Corporate Investment and Stock Market Listing: A Puzzle? [*] [†]

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 document sizeable and surprising 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 substantially less and are less responsive to changes in investment opportunities compared to matched private firms, even during the recent financial crisis. These differences do not reflect observable economic differences between public and private firms (such as lifecycle differences) and instead appear to be driven by a propensity for public firms to suffer greater agency costs. Evidence showing that investment behavior diverges most strongly in industries in which stock prices are particularly sensitive to current earnings suggests public firms may suffer from 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 300,000 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, 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)). This is true even during the recent financial crisis.

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 and so change only their ownership structure: IPO firms are significantly more sensitive to investment opportunities in the five years before they go public than after. Indeed, once they have gone 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? One possibility is that the striking

[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.

difference in investment sensitivities is simply an artifact of our sampling, measurement, or methodological choices. However, extensive robustness tests show that our samples are representative, that our results are robust to various alternative matching approaches, and that the difference in investment behavior does not appear to be driven by how we measure investment opportunities.

This suggests that we need to look to more fundamental economic differences between public and private firms for an explanation. After ruling out differences in age or lifecycle; differences in the importance of investment in intangibles; differences in tax treatment or accounting choices; and differences in other observable characteristics such as size, cash holdings, or debt, we are left with differences in ownership and agency problems as the leading candidate explanation.

The corporate finance literature has long argued that stock market listed firms are prone to agency problems. While listing a firm on a stock market provides access to a deep pool of low cost capital, this can also 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 can 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

[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.

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 public-firm managers derive utility from both the firm's current stock price and its long-term value. [5] 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, managers see underinvestment as a way to create the impression that the firm's profitability is greater than it really is, hoping to thereby boost today's share price (Stein (1989)). This leads managers to use a higher hurdle rate when evaluating investment projects than would be used absent myopic distortions, resulting in lower investment levels and lower sensitivity to changes in investment opportunities.

The fact that we find *lower* investment levels among public firms seems inconsistent with empire building. On the other hand, both the quiet life argument and short-termism models predict underinvestment, thus fitting 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 coefficient' or ERC (Ball and Brown (1968)). For industries in which share prices are unresponsive to

[5] See Miller and Rock (1985), Narayanan (1985), Stein (1989), Shleifer and Vishny (1990), von Thadden (1995), and Holmström (1999).

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 two 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 public and private firms that are similar in size, 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, our analysis suggests that agency problems in public firms, and in particular short-termism, are a plausible driver of the differences in investment behavior that we document. 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 (2011) 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 the pay-performance link is much stronger in public than in private firms. 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 (2013), 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 take costly actions to avoid negative earnings surprises and interprets this as evidence of short-termist behavior. For example, Bhojraj et al. (2009) show that firms that barely beat analysts' earnings forecasts 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.[6] 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.[7] 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.

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. Sageworks differs from Compustat in two key respects: It provides data on private (as opposed to public) firms and it masks firm names (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,

[6] This figure does not include the self-employed. (Source: http://www.census.gov/econ/susb)
[7] In Brau and Fawcett's (2006) survey of large private firms, only 10.5% had considered going public.

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. 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. This gives a ten-year, unbalanced panel.[9] Figure 2 illustrates the growth of the Sageworks database 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. This allows us to exploit within-firm variation. The final sample contains 307,803 firm-years for 99,040 private firms 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; be incorporated in the U.S. and listed on a major U.S. exchange (NYSE, AMEX, or Nasdaq); have valid

[8] NETS contains data on estimated employment and sales, location, industry, and founding year for approximately 18.8 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 does not co-operate with Sageworks, its time series will end but all of its historical data remains in the database.

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. These filters leave us with 29,718 firm-years for 4,360 public firms in 2002-2011.

2.2 Matching

To control for observable differences between public and private firms, we follow prior literature and use a matching procedure. The aim is to identify firms that are observably similar on dimensions likely to affect investment in a way that imposes minimal functional-form assumptions on the data.

Prior work documents that corporate investment varies with firm size (Gala and Julio (2011)) and across industries (Jorgenson (1971)). These are two dimensions on which public and private firms differ significantly. Not surprisingly, public firms are much larger than private firms. Table 1 shows that the mean (median) public firm in our Compustat sample has total assets of $2,869.4 million ($392.2 million), compared to $13.5 million ($1.2 million) for the private firms in Sageworks. The top graph in Figure 3 plots the two size distributions; they overlap only to a limited extent. Similarly, Figure 4 illustrates that the distributions of public and private firms differ substantially across Fama-French 30-industry groups. Our preferred match hence is based on size and industry. Note that this implies that our matched sample consists of *small* public and *large* private firms with the same industry distribution. That said, we also consider various alternative sets of matching variables for robustness.

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 closest in size in the same four-digit NAICS industry, requiring that the ratio of their total assets (TA) is less than 2 (i.e., $\max(TA_{public}, TA_{private}) / \min(TA_{public}, TA_{private}) < 2$).[10] 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. This will allow us to estimate the within-firm sensitivity of investment to investment opportunities. We

[10] As we will show, our results are robust to using finer industry classifications, such as NAICS5 or NAICS6 and to imposing tighter calipers on the maximum difference in size.

match with replacement, though our results are not sensitive to this.[11] 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. As we match with replacement, the sample contains 2,595 public firms and 1,476 private firms.

As the bottom graph in Figure 3 shows, matching produces size distributions that look nearly identical. To test this formally, we report two standard statistical measures of match quality. The first is Hotelling's 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). (Industry distribution, by construction, cannot differ.) The second test of match quality is Rosenbaum and Rubin's (1985) *SDIFFF* test. While critical values have not yet been derived, Rosenbaum and Rubin suggest that a value of 20 warrants concern about the extent to which matched groups are balanced. In our sample, *SDIFF* = 1.44, suggesting that our matched sample is balanced.

