-invest public firms, by 6.8% to 3.7% on average.[16] The same is true when we focus on net investment, which is 2.9 percentage points higher per year among private firms.

Differences in medians are much smaller, mainly because neither the median private nor the median public firm invests much. However, our results are not obviously driven by outliers: When we compare the investment of public and private firm-years with above-median investment, we find that private firms outspend public firms at each point in the investment distribution.

These patterns are robust to matching on finer industry codes: Using 5-digit NAICS has virtually no effect (row 3) while using 6-digit NAICS narrows the difference in investment from 3.1 percentage points in favor of private firms to 2.4 percentage points (row 4). Including other characteristics besides size and industry among the matching criteria does not close the gap either:[17] When matched on size, NAICS4 industry, and sales growth, private firms invest 8.5 percentage points more than public firms (row 5), and when additionally matched on ROA, cash holdings, and book leverage, the difference is 2.3 percentage points (row 6).[18]

3.2 Differences in Investment Levels: Potential Confounds

The final four rows of Table 2 address three potential confounds. The first concerns potential lifecycle effects. Public firms might simply be more mature than private ones, and so may have reached that point in their lifecycle when investment naturally slows down. We address this potential confound in two ways. First, we augment our size-and-industry matching criteria with a popular measure of a firm's lifecycle stage: The ratio of the firm's retained earnings to its total assets (*RE/TA*). As DeAngelo, DeAngelo, and Stulz (2006) note, this variable "is a logical proxy for the lifecycle stage at which a firm currently finds

[16] In fact, private firms invest substantially and significantly more in *every* sample year except during the financial crisis of 2009 and in 2011. Using gross investment, they outspend public firms by between 1.5% and 7.5% a year

[17] Throughout the paper, whenever we match on other variables in addition to industry and size, we define a propensity score that is based on size and the additional matching variables. We then adapt the matching algorithm described in Section 2.2.3 as follows: For each public firm, we find the private firm with the closest propensity score that operates in the same four-digit NAICS industry, imposing a 0.05 caliper.

[18] This specification is included for robustness. As outlined in Section 2.2, matching on endogenous variables such as cash holdings and leverage is problematic, as these variables are directly affected by a firm's listing status (see, e.g., Brav (2009)).

itself because it measures the extent to which the firm is self-financing or reliant on external capital." These authors show that firms with low *RE/TA* ratios tend to be at the growth and capital-raising stages of their lifecycles while firms with high *RE/TA* ratios tend to be more mature, highly profitable, and hence largely self-financing. As the results in row 7 show, controlling for *RE/TA* in fact *increases* the gap in investment by another percentage point compared to the baseline in row 2, to 4.1% ($p<0.001$).

Another proxy for maturity is firm age. Controlling for age directly is not straightforward, as neither Compustat nor Sageworks reports founding dates. To get around this data limitation, we hand-collect founding years for every one of the 29,718 firm-years in the Compustat sample from firm directories, google searches, and corporate websites. As Sageworks lacks firm names, we cannot search these sources for private firms' founding dates. Instead, we exploit the National Establishment Time Series (NETS) database, which offers comprehensive coverage of the U.S. economy and does report firm age. To be conservative in matching Sageworks firms to NETS, absent firm names or other unique identifiers, we restrict the sample to cases where there is a unique Sageworks firm and a unique NETS firm in a given zip code and NAICS5 industry. This yields imputed age information for 9,467 Sageworks firm-years.[19]

Over the 2002-2011 sample period, the average Compustat firm is 40.7 years old and the average Sageworks firm that can be matched to NETS is 30.8 years old. Compustat firms thus appear to be significantly older on average, and this could potentially account for the observed differences in investment levels. However, the results shown in row 8 suggest that differences in age cannot explain the gap in investment levels. When matched on age, size, and industry, public firms invest 4.7% a year on average, significantly less than the 7.4% investment rate we see among observably similar private firms.

The final two rows address potential confounds due to differences in legal form and accounting choices. Private firms can be organized as sole proprietorships, limited liability companies (LLCs), or partnerships or be incorporated under Subchapters C or S of the Internal Revenue Code, while virtually all public firms are C Corps. When we focus on the subset of private incorporated firms, we continue to find a large gap in investment levels, averaging 3.1 percentage points in favor of private firms (row 9). Finally,

[19] Alternatively, we could relax the unique-firm condition and choose the NETS firm in the same zip code/NAICS5 industry with the closest level of sales. However, NETS sales data are generally Dun & Bradstreet's estimates rather than actuals, so matching Sageworks and NETS on sales, industry, and location is likely to produce noisy matches.

when we exclude private firms that use cash-basis (rather than accrual) accounting, the difference also remains large, at 3.2 percentage points (row 10).

3.3 Differences in Investment Sensitivity: Baseline Models

To investigate differences in public and private firms' sensitivity to changes in investment opportunities, we estimate standard investment regressions of the following general form:

$$\frac{I_{it}}{A_{it-1}} = \frac{K_{it} - K_{it-1}}{A_{it-1}}$$
$$= \alpha\left(\frac{S_{it} - S_{it-1}}{S_{it-1}}\right) + \beta\left\{PUBLIC_i \times \left(\frac{S_{it} - S_{it-1}}{S_{it-1}}\right)\right\} + \delta\left(\frac{Z_{it}}{A_{it-1}}\right) + \phi\left\{PUBLIC_i \times \left(\frac{Z_{it}}{A_{it-1}}\right)\right\} + \mu_i + \eta_t + \varepsilon_{it}$$

where I is gross investment,[20] K is gross fixed assets, A is total assets, S is sales, Z is operating income before depreciation,[21] and *PUBLIC* is a dummy for whether the firm is publicly listed. We remove unobserved time-invariant heterogeneity through firm fixed effects, and we include year effects.[22] Standard errors are clustered at the firm level in the usual manner.

The results in column 1 of Table 3 suggest that public firms' investment decisions are significantly *less* sensitive to changes in investment opportunities. The coefficient estimate is 0.118 for private firms, 4.4 times greater than the 0.118 – 0.091 = 0.027 coefficient estimate for matched public firms. The difference between these estimates is statistically significant at the 0.2% level.

One possible interpretation of these findings is that public firms invest differently than private firms. Another is that our investment model is simply better specified for private firms than for public ones. To investigate this further, column 2 reports the results of estimating the investment model in the sample of matched public firms only. This reveals that our model of investment for public firms is as good as those in published work that uses public U.S. firms (see, for example, Shin and Stulz (1998)). In particular, the sensitivity of investment to sales growth for public firms is 0.028 with a *t*-statistic of 4.72. This is

[20] As we will show later, we obtain similar results using net investment instead and when we include R&D spending and other forms of investment in intangibles, which are left out of gross fixed investment.

[21] The variable *Z/A* is ROA. Prior work shows that standard proxies for investment opportunities are not, as neoclassical theory predicts, a sufficient statistic for investment and that ROA correlates positively with investment. The latter is often interpreted as a sign of financing constraints (Fazzari, Hubbard, and Petersen (1988)), though some disagree (Kaplan and Zingales (1997)). While we are agnostic about the debate surrounding its interpretation, we follow the literature by including ROA.

[22] Recall that all firms in Sageworks are unlisted and all firms in Compustat are listed. Thus, our fixed-effects specifications cannot include a public-firm indicator, though we can let the effect of investment opportunities or ROA vary by listing status.

considerably lower than the 0.118 coefficient estimated for matched private firms in column 1.

To see if our matched sample inadvertently oversamples public firms with low investment sensitivities, column 3 re-estimates the investment model in the full sample of 29,718 public firm-years. This yields an estimated investment sensitivity that is only marginally larger than in the matched sample of public firms, at 0.037 (*t*-statistic = 7.72), substantially below that of private firms.

Another way to investigate this concern is to let investment sensitivities vary with firm size. To this end, we assign all public firms in the full sample to one of ten size deciles based on their total assets in their first panel year. (Not surprisingly, the public firms that end up in our matched sample are small by public-firm standards: 71.3% come from decile 10, 12.4% from decile 9, and 16.3% from the remaining eight deciles.) Figure 4 plots the estimated investment sensitivity for each decile. The coefficients of around 0.04 in the bottom nine deciles are significantly different from zero but not from each other. In this sense, the public firms that end up in our matched sample appear to be representative of 90% of public firms. The coefficient estimated for size decile 1, the largest public firms, is 0.163, four times the magnitude of the sensitivity in the bottom nine deciles and nearly six times the magnitude we observe among public firms in our matched sample. Thus, our results appear representative of most public firms but do not extend to the very largest public firms, whose investment is (even) more sensitive to investment opportunities than that of private firms in our sample.

It is possible that investment among all but the very largest public firms was "unusually" insensitive during our time period and so is unrepresentative of public firms' investment behavior in more "normal" times. To see if this is the case, we re-estimate the public-firm investment model shown in column 3 over consecutive ten-year windows starting in 1970 using Compustat data for all eligible publicly traded firms.[23] Figure 5 shows the resulting coefficient estimates, along with 95% confidence intervals, for the coefficient on sales growth in our investment regression. This reveals that the low public-firm investment sensitivity shown in Table 3 is *not* specific to our sample period: It has been a feature of public-firm behavior since the 1980s. Public-firm investment sensitivities have declined from a high of more than 0.1 in the 1970s and mid-1980s to 0.049 in the 1982-1991 window. They have stayed below 0.05 in every

[23] The Compustat data are filtered exactly as in our 2002-2011 Compustat sample of 29,718 firm-years. Samples sizes vary from a low of 20,666 firm-years in the 1970-1979 window to a high of 43,173 firms-years in the 1993-2002 window.

ten-year window since; and they have not been as high as the 0.118 point estimate we find for private firms in 2002-2011 for more than two decades.

Might our results be driven by our use of sales growth to proxy for investment opportunities? While Tobin's *Q* cannot be computed for private firms for obvious reasons, we can use either an industry-based *Q* and or a predicted *Q* as an alternative proxy for investment opportunities. Columns 4 and 5 show that our results continue to hold in either case. In light of this, and because sales growth is the more appropriate proxy in our setting, the remainder of the paper reports results using only sales growth.[24]

The main takeaway from Table 3 is the apparent existence of a wide gap in the investment sensitivities of public and private firms. In an effort to eliminate this gap, Section 3.4 considers variations on our matching algorithm, Section 3.5 investigates the possibility that public and private firms simply differ in the speed with which they respond to changes in investment opportunities, Section 3.6 tests for a broad range of potential confounding factors, Section 3.7 examines potential biases due to error in the measurement of investment opportunities, and Section 3.8 uses an alternative sample of firms that go public. None of these further tests can eliminate the observed gap in investment sensitivities.