2.3 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 comparing 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 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

[11] 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.

investment decisions.[12] (For detailed definitions of these and all other variables, see Appendix A.)

2.4 Measures of Investment Opportunities

The investment literature proxies for a firm's investment opportunities using either Tobin's *Q* or sales growth. *Q* is usually constructed as the ratio of the firm's market value to the book value of its assets, but since private firms are not traded on a stock exchange, their market value is not observed. We 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 Michaely and Roberts (2012).

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.

Finally, we explore an exogenous shock to investment opportunities (in the form of state-level variation in corporate income tax rates) which allows us to sidestep the need to directly measure investment opportunities altogether. Over our sample period, there are 27 tax cuts and 13 tax increases in a total of 20 states. For example, Kentucky cut its corporate income tax rate from 8.25% to 7% in January 2005.[13] 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.

[12] 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.

[13] For this test, we restrict our sample of private firms to C Corps as only C Corps are subject to state corporate income taxes in most states.

2.5 Other Firm Characteristics

Table 1 shows that even after we match on size and industry, private firms have higher ROA, less cash, more debt, and more retained earnings. These patterns are consistent with those documented in prior studies, reflecting differences between public and private that are a direct result of their different ownership structures and thus that we neither expect to, nor aim to, eliminate by matching. As we will show, the observed differences in these quantities do not drive our results.

3. Differences in Public and Private Firm Investment Behavior

In this section, we document that private firms invest substantially more than public firms and that private firms are much more responsive to changes in their investment opportunities. Our evidence suggests that these differences in investment behavior are not driven by our sampling or methodological choices and instead must reflect fundamental economic differences between public and private firms. The question is, what differences? We show that the observed differences in investment behavior are not explained by systematic differences in age or lifecycle; differences in the importance of investment in intangibles; differences in tax treatment or accounting choices; or differences in other observable characteristics such as cash holdings or debt. This leaves differences in ownership and agency problems as the leading candidate explanation. We investigate this possibility in detail in Section 4.

3.1 Differences in Investment Levels

Table 2 shows that private firms invest significantly more than public firms on average. The differences are substantial. Row 1 in Table 2 shows that in the full samples, private firms increase gross fixed assets by 7.5% of total assets a year on average, 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.[14] Figure 5 shows that this is true in eight of the 10 sample years. The two exceptions are 2009, when both public and private firms reduced investment drastically, leaving their investment rates statistically indistinguishable; and 2011, when both groups of firms increased their investment rates to around 4.4% of assets.

[14] 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 do not depend on using gross investment. Results are similar for net investment, which is a significant 2.9 percentage points higher for private firms (row 3). They are also robust to matching on finer industry codes: Using 5-digit NAICS has virtually no effect (row 4) while using 6-digit NAICS narrows the difference in investment from 3.1 percentage points in favor of private firms to a still large 2.4 percentage points (row 5). Including other characteristics besides size and industry among the matching criteria does not close the gap either: When we follow Michaely and Roberts (2012) and match on size, NAICS4 industry, sales growth, ROA, book leverage, and cash, we find that private firms invest 2.3 percentage points more than public ones (row 6).[15]

3.2 Differences in Investment Sensitivity

To investigate differences in public and private firms' sensitivity to changes in investment opportunities, we estimate standard investment regressions in the style of the *Q*-theory literature 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,[16] K is gross fixed assets, A is total assets, S is sales, Z is operating income before depreciation,[17] and *PUBLIC* is a dummy for whether the firm is publicly listed. We remove unobserved time-invariant heterogeneity through firm fixed effects and include year effects to control for common trends.[18] Standard errors are clustered at the firm level in the usual manner.

3.2.1 Baseline Results

The results in column 1 of Table 3 suggest that public firms' investment decisions are significantly

[15] To match on other variables besides industry and size, we construct a propensity score based on size and the additional matching variables. We then adapt the matching algorithm described in Section 2.2 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.
[16] 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.
[17] 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.
[18] 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.

less sensitive to changes in investment opportunities. The coefficient estimate is 0.118 for private firms, which is 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.

Column 2 lets investment sensitivities vary between the boom years before the recent financial crisis (2002-2007) and the crisis and its aftermath (2008-2011). This reveals two patterns. First, both public and private firms reduced their investment sensitivities once the crisis began, perhaps because financial constraints became more binding in the wake of the "credit crunch." Remarkably, though, private firms' investment continues to be significantly more sensitive to changes in investment opportunities even during the financial crisis. The crisis-period coefficient for private firms is 0.053 (p=0.006), which is significantly greater than the 0.053 – 0.041 = 0.012 coefficient estimate for matched public firms.

These results point to a new stylized fact: the apparent existence of a wide gap in the investment sensitivities of public and private firms, even during the financial crisis. In the remainder of the paper, we investigate whether this gap is an artifact of our sampling, measurement, or methodological choices or whether it reflects more fundamental economic differences between public and private firms.

3.2.2 Sampling, Measurement, and Methodological Choices

To see whether public firms really invest differently than private firms, we first investigate what impact our sampling, measurement, and methodological choices have on the estimated difference in investment sensitivities.

First, it is possible that our investment model is simply better specified for private firms than for public ones. To investigate this, column 3 reports the results of estimating the investment model in the sample of matched public firms only. This reveals that our investment model 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 considerably lower than the 0.118 coefficient estimated for matched private firms in column 1.

Second, our matched sample may inadvertently oversample public firms with low investment sensitivities and so be unrepresentative of public firms in general. But when we re-estimate the investment model in the full sample of public firms, we find an estimated investment sensitivity that is

only marginally larger than in the matched sample of public firms, at 0.037 (*t*-statistic = 7.72; see column 4). Alternatively, we can let sensitivities vary with firm size. Figure 6 assigns the public firms in the full sample to one of 10 size deciles based on total assets and plots the estimated investment sensitivity for each decile.[19] In the bottom nine deciles, the coefficients measure around 0.04; they 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. Thus, our results appear representative of most public firms but do not extend to the very largest public firms (whose investment is more sensitive to investment opportunities than that of private firms in our sample).

Third, it is possible that public-firm investment was "unusually" insensitive during our time period (except among the very largest public firms) 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 10-year windows starting in 1970 using Compustat data for all eligible publicly traded firms.[20] Figure 7 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 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.

Fourth, our results might 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 5 and 6 show that our results continue to hold in either case. We also use a natural experiment to sidestep the need to directly measure investment opportunities altogether. The experiment exploits plausibly exogenous

[19] 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.
[20] The Compustat data are filtered exactly as in our 2002-2011 full 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.

variation in corporate tax rates at the state level (see Heider and Ljungqvist (2012) for further details). A cut in a state's corporate income tax rate reduces the user cost of capital for firms operating in that state, which should boost investment, and vice versa for tax increases. Tax changes can thus be viewed as shocks to firms' after-tax returns on investment and thus to their investment opportunity sets. Column 7 shows that private firms increase investment spending in response to tax cuts and reduce it in response to tax increases ($p<0.001$).[21] Public firms, on the other hand, barely respond to tax changes at all ($p=0.590$).

Fifth, our results could be driven by an overly noisy industry match. Columns 8 and 9 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.

Sixth, our matching criteria may be too parsimonious. Column 10 use the matching criteria of Michaely and Roberts (2012) – size, sales growth, ROA, book leverage, cash and industry – without changing our results. Matching also involves other choices. Table IA.1 in the Internet Appendix shows that our results are unchanged if we do not splice in a new match when a private firm exits the sample; match without replacement; use the five nearest neighbors instead of a single nearest neighbor; impose ever tighter calipers on the maximum difference in firm size; or match using a propensity score based on total assets (rather than matching 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.[22]

Finally, 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 Wooldridge test of autocorrelation in panel data for the baseline results from Table 3, column 1. The test indicates the

[21] In this test, we limit our sample of private firms to C Corps, given that in most states firms with other legal forms (sole proprietorships, LLCs, partnerships, and S Corps) are taxed at the personal rather than at the corporate income level.

[22] 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.

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, the difference in contemporaneous investment sensitivities remains large, at 7.3 percentage points, and statistically significant (p=0.034). Neither public nor private firms respond significantly to the additional lag. 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 even 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 GMM 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.

3.3 Economic Differences Between Public and Private Firms

The previous section suggests that the wide gap in investment sensitivities we observe in the data is unlikely to be an artifact of our sampling, measurement, or methodological choices. We next consider

whether it reflects more fundamental economic differences between public and private firms. We begin by investigating potential differences in firm maturity.

3.3.1 Lifecycle Differences

Differences in firm maturity or age 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 explain our results.

We examine this hypothesis using two approaches. 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, firms with low *RE/TA* ratios tend to be at the growth stages of their lifecycles while firms with high *RE/TA* ratios tend to be more mature.

Second, we use firm age as a direct proxy for maturity. This is not entirely straightforward, because 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 impute founding dates from the National Establishment Time Series (NETS) database, which offers comprehensive coverage of the entire U.S. economy. (See Appendix A for further details.) This yields imputed age information for 9,467 Sageworks firm-years. 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.

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, with private firms' investment being twice as sensitive to changes in investment opportunities as public firms' (p<0.001).

In either specification, lifecycle differences do not appear to fully explain the observed differences in investment sensitivities between public and private firms

3.3.2 Differences in the Importance of Intangibles

So far, we have focused on investment in fixed assets. Firms also invest in intangibles, such as R&D, advertising, and goodwill. Might systematic differences in the types of assets public and private firms invest in 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.

3.3.3 Differences in Legal Form or Accounting Choices

Private firms can be organized as sole proprietorships, limited liability companies (LLCs), or partnerships, or they can be incorporated under Subchapters C or S of the Internal Revenue Code. While virtually all public firms are incorporated, close to 16% of the private firms in the full Sageworks sample are not. To see if differences in legal form between public and private firms may be driving our results, column 6 requires that only incorporated private firms be matched to public 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).

3.3.4 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).

3.3.5 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 is statistically

significant, their inclusion does not alter the finding that public firms are significantly less responsive to changes in investment opportunities. In fact, the coefficient for the difference in investment sensitivity between public and private firms is indistinguishable from our baseline estimate of –0.091 (p=0.002).

4. Differences in Agency Problems

So far, we have come up empty-handed in our attempts to identify a key economic difference between public and private firms that could explain the observed difference in investment behavior. It is hence natural to wonder whether it is the listing itself (or perhaps some characteristic that correlates with it) which causes investment behavior to differ.

An obvious way in which a stock market listing affects firms is by lowering the cost of capital. But this should generate patterns opposite to those we find in our data: Financing frictions should make it *harder* for private firms to take advantage of investment opportunities. Evidence in a related paper focusing narrowly on natural gas producers in the U.S. confirms this. Using detailed hand-collected data, Gilje and Taillard (2013) show that privately held gas producers in the U.S. face greater financial constraints than stock-market listed gas producers. Importantly, in stark contrast to our findings, they find strong evidence that private gas producers invest significantly *less* and are significantly *less* sensitive to changes in investment opportunities than are public ones.

Our broad cross-industry evidence suggests that Gilje and Taillard's findings, though undoubtedly important, do not generalize to private firms in the aggregate.[23] While private firms may indeed face greater financing frictions than public ones, the patterns in our data suggest the presence of some other difference between public and private firms which, on average, outweighs private firms' cost-of-capital disadvantage and so allows them to invest more nimbly than public firms on average.[24] The natural place to look for such a difference is agency problems.