3.4 Alternative Matching Approaches

3.4.1 Alternative Matching Variables

Columns 6 and 7 of Table 3 show that our results are robust to matching on finer industry codes. Using NAICS5 (rather than NAICS4 as in column 1) widens the gap in investment sensitivities a little, from 9.1 to 10.9 percentage points, while using NAICS6 reduces the gap a little, to a still highly significant 7.8 percentage points. Our findings thus do not appear to be driven by an overly noisy industry match, so we match on NAICS4 in the remainder of the paper.

Columns 8 and 9 expand the set of matching variables. Column 8 matches on sales growth in addition to size and industry, while column 9 matches on size, industry, sales growth, ROA, cash holdings, and book leverage.[25] Our inference remains unchanged. The point estimates of –0.055 (p=0.001) and –0.078 (p=0.007) imply that private-firm investment is between 2.4 and 2.7 times more sensitive to changes in

[24] Complete results using the two *Q* measures are available on request.

[25] The specification in column 7 is included for robustness. Recall from Section 2.2 that matching on endogenous variables such as cash holdings and leverage can mask the effects of listing status on investment (see Heckman, LaLonde, and Smith (1999)).

investment opportunities than investment among public firms with similar characteristics.

3.4.2 Variations in the Matching Algorithm

Any matching estimator involves choices. Table IA.1 in the Internet Appendix investigates the effects of our particular choices. It shows that our results are unchanged if instead of splicing in a new match when a private firm exits the sample, we do not splice in a new match; if we match without replacement; if we use the five nearest neighbors instead of a single nearest neighbor; if we impose ever tighter calipers on the maximum difference in firm size between public and private matched firms; and if we match using a propensity score based on total assets rather than on total assets itself. In each of these variations, we find that public-firm investment is significantly less sensitive to changes in investment opportunities than is private-firm investment. The magnitude of the difference in investment sensitivities ranges from 4.9 percentage points to 10.1 percentage points and is consistently highly statistically significant.

Table IA.1 also provides a lower bound for the difference in investment sensitivities by estimating the investment model in the full samples of public and private firms (i.e., without any attempt at matching). Even in this setting, we find that public-firm investment is significantly less sensitive to changes in investment opportunities than is private-firm investment – even though the full sample of private firms contains a large number of quite small companies that invest very little.

3.5 Investment Dynamics

It is possible that public firms simply respond more slowly to changes in investment opportunities than private firms. In Table 4, we investigate whether such differential investment dynamics might bias the observed investment sensitivities found in Table 3. Column 1 reports a Wooldrige test of autocorrelation in panel data for the baseline results from Table 3, column 1. The test indicates the presence of modest autocorrelation in the data (p=0.087). In an attempt to soak up this autocorrelation, columns 2 and 3 add a further lag of our proxy for investment opportunities. To allow firms to differ in the speed with which they respond to changes in investment opportunities, we interact this lag with the public-firm indicator. Column 2 (like column 1) is estimated using OLS with firm and year fixed effects. Column 3 is estimated using the Blundell and Bond (1998) system GMM estimator, which jointly estimates a first-differenced equation (instrumented with lags dated t-1 and earlier of ROA and lags dated

t-2 and earlier of sales growth) and an equation in levels (instrumented with lagged differences).

Allowing for differential investment dynamics does not affect our findings. In column 2, neither public nor private firms respond significantly to the additional lag, and the difference in contemporaneous investment sensitivities remains large, at 7.3 percentage points, and highly statistically significant (p=0.034). The Wooldridge test rejects further autocorrelation (p=0.609). In column 3, using GMM, we find some evidence that private (but not public) firms are sensitive to past changes in investment opportunities, though the effect is small economically and weak statistically (p=0.073). Compared to the OLS specifications, the difference in contemporaneous investment sensitivities becomes larger (11.2 percentage points, p=0.042). The GMM specification passes the standard specification tests.

Column 4 adds the lagged dependent variable to the column 3 specification to allow for richer investment dynamics. Investment is significantly related to its lag (p=0.013), consistent with the Wooldridge test in column 1, but allowing for this in fact strengthens our conclusions: While private firms respond strongly to contemporaneous changes in investment opportunities (p<0.001) and moderately to lagged ones (p=0.001), public firms respond significantly less strongly contemporaneously and not at all to lagged changes. The difference in investment sensitivities is 8.3 percentage points contemporaneously and 2.9 percentage points lagged.

Thus, differences in the speed with which firms pursue investment opportunities do not appear able to explain the observed difference in investment sensitivities between public and private firms.

3.6 Potential Confounds

It is possible that even after matching, public and private firms differ in some unobserved way that would cause the observed difference in investment sensitivities. We next consider the leading potential confounding variables.

3.6.1 Lifecycle Effects

While we have found no evidence that differences in maturity or age can explain the observed difference in investment *levels* between public and private firms, they could potentially generate differences in investment sensitivities. 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 less mature than public firms, such lifecycle effects could confound our results. As before, we use the mix of retained and contributed capital (*RE/TA*) and firm age to attempt to control for lifecycle differences. Columns 1 and 2 of Table 5 report the results.

When we match on *RE/TA* in addition to size and industry, the estimated difference in investment sensitivities between public and private firms is –0.070 (p<0.001). This is marginally smaller than in the baseline specification (–0.091), but remains large economically. Matching on age in addition to size and industry yields similar results. The estimated difference in investment sensitivities becomes –0.032 (p<0.001). In either case, lifecycle differences do not appear to be an important confound.

3.6.2 Intangibles

So far, we have focused on investment in fixed assets. Firms also invest in intangibles, such as R&D, advertising, and goodwill. We now test if systematic differences between public and private firms in the types of assets they invest in can account for the observed difference in fixed investment sensitivities.

Sageworks does not report R&D spending. Prior work links 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', perhaps 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 hypothesis directly, we can assess its plausibility indirectly. To do so, we test if including public-firm R&D spending in the dependent variable eliminates the difference in investment sensitivities between public and private firms.

Column 3 in Table 5 shows that it does not. The estimated difference in investment sensitivities is −0.082 when we allow public firms to respond to changes in investment opportunities through R&D, only marginally smaller than the point estimate of –0.091 when we omit R&D (cf. Table 3). On its own, R&D is thus insufficient to close the gap in investment sensitivities between public and private firms. Closing the gap would require that private firms' R&D spending, which we do not observe, correlated *negatively* with investment opportunities. While this is possible, there is no compelling reason to think it is likely.

In column 4, we add investment in advertising to gross investment in fixed assets for both public and private firms. This actually widens the gap in investment sensitivities to –0.100 (p=0.004), suggesting that private firms' advertising expenditure is more sensitive to changes in investment opportunities.

Unlike Compustat, Sageworks does not report data on goodwill. In column 5, the dependent variable is (gross investment + advertising + R&D + change in goodwill) for public firms and (gross investment + advertising) for private firms, scaled by lagged total assets. This horserace is clearly rigged in favor of public firms. But even when we allow public (but not private) firms to respond to changes in investment opportunities through R&D and/or investment in goodwill, we fail to eliminate the gap in investment sensitivities, which is estimated at –0.084 (p=0.017).

Overall, these tests suggest that the observed differences in fixed investment sensitivities cannot easily be attributed to potential differences in the types of assets public and private firms invest in.

3.6.3 Differences in Legal Form or Accounting Choices

While virtually all public firms are incorporated, close to 16% of the private firms in the full Sageworks sample are not (unincorporated firms include sole proprietorships, LLCs, and partnerships). To see if differences in legal form between public and private firms may be driving our results, column 6 restricts the private-firm sample prior to matching to incorporated firms. This actually increases the difference in private and public firms' investment sensitivity to −0.107 (p=0.003).

Column 7 holds accounting choices constant by excluding private firms that use cash-basis (rather than accrual) accounting. This leaves the coefficient of interest barely changed, at −0.088 (p=0.003).

In column 8, we model net rather than gross investment and continue to find that private firms are more sensitive to changes in investment opportunities than are public firms (p=0.007).

3.6.4 Overseas Activities

Column 9 removes public multinationals before creating the matched sample. The aim is to address the possibility that investment opportunities are measured with greater error for firms that operate internationally, which could lead to a downward bias in the estimate of public firms' investment sensitivity. The results suggest the opposite: If anything, the difference in private and public firms' investment sensitivity increases, from –0.091 in the baseline specification to –0.095 here (p=0.001).

3.6.5 Controlling for Further Observable Differences

Finally, we test if observable differences between public and private firms that remain after matching on size and industry can account for observed differences in investment behavior. For example, firms with more cash or less debt might 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 have less cash and more debt than public firms.

Column 10 adds cash holdings and leverage as additional regressors. 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. In this augmented specification, the coefficient for the difference in investment sensitivity between public and private firms is nearly identical to our baseline estimate: –0.091 (p=0.002).

3.7 Errors in the Measurement of Investment Opportunities

Across all specifications considered in Tables 3-5, we find the same result: Public firms appear to be substantially less responsive to changes in investment opportunities. We next investigate if measurement error in our proxy for investment opportunities might spuriously generate this pattern. Specifically, we exploit a natural experiment which helps us sidestep the need to directly measure investment opportunities altogether. This experiment involves plausibly exogenous variation in corporate tax rates. While federal taxes change relatively rarely, there is considerable variation in state corporate income taxes across time. Moreover, state taxes change in a staggered way that can be usefully exploited for identification in the spirit of Bertrand and Mullainathan (2003). A cut in a state's corporate income tax reduces the user cost of capital for firms operating in that state, which should boost investment, and vice versa for tax increases. Changes in state taxes can thus be viewed as shocks to firms' after-tax returns on investment and thus to their investment opportunity sets.

Are tax changes plausibly exogenous? While tax cuts could, potentially, reflect political economy factors (such as firms lobbying the state legislature), it is hard to see why firms would ever lobby for tax increases. As we will show, both tax cuts and tax increases affect investment in our sample.