It has long been recognized that a stock market listing can increase the scope for 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

[23] However, we can replicate their results in our data when we restrict the sample to natural gas producers (NAICS 211111).
[24] We stress that our tests compare *large* private firms to *small* public firms. Financial constraints may well have a decisive impact on the investment behavior of the vast number of small private firms in the economy.

can now be traded may induce shareholders to sell rather than monitor managers.

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 Stein's (1989) and related short-termism models.[25] For these models 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 short-termism among public-firm managers being a plausible driver of the observed difference in investment sensitivities between public and private firms.

4.1 Do Private Firms Plausibly 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, LLCs, and partnerships. Sole proprietorships are by definition owner-managed. According to the Federal Reserve's SSBF survey, 67.3% of LLCs and around 90% of partnerships in the U.S. economy are owner-managed. Together, these three legal forms comprise 16% of the private firms in our full Sageworks sample. The remaining two legal forms can *theoretically* have dispersed ownership: C Corps can have an unlimited number of shareholders while S Corps can have up to 100. (Virtually all stock market listed firms are C Corps.)

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 6 tests this by allowing investment sensitivities among private firms to vary by legal form. Column 1 includes a set of interaction terms for each legal form, capturing differences in investment sensitivities relative to C Corps, in the full private-firm sample. The interaction terms are statistically insignificant, individually and jointly. Thus, investment sensitivities among private sample firms are no

[25] Some alternative models of short-termism (e.g., Bebchuk and Stole (1996)) predict over-investment instead of under-investment and are thus inconsistent with our empirical evidence.

different for C and S Corps, which *potentially* have dispersed ownership structures, and for 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 in our sample – including the C and S Corps – suffer from fewer investment-distorting agency problems than do the public sample firms.

4.2 Short-termism: Theory

The results in Table 6 open the door to agency considerations, and in particular either 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.[26] 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. Importantly, the mechanism in short-termism models is not fraudulent accounting 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,

[26] 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)).

a short-termist manager foregoes positive NPV projects, which results in lower investment levels and lower sensitivity to changes in investment opportunities.

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 each of them. As a result, they do not know the extent of underinvestment but they understand the manager's incentives. Investors thus realize that high current reported cash flows will in fact lead to lower cash flows in the future than in a world without agency problems. In response, they 'discount' the manager's report of current earnings. And yet Stein shows that, 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.

4.3 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 (i.e., whose $\alpha_0 = 0$). In this case, the manager cannot manipulate investors' cash flow expectations, so there is 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, leading to greater distortions. In sum, 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 theories, these predictions are unique to short-termism models.

4.4 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 (see 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-sample investment regression.

Table 7 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 the implied point estimate for public firms is 0.091 (=0.113–0.021). This is the first time in our various tests that we see public firms investing with a high sensitivity. The difference in investment sensitivities between public and private firms is small (0.021) and statistically insignificant (p=0.676), 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. This supports our second prediction.

Row 2 shows that the results are similar when we use Fama-French 48 industries.

4.5 Interpretation

The findings in Table 7 are consistent with the interpretation that short-termism could play a key role in explaining the observed differences in investment behavior between public and private firms in our sample.[27] 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 theoretically explain the reported differences in investment behavior between public and private firms, such as a preference for a quiet

[27] Following our approach, Gilje and Taillard (2013) show that natural gas producers have a below-median ERC, which in turn could explain why the public firms in their sample appear not to invest myopically.

life.[28]

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 other differences 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.

4.6 Within-Firm Variation in Listing Status

As a final reality check, we follow Michaely and Roberts (2012) in considering an alternative sample: firms that go public. If the observed difference in investment sensitivities is indeed the result of a change in managers' investment horizons, we should see that investment behavior changes as a firm transitions from private (i.e., concentrated) to public (i.e., dispersed) ownership. This set-up differs from our tests so far in that it examines how a *given* firm's investment behavior changes as it transitions from private to public status.[29]

Of course, most firms go public precisely in order to fund a planned increase in investment (Brau and Fawcett (2006)). As this could mask the expected change in investment behavior, 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 sample thus gets closer to the ideal experiment, in which only managerial incentives change as a firm goes public, allowing us to isolate the effect of incentives on investment.[30]

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

[28] 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 7 findings.

[29] 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.

[30] That said, we emphasize that an IPO is not a natural experiment and thus stop short of claiming causality.

to fund operations or investment plans or to repay debt.[31] For each firm, we collect five years of post-IPO accounting data from Compustat and hand-collect up to five years of 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 R&D as well as on capital expenditures (CAPEX) from the income and cash flow statements.

Columns 1 and 2 of Table 8 test if investment sensitivities do indeed change around a firm's IPO by interacting investment opportunities with an indicator set equal to one after the IPO. Whether we measure investment as CAPEX (column 1) or the sum of CAPEX and R&D (column 2), we find that it is highly 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 are of the kind that only affect their ownership structure. This finding is consistent with the patterns 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 *already*-public firms matched on size, age, and industry. While the two groups of firms may of course differ in various 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 than before but in fact become indistinguishable from those of matched already-public 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.

Remarkably, these findings continue to hold during the recent financial crisis, when private firms presumably became (even) more financially constrained compared to public firms. Our results are robust

[31] Suitable IPOs are identified from Thomson Reuters' SDC database. Appendix B lists their names, dates, and circumstances.

to matching on other variables in addition to size and industry, as well as to using a variety of matching algorithms, and 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 or lifecycle 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 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 increases the hurdle rate used to evaluate investment projects and leads to lower investment levels and lower sensitivity to changes in investment opportunities.

We are careful not to claim causality. Short of a trial that randomly assigns firms to public or private status, we cannot rule out that the patterns we observe reflect some set of latent factors. For example, a private firm whose manager is more prone to succumb to short-termist pressures might also be more likely to go public. However, in the absence of a theoretically founded model along these lines, empirical researchers are poorly placed to test for such effects. To that end, we cautiously report correlations consistent with some of the most influential strands of agency models in the corporate finance literature. We note 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 could play 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.

Tax change equals $tax\ rate_t - tax\ rate_{t\text{-}1}$, where $tax\ rate_t$ is the top corporate income tax rate (in %) in a firm's headquarter state during fiscal year *t*. Tax changes are taken from Appendix A and B in Heider and Ljungqvist (2012). We hand-collect historic HQ states for Compustat firms as Compustat backfills firm locations.

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 IPO firms

The sample used in Table 8 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 18.8 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.

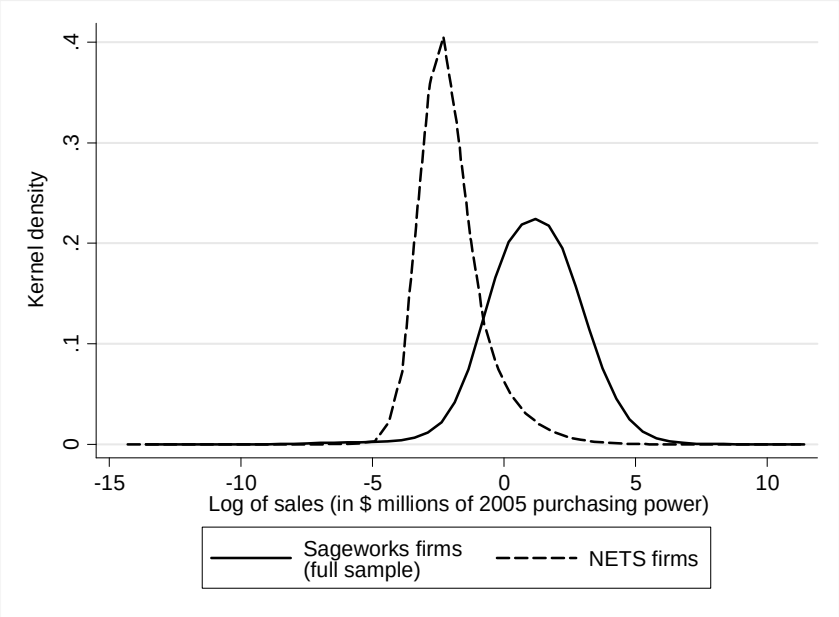


Figure 2. The Sageworks Dataset: Distribution by Year.

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.

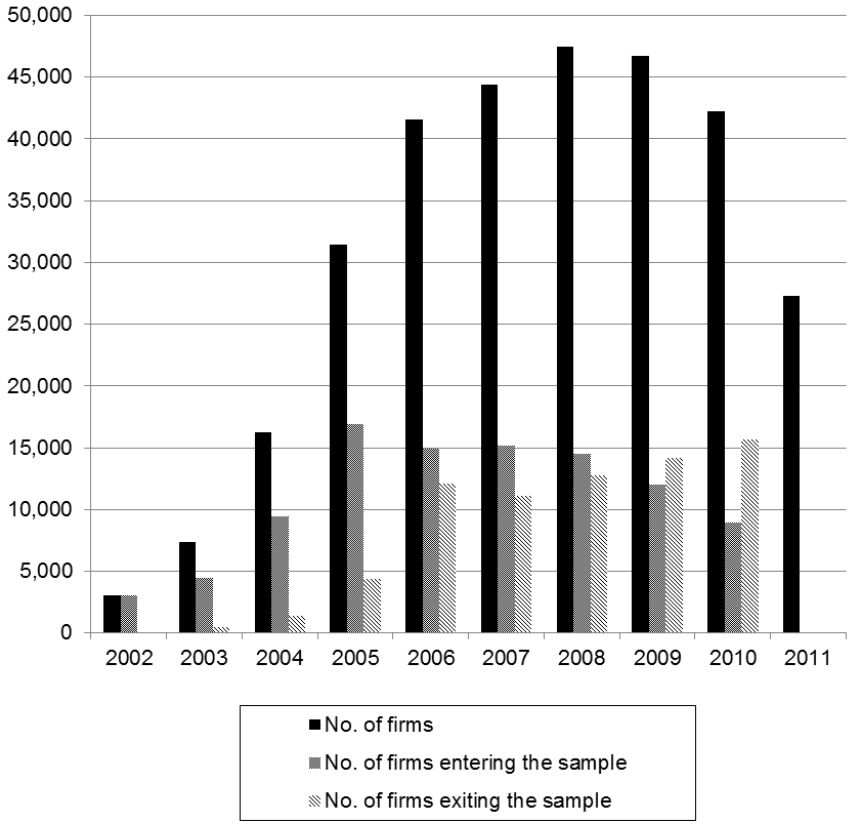


Figure 3. Size Distribution of Public and Private Sample Firms.

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.