Our tax experiment asks if private firms are more sensitive to changes in state corporate income taxes

than public ones, in the sense that they increase investment by more when taxes are cut and decrease investment by more when taxes are raised. We examine this question using a difference-in-difference approach that lets firms' investment sensitivity to tax changes vary by listing status. We limit our sample of private firms to C Corps, given that in most states only C Corps are subject to state corporate income taxes, and rerun our baseline matching algorithm using this sample. Using data obtained from the Tax Foundation and a search of the "Current Corporate Income Tax Developments" feature published periodically in the *Journal of State Taxation*, we identify 27 tax cuts and 13 tax increases over our sample period in a total of 20 states; see Appendix B for details. For example, Kentucky cut its corporate income tax rate from 8.25% to 7% from January 2005.

We code a firm as being affected by a tax change if the state it is headquartered in passed a tax cut or tax increase that came into effect during the fiscal year in question. Since Compustat reports a firm's current – as opposed to historic – location, we verify public firms' historic headquarter states using 10-K and 10-Q filings obtained from the SEC's EDGAR database.[26] This results in corrections to 363 of the 9,764 public firm-years. Overall in our industry-and-size-matched sample, 380 public and 366 private firms are affected by a tax cut, while 188 public firms and 226 private ones are affected by a tax increase. The average size of tax cuts affecting firms in our matched sample is –0.55 percentage points, while the average size of tax increases is 0.64 percentage points.

Table 6 reports the results. We use a continuous measure of tax changes, defined as the change in the top marginal corporate income tax rate (this measure is naturally 0 in years with no tax changes). In column 1, we find that private firms – but not public ones – significantly increase investment spending in response to tax cuts and reduce it in response to tax increases. Compared to the 6.4% unconditional mean investment rate for private C Corps in our matched sample, the point estimate is quite large. All else equal, a one percentage point increase (decrease) in the top marginal state corporate income tax rate in a private C Corp's home state is associated with a decrease (increase) in investment of 4% of total assets. By contrast, public firms' analogous sensitivity is much lower, at 0.4 percentage points (–0.040 + 0.036),

[26] Sageworks' location data is historic and so poses no difficulty.

and not statistically significant (p=0.556).[27]

Column 2 models tax cuts and tax increases separately. The results are similar: Private firms respond to tax changes while public ones do not. The point estimates are somewhat larger, in absolute terms, for tax cuts than for tax increases, but the data cannot reject the null that the effects are symmetric (p=0.503).

Column 3 explores possible pre- and post-trends by adding variables capturing tax changes that will occur in a year's time or that occurred a year ago. Neither public nor private firms show any sign of anticipating future tax changes in their investment behavior or of responding with a lag.

Before we can interpret these patterns as evidence of private firms showing greater sensitivity to exogenous changes in investment opportunities, we have to consider two possible confounds. The first is due to unobserved differences in geographic dispersion. States levy taxes on all corporate activities within their jurisdiction, regardless of where a firm is headquartered. For example, a firm headquartered in VT with a plant in ME will pay taxes in ME for the income generated by the ME plant. If public firms more often operate in multiple states, their firm-level investment decisions will be less sensitive to a tax change in their home state. This could potentially explain the lower tax sensitivity of public firms.

To investigate this potential confound, we hand-collect data on the geography of public firms' operations from Item 2 of their 10-K filings. Of the 568 public firms that are hit with a state corporate income tax shock, 169 are single-state and 399 are multi-state firms as of the beginning of the year in which taxes changed. In column 4, we let public firms' sensitivity to tax changes vary according to whether they are a multi-state or a single-state firm. The point estimates are similar and not statistically different from each other (p=0.810). Compared to private firms' average tax sensitivity of 4 percentage points (in absolute value), the tax sensitivities of single-state and multi-state public firms are a significant 3.2 and 3.7 percentage points lower on average, respectively. Thus, this potential confound does not appear to drive the differential tax sensitivity of public and private firms.

A second potential confound is due to the cash flow consequences of a tax change. Tax changes affect

[27] Prior studies focus on the effect of tax changes on either public-firm or aggregate investment. Like us, Gruber and Rauh (2007) find a zero elasticity of investment to (federal) tax changes in Compustat data. Using cross-country data, Djankov et al. (2010) show that aggregate investment is sensitive to taxes: A 10% increase in tax leads to a 2% drop in aggregate investment to GDP (mean: 21%). If public-firm investment appears, at least locally, to be insensitive to tax changes, then sensitivity at the aggregate level may be driven, at least in part, by greater sensitivity among private firms.

not only the after-tax user cost of capital but also how much cash flow is available for investment. If private firms are more financially constrained than public firms, tax changes could affect their ability to invest differently even if investment opportunities remained unchanged. To investigate whether this could explain the observed differences in tax sensitivity, column 5 controls for tax-induced changes in cash flows by including the change in the firm's tax payments between year *t*-1 and *t*, scaled by total assets. While private firms' investment does appear to be sensitive to the cash flow effects of tax changes, controlling for this cash flow effects leaves the sensitivity of their investment to changes in tax rates virtually unchanged. This finding suggests that our results in columns 1 through 4 are not driven by the cash flow consequences of tax changes, and it is thus consistent with our interpretation that private firms' investment is more sensitive to variation in the cost of capital induced by changes in tax rates.

Finally, column 6 reports an indirect validity test of our tax identification strategy. In most states, only C Corps are subject to state corporate income taxes. Thus, tax changes should have little effect on the investment behavior of private *non*-C Corps. This is precisely what we find: The coefficient estimate for the tax change variable in our full sample of non-C Corp firms is –0.004 with a standard error of 0.004.

The results of the tax experiment suggest that private firms are more responsive to changes in investment opportunities than are public firms, even when we sidestep the need to measure investment opportunities and instead exploit exogenous variation in firms' after-tax user cost of capital.

3.8 Within-Firm Variation in Listing Status

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.

In light of this, we explore an alternative research designs that exploits within-firm variation in listing

status. To do this, we examine how a *given* firm's investment behavior changes as it transitions from private to public status. This allows us to remove unobserved time-invariant heterogeneity using firm fixed effects.[28] Of course, going public is not a natural experiment: Most firms go public for reasons that correlate with investment – usually to fund a planned increase in investment (Brau and Fawcett (2006)). To mitigate problems of the IPO decision affecting investment directly, we focus on firms that go public *without raising capital*. These firms sell stock belonging to their existing shareholders and so experience increased ownership dispersion and, of necessity, some degree of separation of ownership and control.

This within-firm test reduces identification concerns, but it cannot eliminate them. Unfortunately, the obvious instruments for the IPO decision do not work in our setting. In principle, one could exploit discontinuities around stock exchange listing standards to identify the effects of a listing on investment.[29] 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.[30] Alternatively, one could use the 2002 Sarbanes-Oxley Act as an exogenous shock to the cost of being public, because compliance with its Section 404 effectively acts as a tax on being a small publicly traded firm. However, our sample post-dates 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. 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

[28] Gertner, Powers, and Scharfstein (2002) 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 firms.
[29] Exploiting discontinuities around delisting thresholds instead 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 (2011)).
[30] See http://www.nasdaq.com/about/nasdaq_listing_req_fees.pdf.

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 their IPOs were of the kind that only affected their ownership structure. This finding is consistent with the patterns we found in the matched Sageworks-Compustat sample.

Investment sensitivities could change for macroeconomic reasons and not because of the IPO itself. To allow for this, columns 3 and 4 report diff-in-diff tests, combining data from the IPO sample with data for size-and-industry-matched already-public firms. While we cannot rule out that the two groups of 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 become indistinguishable from those of matched already-public firms.

4. Potential Explanations

The results of the three identification strategies reported in Section 3 – matching on observables, 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. Our goal in this section is to investigate possible explanations for this new stylized fact.

To the extent that we have ruled out lifecycle differences as a likely explanation, there remain two principal dimensions on which public and private firms differ that could affect investment: Access to capital and agency problems. These are two key elements of the trade-off firms face when deciding whether to go public or remain private. On the one hand, a stock market listing provides access to a deep pool of low cost capital. On the other hand, it can lead to agency conflicts between managers and shareholders as the firm sells stock to dispersed investors who have little say in the firm's governance. This situation is potentially aggravated by the fact that the ease with which its shares can now be traded may induce shareholders to sell rather than monitor managers. We explore each element of the trade-off

in turn, while recognizing the possibility that other channels might also be at play.

4.1 Differences in Access to Capital

It is an open empirical question whether private firms are more financially constrained than comparable public firms. Clearly, private firms cannot raise equity from public-market investors without going public. But they are able to raise equity from their own shareholders or from private-market investors such as VCs or growth equity funds and (via the 144A private placement market) from qualified institutional buyers without facing the selective disclosure restrictions that the SEC imposes on public firms (e.g., Regulation FD). They can also raise debt, from lenders or the bond markets.

Financial constraints are not directly measurable. Hadlock and Pierce (2010) assess whether a firm is financially constrained by searching the annual reports of 356 public firms for evidence of firms identifying themselves as financially constrained. The most reliable markers of financial constraints in their sample are firm size and age. To the extent that we match firms on size throughout our analysis, and that our results are robust to including age as a matching variable, one could argue that our matching approach helps control (at least partially) for differential financial constraints between public and private firms. Moreover, our results are also robust to matching on *RE*/*TA*, which DeAngelo, DeAngelo, and Stulz (2006) argue captures "the extent to which the firm is self-financing or reliant on external capital."

Almeida, Campello, and Weisbach (2004) argue that the effects of financial constraints can be captured by a firm's propensity to save cash out of cash flows. They show that constrained firms have a positive cash flow sensitivity of cash while unconstrained firms do not. In unreported tests (available on request), we find that the cash flow sensitivity of cash of the private firms in our matched sample is statistically and economically zero and that the difference in the cash flow sensitivity of cash between public and private firms is statistically insignificant. These results reinforce the notion that our matching approach allows us to compare the investment behavior of public and private firms that have similar access to external capital.

Finally, recall from Section 3.1 that private firms invest substantially more in nearly *every* sample year, except during the financial crisis of 2009 and in 2011, when they invested as much as public firms. This casts some doubt on the notion that private firms can only pursue investment opportunities when

macro conditions are favorable.