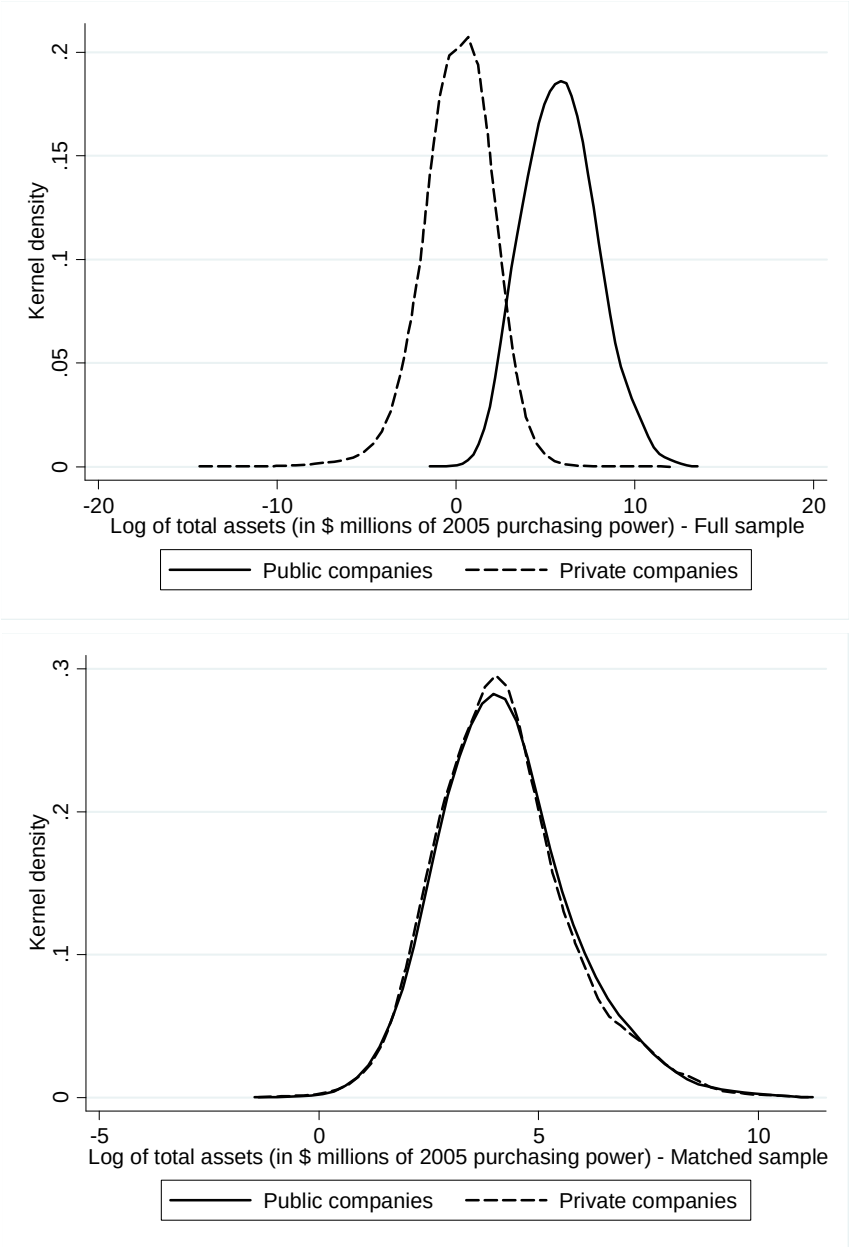
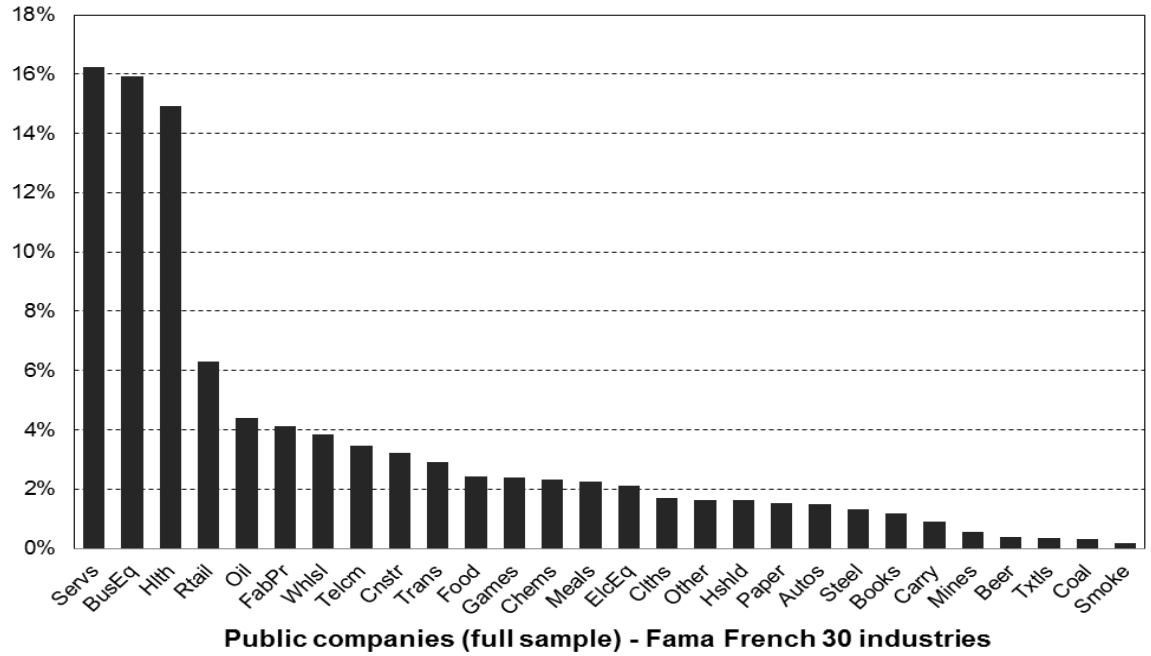


Figure 4. Industry Distribution of Public and Private Sample Firms.

The top and bottom graphs show the industry distribution of the public and private firms in our full samples of Compustat and Sageworks firms, respectively. We use Fama-French (1997) 30 industry definitions, excluding utilities and financial firms.