In conclusion, while we recognize the tentative nature of this discussion, it seems unlikely that differences in access to capital between the matched public and private firms in our sample can account for the observed differences in investment behavior.[31]

4.2 Agency Problems

We now investigate the role of agency problems in explaining our findings. The introduction discusses several agency models that predict suboptimal investment behavior among public firms. Of these, two are consistent with the lower levels of investment we find among public firms: A preference for a quiet life and certain types of short-termism models. In this section, we ask if agency problems are a plausible cause of the patterns we find and if so, which agency problem is most likely.

For agency problems to be able to explain the patterns we document, it must be the case that private firms are less prone to agency problems. We first present evidence consistent with this premise. We then report cross-sectional evidence that is consistent with public-firm managers' short-termism being an important driver of the observed differences in investment sensitivities between public and private firms.

4.2.1 Do Private Firms Have Fewer Agency Problems?

As Sageworks provides no ownership data, we cannot directly compare ownership structures between our public and private firms. Fortunately, Sageworks reports legal form and as a point of law, three legal forms correlate strongly with concentrated ownership and little separation between ownership and control and hence little occasion for an agency problem to arise: Sole proprietorships have a single owner and are by definition owner-managed. For tax purposes and to limit their liability, many sole traders choose LLC status (according to the Federal Reserve's SSBF survey, 67.3% of LLCs are owner-managed). And partnerships give each partner the statutory right to participate in management (in the SSBF, around 90% are owner-managed). In each of these legal forms, comprising 16% of the private firms in our full Sageworks sample, there is essentially no separation of ownership and control and so little possibility of agency problems distorting investment. This leaves two legal forms that can *theoretically* have dispersed ownership: C Corps can have an unlimited number of shareholders while S Corps can have up to 100.

[31] We stress that our tests compare large private firms to small public firms. Financing constraints may well have an important impact on the investment behavior of the vast number of small private firms in the economy.

Our null hypothesis, which is informed by evidence from the SSBF, is that private C and S Corps in practice have concentrated ownership. The alternative hypothesis is that such firms have dispersed ownership and hence potentially suffer from similar agency problems as public firms. If so, we should find that their investment behavior is systematically different from that of the other legal forms.

Table 8 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.

For robustness, 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. Columns 4 and 5 widen the definition of agency cost-free firms by comparing sole proprietorships, LLCs, and partnerships 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 large 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 suggests that the private firms (including C and S Corps) in our sample suffer from fewer investment-distorting agency problems than do the public sample firms.

4.2.2 Short-termism: Theory

These results open the door to agency considerations, and in particular a preference for a quiet life or short-termism, being a driver of the investment differences between public and private firms reported in Section 3. We can shed further light on the importance of such agency considerations by putting some structure on the nature of the possible short-termism. In common with other short-termism models, Stein (1989) assumes that a public-firm manager derives utility from the firm's current stock price as well as

from its long-term value.[32] This gives him an incentive to 'manipulate' the current stock price. Since the stock price equals the present discounted value of the firm's future cash flows, he will try 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 increase their expectations of future cash flows in response. The mechanism in short-termism models is not fraudulent accounting (Kedia and Philippon (2009)) but underinvestment. Specifically, Stein (p. 657) 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 have incomplete information about which projects the firm should invest in to maximize its long-term value and how much it actually does invest in them. As a result, they do not know the extent of underinvestment but they understand the manager's incentives. Investors thus realize that the reported high current cash flows will in fact lead to lower future cash flows than they would in a world without agency problems. In response, they 'discount' the manager's report of current earnings. And yet in equilibrium, the manager will still 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.

4.2.3 Testable Implications

Models of short-termism in the Stein (1989) tradition predict that the extent of a public firm's investment distortion 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. This yields 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

[32] 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) or Bolton, Scheinkman, and Ziong (2006) 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)).

no point investing myopically. Second, as α_0 increases, current cash flows become more and more informative about future cash flows, increasing the incentive to manipulate investors' expectations by underinvesting, and hence leading to greater distortions. 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. To the extent that the α_0 parameter plays no role in alternative stories, these predictions are unique to short-termism.

4.2.4 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)). ERC measures the sensitivity of a firm's stock price to earnings news and so maps directly into α_0. As described more fully in Appendix A, we follow Easton and Zmijewski (1989) and estimate a set of industry ERCs for each year from 2001 to 2010. Industries are defined using Fama and French's (1997) 30 industry groups, which contain enough firms each to estimate ERCs relatively precisely. We also report somewhat noisier results using their finer 48 industries, which contain fewer firms in each industry. We then include a full set of interaction terms involving lagged industry ERCs in our matched-firm investment regression.

Table 9 reports the results. In row 1, using Fama-French 30 industries, we find no significant difference in investment sensitivities between public and private firms for ERC = 0: The point estimate for private firms is 0.113 (p=0.005) while that for public firms is a modest and insignificant 0.021 lower (p=0.676). This pattern is consistent with the predicted absence of distortions for $\alpha_0 = 0$. As ERC increases, public firms' investment sensitivity decreases significantly (p=0.034) while that of private firms remains unchanged (p=0.812). 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.[33] Row 2 shows that the results are similar when we use Fama-French 48 industries.

4.2.5 Interpretation

The findings in Table 9 are consistent with the interpretation that short-termism plays an important role in explaining the observed differences in investment behavior between public and private firms in our

[33] Note that ERC affects investment in Table 9 only through its interaction with investment sensitivities and not directly. This suggests that ERC does not capture financial constraints or some omitted dimension of investment opportunities.

sample. Our findings that public and private firms exhibit similar investment sensitivities when $\alpha_0 = 0$ and that the difference in their sensitivities increases in α_0 fit Stein's (1989) model of short-termism but, as far as we know, are not predicted by other hypotheses that may help explain the reported differences in investment behavior between public and private firms, such as a preference for a quiet life.[34]

In view of this evidence, it seems sensible to consider myopic incentives as a plausible first-order element contributing to the patterns we observe in the data. That said, we caution against interpreting our findings as evidence that short-termism is the *only* driver of our findings. In particular, we stress that our tests do not allow us to conclusively rule out that differences in financial constraints or lifecycle stage between public and private firms might play an incremental role in explaining why public firms invest less and in a way that is less sensitive to investment opportunities compared to matched private firms.

5. Conclusions

This paper compares the investment behavior of comparable public and private firms, matched primarily on size and industry. Our results show that relative to private firms, comparable public firms invest considerably 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 earnings news.

These findings are robust to matching on other variables in addition to size and industry, as well as to using a variety of matching algorithms. They do not appear to be due to time-invariant unobserved differences between public and private firms: 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 how we measure investment opportunities or to age differences between public and private firms.

Our findings are consistent with the presence of a particular type of agency problem. Theorists have long argued that the separation of ownership and control that accompanies a stock market listing can lead to agency problems between managers and dispersed stock market investors and hence to suboptimal

[34] In addition to the hypotheses discussed so far, other potential channels that may explain the observed differences in investment between public and private firms include 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. Alternatively, private firms could be capital inefficient, inexperienced at making investment decisions, or closet empire-builders. None of these alternative channels seems to predict our Table 9 findings.

investment decisions. The literature is divided on whether over- or underinvestment will result, or indeed whether effective corporate governance mechanisms can be devised to ensure investment is not distorted (Tirole (2001), Shleifer and Vishny (1997)). Our results are most consistent with the view that public firms' investment decisions are affected by managerial short-termism, which leads them to underinvest.

We are careful not to claim causality. Short of a trial that randomly assigns firms to public or private status, we cannot categorically rule out endogeneity concerns. For example, a private firm whose manager is more prone to succumb to short-termist pressures might also be more likely to go public. We also emphasize that, in addition to short-termism, other forces such as life-cycle effects or financial constraints might contribute to the patterns we observe. That said, it is worth noting that public firms are not only less responsive to investment opportunities than private firms, but that this difference is increasing in the sensitivity of stock prices to earnings news in a firm's industry – a direct implication of models of short-termism in the Stein (1989) tradition. This suggests that short-termism plays a first-order role in generating the patterns we observe in our novel dataset.

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Appendix A. Variable Definitions

Total assets is Compustat item *at* or its Sageworks equivalent, *TotalAssets*. It is reported in $ millions of 2005 purchasing power, deflated using the annual GDP deflator.

Gross investment is the annual increase in gross fixed assets (Compustat data item *ppegt* or its Sageworks equivalent, *GrossFixedAssets*) 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, *NetFixedAssets*) scaled by beginning-of-year nominal total assets.

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: $Sales_{it}/Sales_{it\text{-}1} - 1$ (using Compustat item *sale* or its Sageworks equivalent, *Sales*).

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, *Sales* – *CostOfSales* – *Payroll* – *Rent* – *Advertising* – *Overhead* + *OtherOperatingIncome* – *OtherOperatingExpenses*) 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, *Cash*), 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, *ShortTermDebt* + *CurrentLongTermDebt* + *LongTermLiabilities*), scaled by beginning-of-year total assets.

RE/TA is retained earnings (Compustat item *re* or its Sageworks equivalent, *RetainedEarnings*), scaled by total assets.

Age is years since founding. Age is not available in either Compustat or Sageworks. For public firms, we hand-collect founding dates from regulatory filings, business directories, and a comprehensive search of online and offline sources. For private firms, we match Sageworks firms to NETS, a database containing founding dates for approximately 18.8 million firms in the U.S. Recall that all firms in Sageworks are anonymous. The only variables that are in both Sageworks and NETS are zip codes, five-digit NAICS, and sales. NETS sales, however, are mostly estimates rather than actuals, so it is unlikely that matching on sales, industry, and location would produce valid matches. To be conservative, we restrict the sample to cases where there is a unique Sageworks firm and a unique NETS firm in a given zip code NAICS5 industry.

Multinational is constructed as in Foley et al. (2007). It is an indicator set equal to 1 if the firm reports paying foreign income taxes (Compustat variable *txfo* non-zero and non-missing) or reports having foreign income (Compustat variable *pifo* non-zero and non-missing), and zero otherwise.

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 2010 by regressing abnormal returns SAR_{ijtq} on a constant and on unexpected earnings UE_{ijtq} using all firms *i* in industry *j*, requiring a minimum of 10 firms per industry. *ERC* for industry *j* in year *t* is the coefficient estimated for UE_{ijtq}. SAR_{ijtq} is firm *i*'s size-adjusted abnormal return in the three-day window centered on the day the firm announced quarterly earnings *q*. UE_{ijtq} 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.)

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 6. We list all changes that occurred within one year either side of our sample period (2002-2011) to allow for the construction of leads and lags. In states with more than one tax bracket, we report the change to the top bracket; lower tax brackets were usually also affected. To identify these changes, we use data obtained from the Tax Foundation (an abbreviated version of which is available at http://www.taxfoundation.org) and a search of the "Current Corporate Income Tax Developments" feature published periodically in the *Journal of State Taxation*, and verify the resulting information using the relevant state's Department of Revenue and Legislature websites.

State	Date	Brief description of tax change
Tax increases:		
AL	1/2001	Increase in top corporate income tax rate from 5% to 6.5%
AR	1/2003	Introduction of 3% tax surcharge on tax liability
CT	1/2003	Introduction of 20% tax surcharge on tax liability
CT	1/2004	Increase in tax surcharge on tax liability to 25%
IL	1/2011	Increase in top corporate income tax rate from 4.8% to 7%
IN	1/2003	Repeal of gross income tax (based on revenue rather than profits) and of supplemental income tax; effective adjusted gross income tax rate (on profits) increased from 7.75% to 8.5%
KS	1/2002	Increase in tax surcharge on taxable income from 3.35% to 4.5%
MD	1/2008	Increase in top corporate income tax rate from 7% to 8.25%
NC	1/2009	Introduction of 3% tax surcharge on tax liability
NH	7/2001	Increase in top corporate income tax rate from 8% to 8.5%
NJ	7/2006	Introduction of 4% tax surcharge on tax liability
OR	1/2009	Increase in top corporate income tax rate from 6.6% to 7.9%
TN	7/2002	Increase in top corporate income tax rate from 6% to 6.5%
Tax cuts:		
AR	1/2005	Repeal of 3% tax surcharge
AZ	1/2001	Reduction in top corporate income tax rate from 7.968% to 6.968%
CT	1/2006	Reduction in tax surcharge from 25% to 20%
CT	1/2008	Repeal of 20% tax surcharge
ID	1/2001	Reduction in top corporate income tax rate from 8% to 7.6%
KS	1/2003	Reduction in tax surcharge from 4.5% to 3.35%
KS	7/2008	Reduction in tax surcharge from 3.35% to 3.1%
KS	7/2009	Reduction in tax surcharge from 3.1% to 3.05%
KS	7/2011	Reduction in tax surcharge from 3.05% to 3%
KY	1/2005	Reduction in top corporate income tax rate from 8.25% to 7%
KY	1/2007	Reduction in top corporate income tax rate from 7% to 6%
MA	1/2010	Reduction in top corporate income tax rate from 9.5% to 8.75%
MA	1/2011	Reduction in top corporate income tax rate from 8.75% to 8.25%
NC	1/2011	Repeal of 3% tax surcharge
ND	1/2004	Reduction in top corporate income tax rate from 10.5% to 7%
ND	1/2007	Reduction in top corporate income tax rate from 7% to 6.5%
ND	1/2009	Reduction in top corporate income tax rate from 6.5% to 6.4%
ND	1/2011	Reduction in top corporate income tax rate from 6.4% to 5.4%
NJ	7/2010	Repeal of 4% tax surcharge
NY	7/2000	Reduction in top corporate income tax rate from 8.5% to 8%
NY	7/2001	Reduction in top corporate income tax rate from 8% to 7.5%
NY	7/2007	Reduction in top corporate income tax rate from 7.5% to 7.1%
OR	1/2011	Reduction in top corporate income tax rate from 7.9% to 7.6%
VT	1/2006	Reduction in top corporate income tax rate from 9.75% to 8.9%
VT	1/2007	Reduction in top corporate income tax rate from 8.9% to 8.5%
WV	1/2007	Reduction in top corporate income tax rate from 9% to 8.75%
WV	1/2009	Reduction in top corporate income tax rate from 8.75 to 8.5%

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. Comparing the size distribution of private firms in Sageworks and in NETS.

This graph compares the size distribution of private firms in the full sample of Sageworks and in the National Establishment Time Series (NETS), a database that contains data on employment, estimated sales, location, industry, and founding year for approximately 18 million firms in the U.S. (The underlying data come from Dun & Bradstreet, a credit reference agency.) We perform the comparison in 2008, the year when the coverage of Sageworks is largest. Given that NETS does not contain data on total assets, we use sales to measure size. The graph presents, for each set of firms, Epanechnikov kernel densities of the natural logarithm of sales in $ millions of 2005 purchasing power. The width of the kernel density window around each point is set to 0.4. The unit of observation is a firm.

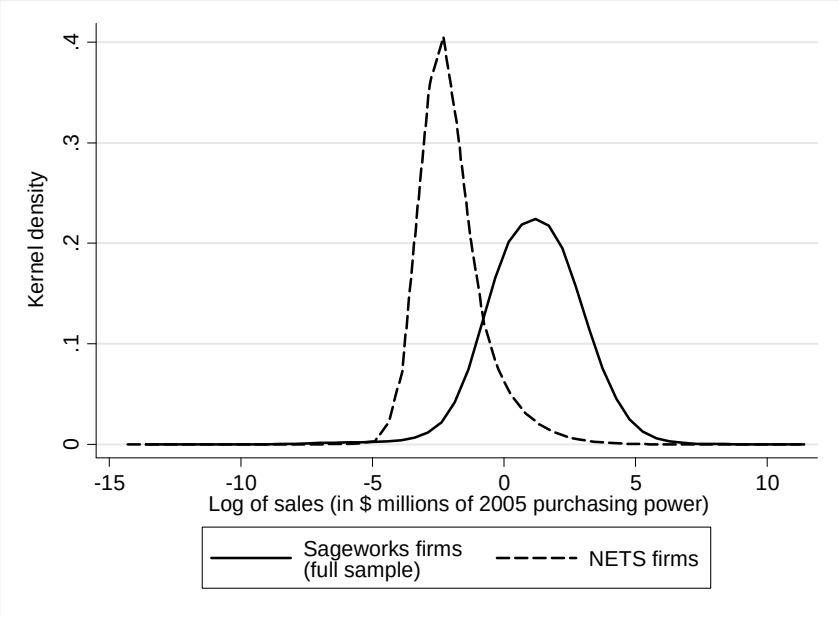

Figure 2. The Sageworks Dataset: Distribution by Year.

This graph illustrates the growth of the Sageworks database over time by showing the distribution by year of the 307,803 firm-year observations in the full Sageworks sample, corresponding to 99,040 unique firms over the period from 2002 to 2011. The figure also reports the number of firms entering and exiting the sample per year.

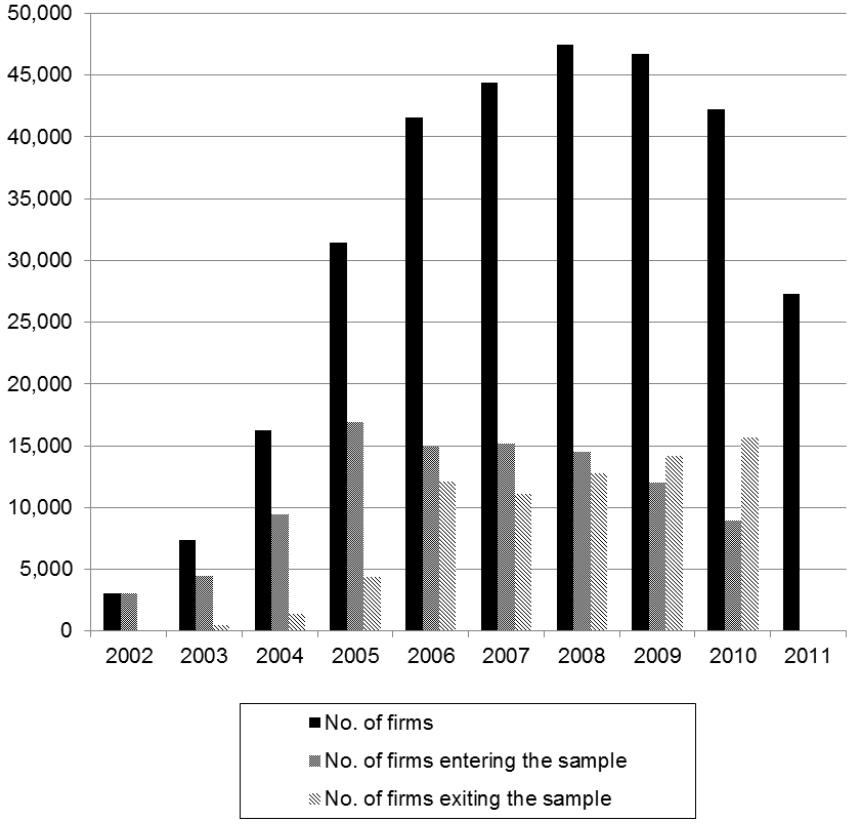

Figure 3. Size Distribution of Public and Private Sample Firms.

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 size-and-industry matched sample. The graphs present, for each set of firms, Epanechnikov kernel densities of the natural logarithm of total assets in $ millions of 2005 purchasing power. The width of the kernel density window around each point is set to 0.4. The unit of observation in each graph is a firm-year.