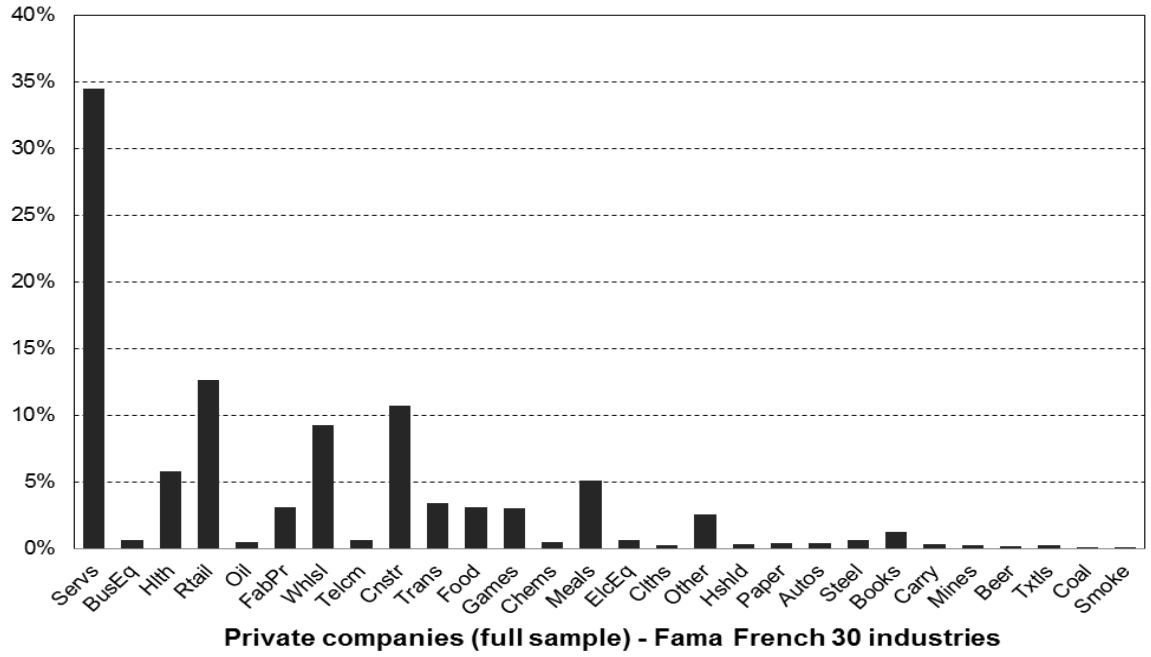


Figure 5. Public and Private Firm Investment Levels, 2002-2011.

The figure shows the average annual change in gross fixed assets (scaled by total assets) for public and private firms in our size-and-industry matched sample. All pairwise differences are statistically significantly different from zero except for the years 2009 and 2011.

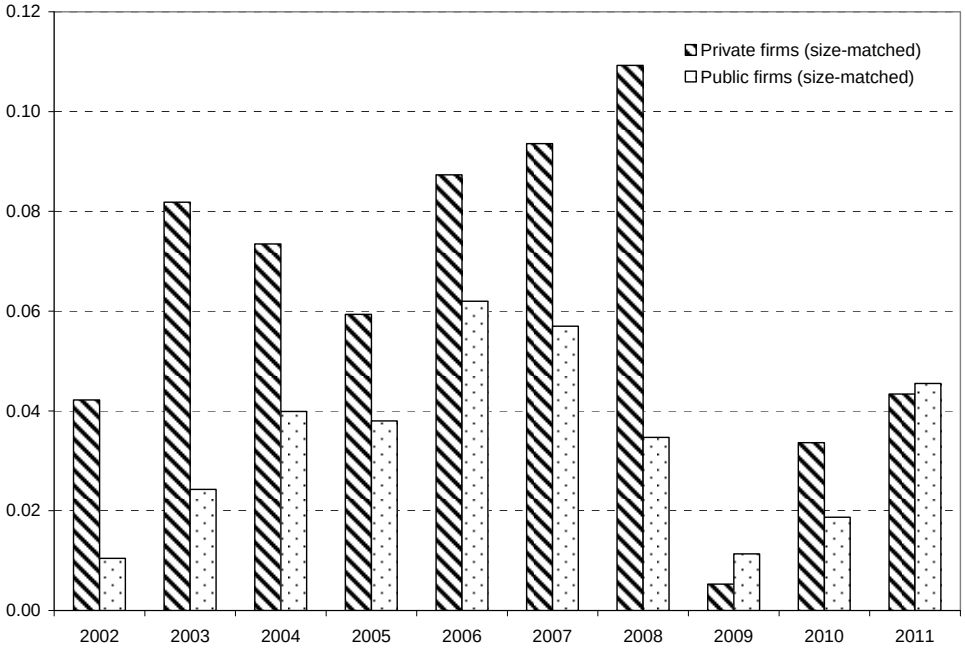


Figure 6. Public-Firm Investment Sensitivities by Size Decile, 2002-2011.

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 4. 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.

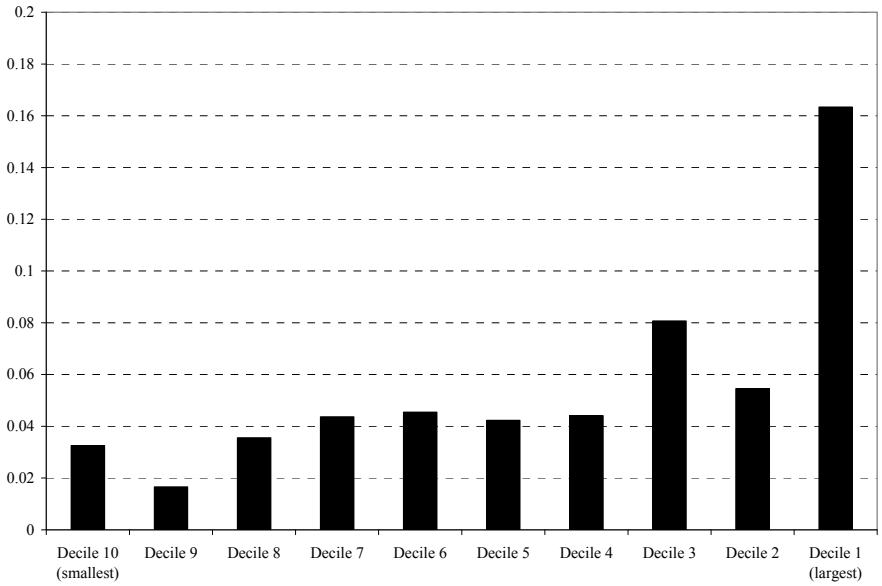


Figure 7. Public-Firm Investment Sensitivities for Consecutive Ten-Year Windows, 1970-2011.