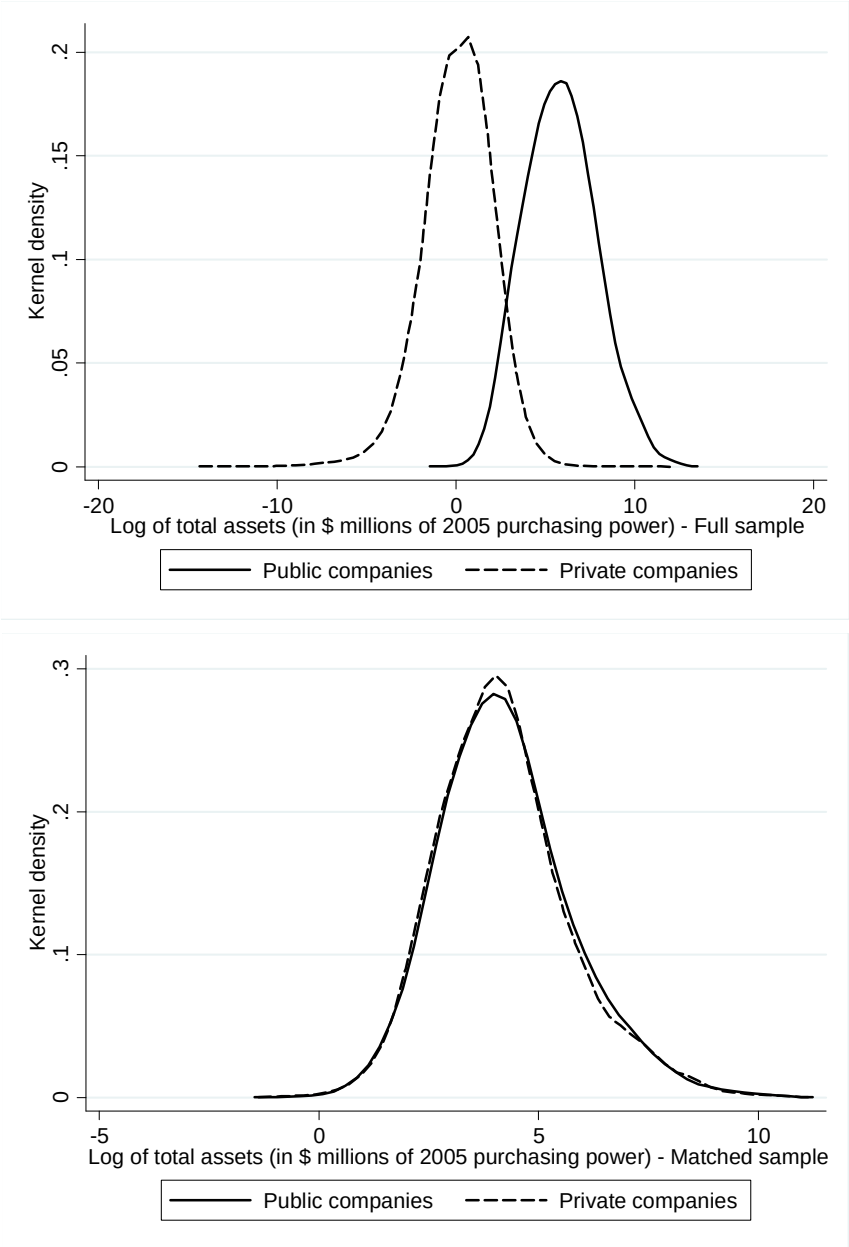

Figure 4. Public-Firm Investment Sensitivities by Size Decile, 2002-2011.

The figure shows estimates of the coefficient on investment opportunities for each size decile from an investment regression that interacts the firm's size decile with sales growth, using data for the Compustat universe over the 2002-2011 period. The investment regression is otherwise specified exactly as in Table 3, column 3. The coefficients estimated for size deciles 10 through 2 are significantly different from zero but not from each other. The coefficient estimated for size decile 1 is significantly different from zero and from the coefficients estimated for the other nine deciles.

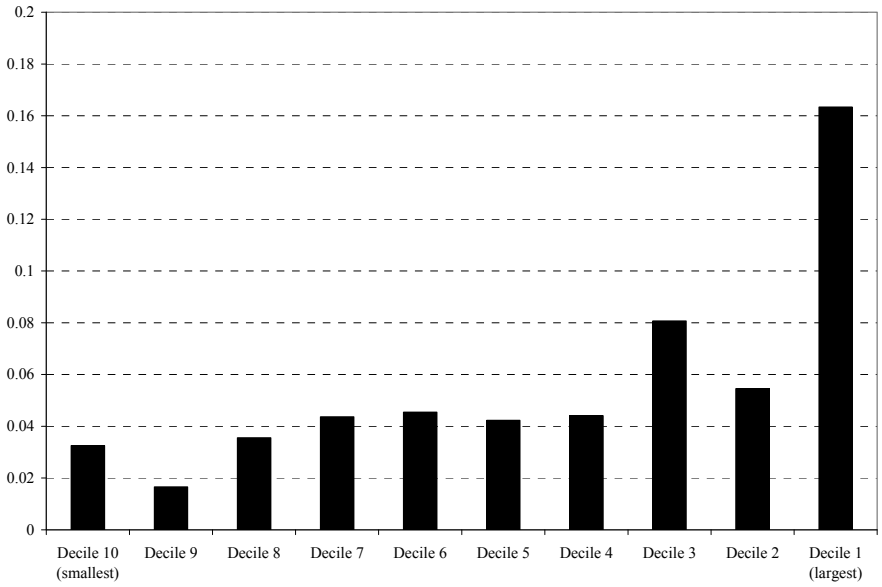

Figure 5. Public-Firm Investment Sensitivities for Consecutive Six-Year Windows, 1970-2011.

The figure shows coefficient estimates and 95% confidence intervals for the coefficient on sales growth in standard investment regressions estimated over consecutive ten-year windows starting in 1970 using Compustat data for publicly traded firms. The Compustat data are filtered as in our 2002-2011 Compustat sample of 29,718 firm-years; that is, a firm has to be recorded in both Compustat and CRSP during the relevant window; be incorporated in the U.S. and listed on a major U.S. exchange (NYSE, AMEX, or Nasdaq); have valid stock prices in CRSP; and have a CRSP share code of 10 or 11 (which screens out non-operating entities such as real estate investment trusts, mutual funds, or closed-end funds). We further exclude financial firms (the NAICS equivalent to SIC 6) and regulated utilities (SIC 49) as well as firms with fewer than two years of complete data. The investment regression is specified exactly as in Table 3, column 3.

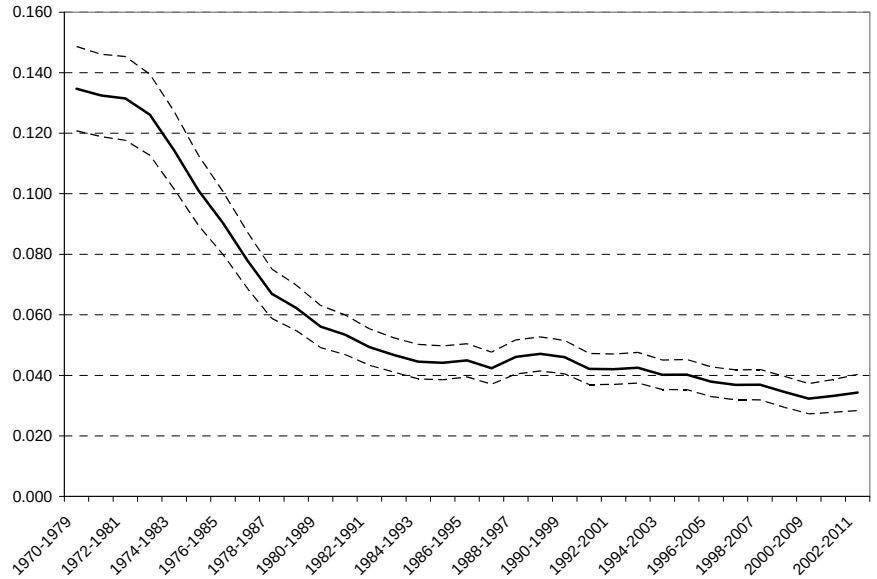

Table 1. Descriptive Statistics.

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 2011. 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 age, industry *Q*, and predicted *Q*) are winsorized 0.5% in each tail to reduce the impact of outliers. The unit of observation is a firm-year.

		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	2,869.4	13.5	2,855.9***	364.1	337.1	27.1
	median	329.2	1.2	327.9***	73.8	63.8	10.0***
	st.dev.	13,252.4	562.4		1,891.8	1,855.6	
Investment opportunities							
Sales growth	mean	0.165	0.147	0.018***	0.226	0.177	0.049***
	median	0.076	0.045	0.031***	0.078	0.091	-0.014***
	st.dev.	0.692	0.721		0.919	0.595	
Industry *Q*	mean	1.582	0.872	0.710***	1.636	1.636	0.000
	median	1.415	0.778	0.637***	1.506	1.506	0.000
	st.dev.	0.814	0.612		0.777	0.777	
Predicted *Q*	mean	1.744	1.475	0.269***	1.937	1.778	0.159***
	median	1.693	1.311	0.382***	1.918	1.815	0.103***
	st.dev.	0.640	1.379		0.737	0.650	
Firm characteristics							
ROA	mean	0.064	-0.118	-0.182***	-0.028	0.111	-0.140***
	median	0.111	0.061	0.050***	0.063	0.116	-0.053***
	st.dev.	0.319	1.736		0.458	0.505	
Cash holdings	mean	0.223	0.151	0.071***	0.294	0.133	0.161***
	median	0.136	0.072	0.064***	0.217	0.065	0.151***
	st.dev.	0.231	0.205		0.265	0.165	
Book leverage	mean	0.204	0.446	-0.242***	0.163	0.259	-0.096***
	median	0.146	0.278	-0.132***	0.063	0.170	-0.107***
	st.dev.	0.250	0.666		0.267	0.306	
RE/TA	mean	-0.611	0.072	-0.683***	-1.363	0.126	-1.489***
	median	0.087	0.096	0.009***	-0.180	0.106	-0.286***
	st.dev.	2.057	0.967		2.704	0.827	
Age	mean	40.7			29.9		
	median	26.0			22.0		
	st.dev.	36.3			25.5		
Multinationals	fraction	0.542			0.396		
No. of observations		29,718	307,803		11,372	11,372	
No. of firms		4,360	99,040		2,595	1,476	

Table 2. Investment Levels.

This table compares the investment levels of public and private firms in the full samples of Compustat and Sageworks firms, our size-and-industry matched samples, and various variations of our basic matching specification. For details on the matching approach see Section 2.2. The table reports means, medians, and standard deviations of investment levels of public and private firms under the different matching specifications, 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. Investment levels are winsorized 0.5% in each tail to reduce the impact of outliers. The last four rows investigate potential confounds, beginning with the possibility that public and private firms are at different points in their lifecycle. The variable *RE/TA* equals retained earnings over total assets and is a common measure of a firm's lifecycle stage (see DeAngelo, DeAngelo, and Stulz (2006)). Another way to control for lifecycle differences is to match on age (years since founding). This variable is not available in Compustat or Sageworks. For public firms, we hand-collect founding dates from business directories and websites. For private firms, we match Sageworks firms to NETS, a database containing founding dates for approximately 18.8 million firms in the U.S.; see Appendix A for details. The last two rows restrict the sample to incorporated firms (i.e., C and S Corps) and to firms using accrual-basis rather than cash accounting, respectively.

			Public firms					Private firms					Public - private firms	
Row	Sample	Investment measure	Mean	Std. dev.	Median	No. of obs.	No. of firms	Mean	Std. dev.	Median	No. of obs.	No. of firms	Diff. in means	Diff. in medians
1	Full sample	Gross	**0.041**	0.154	0.020	29,718	4,360	**0.075**	0.303	0.014	307,803	99,040	-0.034***	0.006***
	Samples matched on:													
2	NAICS4, size	Gross	**0.037**	0.183	0.015	11,372	2,595	**0.068**	0.260	0.016	11,372	1,476	-0.031***	-0.001
		Net	**0.019**	0.139	0.000	11,372	2,595	**0.048**	0.214	0.004	11,372	1,476	-0.029***	-0.004***
3	NAICS5, size	Gross	**0.038**	0.190	0.015	9,884	2,331	**0.070**	0.265	0.016	9,884	1,301	-0.032***	-0.001*
4	NAICS6, size	Gross	**0.046**	0.211	0.016	6,150	1,662	**0.070**	0.256	0.018	6,150	986	-0.024***	-0.002***
5	NAICS4, size, sales growth	Gross	**0.038**	0.168	0.017	15,916	3,345	**0.123**	0.401	0.022	15,916	2,387	-0.085***	-0.005***
6	NAICS4, size, ROA, sales growth, cash, and debt	Gross	**0.044**	0.169	0.019	16,217	3,491	**0.068**	0.209	0.020	16,217	3,057	-0.023***	-0.001***
7	NAICS4, size, *RE/TA*	Gross	**0.041**	0.155	0.018	25,867	4,089	**0.082**	0.315	0.020	25,867	2,899	-0.041***	0.002***
8	NAICS4, size, age	Gross	**0.047**	0.167	0.021	17,635	3,388	**0.074**	0.202	0.020	17,635	1,086	-0.027***	0.001***
9	NAICS4, size, restricted to C and S Corps	Gross	**0.036**	0.182	0.015	10,804	2,518	**0.067**	0.240	0.016	10,804	1,328	-0.031***	-0.001*
10	NAICS4, size, restricted to accrual basis accounting	Gross	**0.037**	0.182	0.015	11,369	2,595	**0.069**	0.262	0.016	11,369	1,477	-0.032***	-0.001

Table 3. 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 8 in Table 5. We use three different measures of investment opportunities: Sales growth, our preferred measure (columns 1-3 and 6-9); industry *Q* (column 4); and predicted *Q* (column 5). For variable definitions and details of their construction, see Appendix A. Following parts of the empirical investment literature, all specifications include ROA, sometimes interpreted as a possible proxy for financing constraints. Our baseline specification in column 1 uses a size-and-industry matched sample based on four-digit NAICS industries. (See Section 2.2 for further details of the matching.) Column 2 reports the investment model for matched public firms only, while column 3 uses all public firms over our sample period. Columns 6 and 7 investigate robustness to matching on finer industry classifications. In columns 8 and 9, we add further matching criteria to the baseline specification. Column 8 matches on sales growth in addition to total assets and NAICS4 industry while column 9 matches on industry, total assets, sales growth, ROA, cash holdings, and book leverage. Both of these specifications use a nearest-neighbor propensity score match with a 5% caliper. 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. Each regression also includes 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. 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								
Proxy for inv. opps.	Sales growth			Industry *Q*	Predicted *Q*	Sales growth			
Sample	Matched on size and industry (NAICS4) (1)	Matched public firms only (2)	All public firms (3)	Matched on size and industry (NAICS4) (4)	Matched on size and industry (NAICS4) (5)	Matched on size and industry (NAICS5) (6)	Matched on size and industry (NAICS6) (7)	Matched on size, sales growth & industry (NAICS4) (8)	Matched on size, sales growth, ROA, cash, debt & industry (NAICS4) (9)
Investment opportunities	0.118***	0.028***	0.037***	0.066**	0.272***	0.136***	0.115***	0.087***	0.134***
	0.029	*0.006*	*0.005*	*0.028*	*0.068*	*0.033*	*0.032*	*0.016*	*0.027*
Investment opp. x public	-0.091***			-0.080**	-0.148**	-0.109***	-0.078**	-0.055***	-0.078***
	0.030			*0.035*	*0.064*	*0.033*	*0.034*	*0.017*	*0.029*
ROA	0.089***	0.039**	0.042*	0.060	0.282***	0.069***	0.055***	0.032**	-0.015
	0.032	*0.018*	*0.023*	*0.052*	*0.072*	*0.024*	*0.014*	*0.014*	*0.021*
ROA x public	-0.050			-0.013	-0.151**	-0.036	-0.034	-0.018	0.032
	0.037			*0.055*	*0.072*	*0.029*	*0.022*	*0.026*	*0.039*
R^2 (within)	8.8 %	4.5 %	5.0 %	3.7 %	8.0 %	9.7 %	9.9 %	15.3 %	8.3 %
F-test: all coeff. = 0	9.2***	11.6***	29.6***	7.6***	9.0***	8.2***	7.6***	8.7***	13.5***
No. observations	22,744	11,372	29,718	22,744	22,744	19,768	12,300	31,832	32,434
No. firms	4,071	2,595	4,360	4,071	4,071	3,632	2,648	5,732	6,548

Table 4. Investment Dynamics.

This table investigates whether investment dynamics could bias the observed investment sensitivities in Table 3. For ease of comparison, column 1 shows the baseline results from Table 3, column 1. Columns 2 and 3 add a further lag of our proxy for investment opportunities (i.e., sales growth) to allow firms to respond to changes in investment opportunities with a delay. Column 2 (like column 1) is estimated using OLS with firm and year fixed effects. Column 3 is estimated using the Blundell and Bond (1998) system GMM estimator, which jointly estimates a first-differenced equation (instrumented with lags dated *t*-1 and earlier of ROA and lags dated *t*-2 and earlier of sales growth) and an equation in levels (instrumented with lagged differences). Column 4 adds the lagged dependent variable to the column 3 specification to allow for richer investment dynamics. In columns 1 and 2, we test for serial correlation in the idiosyncratic errors of our linear panel-data model using Wooldridge's (2002) test. In columns 3 and 4, we report *p*-values for the Arellano-Bond test for second-order auto-correlation in the differenced residuals and a Hansen over-identification test. For variable definitions and details of their construction, see Appendix A. 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			
	Baseline (within-groups) (1)	Baseline w/ lagged inv. opps. (2)	Static system GMM (3)	Dynamic system GMM (4)
Investment opportunities	0.118***	0.105***	0.150***	0.121***
	0.029	*0.033*	*0.050*	*0.026*
Investment opp. x public	-0.091***	-0.073**	-0.112**	-0.083***
	0.030	*0.034*	*0.055*	*0.029*
Lagged investment opportunities		0.005	0.036*	0.030***
		0.010	*0.020*	*0.009*
Lagged investment opp. x public		0.005	-0.031	-0.029**
		0.010	*0.024*	*0.011*
ROA	0.089***	0.082*	0.255*	0.352***
	0.032	*0.044*	*0.153*	*0.087*
ROA x public	-0.050	-0.047	-0.266	-0.330***
	0.037	*0.048*	*0.167*	*0.088*
Lagged dependent variable				0.064**
				0.026
R^2 (within)	8.8 %	6.7 %	n.a.	n.a.
F-test: all coeff. = 0	9.2***	16.0***	17.5***	18.9***
Wooldridge test for autocorrelation in panel data (*p*)	0.087	0.609	n.a.	n.a.
Arellano-Bond test for AR(2) (*p*)	n.a.	n.a.	0.954	0.406
Hansen over-identification test (*p*)	n.a.	n.a.	0.215	0.195
No. observations	22,744	17,768	17,768	17,768
No. firms	4,071	3,973	3,973	3,973

Table 5. Potential Confounds.

As in Table 3, we use sales growth to proxy for investment opportunities and exploit within-firm variation using OLS with firm and year fixed effects. Columns 1 and 2 investigate lifecycle effects. In column 1, we control for lifecycle-stage by propensity-score matching public and private firms on size and the ratio of retained earnings to total assets (*RE/TA*) within a NAICS4 industry. *RE/TA* is a common measure of a firm's lifecycle stage (see DeAngelo, DeAngelo, and Stulz (2006)). Column 2 controls for lifecycle differences by matching on firm age (years since founding). This variable is not available in either Compustat or Sageworks. For public firms, we hand-collect founding dates from regulatory filings, business directories, and a comprehensive search of online and offline sources. For private firms, we match Sageworks firms to NETS, a database containing founding dates for approximately 18.8 million firms in the U.S.; see Appendix A for details. Columns 3-5 allow for a broader range of assets that firms can invest in. Column 3 includes R&D spending in the dependent variable for public firms (R&D is unavailable for private firms). Column 4 adds investment in advertising to gross investment in fixed assets; this is available for both public and private firms. In column 5, the dependent variable is (gross investment + advertising) for private firms and (gross investment + advertising + R&D + change in goodwill) for public firms (goodwill is not available for private firms). Column 6 restricts the sample to C and S Corps, while column 7 restricts the sample to firms using accrual-basis rather than cash accounting. In column 8, we change the dependent variable from gross to net investment (i.e., the change in net fixed assets over beginning-of-year total assets). Column 9 drops public firms that report having overseas operations. In column 10, we test whether the results presented in Table 3, column 1 are robust to observable heterogeneity in cash holdings and book leverage. For variable definitions and details of their construction, see Appendix A. 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		Intangibles							
	Matched on size, industry and *RE/TA* (1)	Matched on size, industry, and age (2)	R&D (3)	Advertising (4)	R&D, advertising, change in goodwill (5)	Only C or S Corps (6)	Only accrual-basis accounting (7)	Net rather than gross investment (8)	Exclude multi-nationals (9)	Additional controls (10)
Investment opportunities	0.106***	0.064***	0.119***	0.131***	0.131***	0.137***	0.117***	0.086***	0.118***	0.118***
	0.019	*0.004*	*0.029*	*0.034*	*0.034*	*0.035*	*0.029*	*0.023*	*0.029*	*0.029*
… x public	-0.070***	-0.032***	-0.082***	-0.100***	-0.084**	-0.107***	-0.088***	-0.063***	-0.095***	-0.091***
	0.019	*0.005*	*0.030*	*0.035*	*0.035*	*0.036*	*0.030*	*0.023*	*0.029*	*0.029*
ROA	-0.014	0.020***	0.090***	0.094***	0.094***	0.091***	0.089***	-0.032	0.072***	0.088***
	0.012	*0.003*	*0.032*	*0.035*	*0.035*	*0.027*	*0.032*	*0.051*	*0.021*	*0.031*
… x public	0.048*	0.014**	-0.219***	-0.052	-0.253***	-0.053*	-0.050	0.055	-0.028	-0.053
	0.026	*0.007*	*0.040*	*0.040*	*0.048*	*0.032*	*0.037*	*0.053*	*0.027*	*0.037*
Cash holdings										0.107***
										0.030
Book leverage										-0.180**
										0.076
R^2 (within)	6.2 %	3.9 %	9.1 %	9.1 %	10.3 %	9.8 %	8.7 %	7.9 %	8.0 %	10.9 %
F-test: all coeff. = 0	16.5***	7.5***	10.7***	7.7***	9.6***	10.6***	9.1***	8.0***	8.0***	9.8***
No. observations	51,734	35,270	22,744	22,744	22,744	21,608	22,738	22,744	13,732	22,744
No. firms	6,988	4,474	4,071	4,071	4,071	3,846	4,072	4,071	2,901	4,071

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

We use staggered changes in state corporate income taxes (listed in Appendix B) as plausibly exogenous shocks to investment opportunities. Except in column 2, we measure tax changes continuously as $tax\ rate_t - tax\ rate_{t-1}$, where $tax\ rate_t$ is the top marginal corporate income tax rate (in %) in a firm's headquarter state during fiscal year t. (We hand-collect historic HQ states for Compustat firms as Compustat backfills firm locations.) Column 2 splits the tax change variable into tax increases and tax cuts to allow for asymmetry. A negative coefficient on the tax cut (tax increase) variable indicates that tax cuts (tax increases) are associated with increases (reductions) in investment. Multi-state firms in column 4 are those listing operations in multiple states in Item 2 of their 10-K filing for fiscal year t-1. The change in tax payments in column 5 is defined as tax_t / $total\ assets_t - tax_{t-1}$ / $total\ assets_{t-1}$. Each regression includes firm fixed effects and year effects (not reported) and is estimated using least-squares. 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					
	Matched sample (C Corps only) (No. observations = 19,177; no. firms = 3,471)					Private non-C Corps
	(1)	(2)	(3)	(4)	(5)	(6)
Tax change (in %)	-0.040***		-0.065**	-0.040***	-0.039***	-0.004
	0.009		*0.026*	*0.009*	*0.009*	*0.004*
x public	0.036***		0.064**		0.034***	
	0.012		*0.028*		*0.012*	
x public single-state firm				0.032*		
				0.019		
x public multi-state firm				0.037***		
				0.012		
Tax increase (in %)		-0.029**				
		0.011				
x public		0.020				
		0.014				
Tax cut (in %)		-0.046***				
		0.013				
x public		0.047**				
		0.019				
Tax change (in %, t-1)			-0.022			
			0.019			
x public			0.032			
			0.021			
Tax change (in %, t+1)			0.015			
			0.012			
x public			-0.014			
			0.014			
Change in tax payments t-1 to t					-0.163**	
					0.068	
x public					0.118	
					0.085	
Sales growth	0.077***	0.073***	0.071**	0.077***	0.078***	0.054***
	0.017	*0.015*	*0.032*	*0.017*	*0.017*	*0.003*
x public	-0.052***	-0.045***	-0.046	-0.052***	-0.052***	
	0.018	*0.017*	*0.033*	*0.018*	*0.018*	
ROA	0.042***	0.040***	0.168***	0.042***	0.042***	0.005**
	0.009	*0.008*	*0.049*	*0.009*	*0.009*	*0.002*
x public	-0.008	-0.004	-0.130**	-0.008	-0.008	
	0.019	*0.019*	*0.053*	*0.019*	*0.019*	
R^2 (within)	5.9 %	6.1 %	5.2 %	5.9 %	6.0 %	2.3 %
F-test: all coefficients = 0?	14.1***	13.7***	7.6***	13.2***	12.6***	92.0***
F-test: coeff.(tax change, public) = 0?	0.4	0.6	0.0	0.2	0.4	n.a.
F-test: tax cut = tax increase?	n.a.	0.7	n.a.	n.a.	n.a.	n.a.
F-test: single-state = multi-state?	n.a.	n.a.	n.a.	0.1	n.a.	n.a.

Table 7. Changes in Investment Sensitivities 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. Appendix C lists their names, dates, and circumstances. 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 firm fixed effects 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 %
F-test: all coefficients = 0	6.7***	7.3***	16.6***	14.8***
No. observations	963	963	4,501	4,501
No. firms	90	90	419	419

Table 8. 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), and partnerships, and limited liability partnerships (LLPs) in the U.S. are overwhelmingly owner-managed and have highly concentrated ownership. 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 matching algorithm described in Section 2.2. 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 firm fixed effects 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	0.056***	0.056***	0.080***	0.057***	0.054***
	0.004	*0.002*	*0.023*	*0.003*	*0.008*
x sole proprietorship	-0.015	-0.014	-0.034		
	0.023	*0.023*	*0.034*		
x LLC	-0.004				
	0.007				
x partnership	-0.006				
	0.008				
x LLP	0.002				
	0.032				
x S Corp	0.001				
	0.005				
x (sole prop.+LLC+partnership+LLP)				-0.006	-0.005
				0.005	*0.009*
ROA	0.002	0.002	-0.015	0.002	0.005
	0.002	*0.002*	*0.015*	*0.002*	*0.005*
x sole proprietorship		0.002	0.024		
		0.015	*0.022*		
x (sole prop.+LLC+partnership+LLP)				0.001	-0.002
				0.015	*0.007*
R^2 (within)	2.3 %	2.3 %	1.9 %	2.3 %	2.4 %
F-test: all coeff. = 0	95.7***	117.8***	2.2***	117.7***	31.1***
F-test: inv. opp. interaction coefficients = 0	0.4	n.a.	n.a.	n.a.	n.a.
No. observations	307,803	307,803	7,404	307,803	83,001
No. firms	99,040	99,040	3,239	99,040	36,049

Table 9. 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 3. For details of how we construct ERC, see Appendix A. As before, we use our size-and-industry 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 and is estimated using least-squares. Standard errors, clustered at the firm level, are shown in italics. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively. The number of firm-years in is 22,744 and the number of firms is 4,071. All continuous variables are winsorized 0.5% in each tail to reduce the impact of outliers.

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	0.113***	-0.021	0.030	-0.373**	0.043	-0.021	0.089***	-0.052**	9.5%	63.5***
		0.037	*0.051*	*0.127*	*0.167*	*0.057*	*0.063*	*0.021*	*0.022*		
2	Fama-French 48 industries	0.091***	-0.036	0.129	-0.283*	0.029	-0.034	0.087***	-0.050**	8.8%	23.5***
		0.029	*0.031*	*0.138*	*0.148*	*0.021*	*0.028*	*0.013*	*0.021*		

INTERNET APPENDIX

(NOT INTENDED FOR PUBLICATION)

Table IA.1. Alternative Matching Choices.

This table explores robustness to variations in matching criteria. As in Table 3, we use sales growth to proxy for investment opportunities and exploit within-firm variation using OLS with firm and year fixed effects. For ease of comparison, column 1 reproduces the within-groups results from column 1 in Table 3 as a baseline. The baseline matched sample is constructed from a nearest-neighbor match with replacement, requiring that the ratio of public and private firms' sizes be less than 2; private firms that drop out of Sageworks are replaced by splicing in a new match. Column 2 shows results without splicing in a new match when the old match dies. Column 3 matches without replacement. Column 4 shows results for a multiple-neighbor match with *N*=5. Columns 5, 6, and 7 tighten the constraint on the permissible ratio of firm sizes. Column 8 shows a propensity score match on size and industry using a .05 caliper. Column 9 estimates the investment model in the full samples of public and private firms (i.e., without any attempt at matching). For variable definitions and details of their construction, see Appendix A. 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.

	Matched on size and industry (NAICS4)								
	Relative size < 2								
	Baseline (1)	w/o splicing in new firm (2)	w/o replace-ment (3)	multiple-neighbor matches (*N*=5) (4)	Relative size < 1.5 (5)	Relative size < 1.33 (6)	Relative size < 1.2 (7)	Propensity score match w/ .05 caliper (8)	Full samples of public and private firms (9)
Investment opportunities	0.118***	0.110***	0.094***	0.090***	0.108***	0.087***	0.077***	0.132***	0.056***
	0.029	*0.030*	*0.025*	*0.018*	*0.028*	*0.023*	*0.021*	*0.049*	*0.002*
Investment opp. x public	-0.091***	-0.084***	-0.061**	-0.062***	-0.079***	-0.061**	-0.049**	-0.101**	-0.019***
	0.030	*0.031*	*0.026*	*0.019*	*0.028*	*0.024*	*0.022*	*0.049*	*0.005*
ROA	0.089***	0.199***	0.070**	0.050***	0.081***	0.072**	0.051***	0.059**	0.003
	0.032	*0.047*	*0.032*	*0.010*	*0.030*	*0.030*	*0.017*	*0.029*	*0.002*
ROA x public	-0.050	-0.147***	-0.042	-0.010	-0.046	-0.035	-0.018	-0.046	0.040*
	0.037	*0.055*	*0.038*	*0.020*	*0.035*	*0.036*	*0.026*	*0.036*	*0.023*
R^2 (within)	8.8 %	9.8 %	8.0 %	7.1 %	7.5 %	6.6 %	5.3 %	9.2 %	2.3 %
F-test: all coeff. = 0	9.2***	24.9***	8.6***	6.3***	8.8***	8.0***	7.3***	10.9***	134.6***
No. observations	22,744	14,228	10,138	45,110	18,882	16,542	13,806	31,374	337,521
No. firms	4,071	3,366	3,220	6,177	3,701	3,468	3,142	5,034	103,400