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 4.

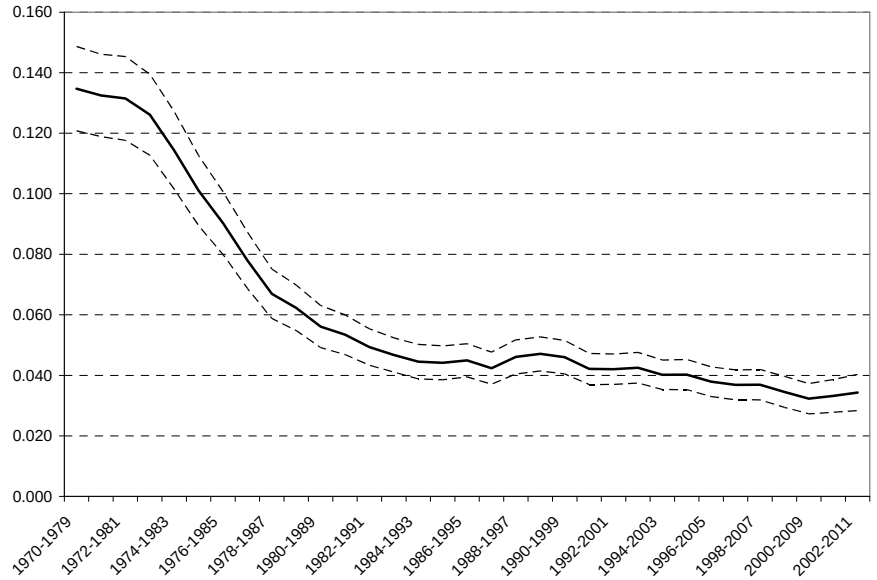


Table 1. Descriptive Statistics.

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 *** indicating a difference that is significant in a *t*-test (for means) or a Pearson χ^2 test (for medians) at the 1% level. 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%, 5%, and 10% 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.

			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
3	NAICS4, size	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***
4	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*
5	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***
6	NAICS4, size, ROA, sales growth, book leverage, and cash	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***

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 four different measures of investment opportunities: Sales growth, our preferred measure (columns 1-4 and 8-10); industry *Q* (column 5); predicted *Q* (column 6); and the change in the top corporate income tax rate in the state a firm is headquartered in (column 7). For variable definitions and details of their construction, see Appendix A. For details of the matching algorithm, see Section 2.2. All regressions include firm fixed effects. Since the sample contains no firms that transition from public to private status or vice versa, the *level* difference in investment between public and private firms is fully absorbed by the firm fixed effects. Each regression also includes year effects (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				Ind. *Q*	Pred. *Q*	Taxes	Sales growth		
Sample	Matched on size and NAICS4 (1)	Matched on size and NAICS4 (2)	Matched public firms only (3)	All public firms (4)	Matched on size and NAICS4 (5)	Matched on size and NAICS4 (6)	Matched on size and NAICS4 (C Corps only) (7)	Matched on size and NAICS5 (8)	Matched on size and NAICS6 (9)	Matched on size, sales growth, ROA, cash, debt & NAICS4 (10)
Investment opportunities	0.118***		0.028***	0.037***	0.066**	0.272***	-0.042***	0.136***	0.115***	0.134***
	0.029		*0.006*	*0.005*	*0.028*	*0.068*	*0.010*	*0.033*	*0.032*	*0.027*
x public	-0.091***				-0.080**	-0.148**	0.038**	-0.109***	-0.078**	-0.078***
	0.030				*0.035*	*0.064*	*0.012*	*0.033*	*0.034*	*0.029*
Inv. opps. (2002-2007)		0.180***								
		0.040								
x public		-0.143***								
		0.040								
Inv. opps. (2008-2011)		0.053***								
		0.019								
x public		-0.041**								
		0.021								
ROA	0.089***	0.117***	0.039**	0.042*	0.060	0.282***	0.010***	0.069***	0.055***	-0.015
	0.032	*0.024*	*0.018*	*0.023*	*0.052*	*0.072*	*0.003*	*0.024*	*0.014*	*0.021*
ROA x public	-0.050	-0.075**			-0.013	-0.151**	0.031*	-0.036	-0.034	0.032
	0.037	*0.031*			*0.055*	*0.072*	*0.019*	*0.029*	*0.022*	*0.039*
R^2 (within)	8.8 %	10.4 %	4.5 %	5.0 %	3.7 %	8.0 %	2.0 %	9.7 %	9.9 %	8.3 %
F-test: all coeff. = 0	9.2***	9.0***	11.6***	29.6***	7.6***	9.0***	14.1***	8.2***	7.6***	13.5***
No. observations	22,744	22,744	11,372	29,718	22,744	22,744	19,177	19,768	12,300	32,434
No. firms	4,071	4,071	2,595	4,360	4,071	4,071	3,471	3,632	2,648	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. Economic Differences Between Public and Private Firms.

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 incorporated firms (i.e., 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. 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 (including limited liability partnerships (LLPs)) 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 and 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 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. 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 and partnerships 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 7. 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*		

Table 8. 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 B 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 propensity-score matched in its first sample year to up to five public firms in the same industry (three-digit SIC) with the closest age and size (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***	0.016**	0.030***
	0.025	*0.031*	*0.006*	*0.009*
Investment opp. x pre-IPO			0.064**	0.088**
			0.028	*0.035*
Investment opp. x post-IPO	-0.058*	-0.080*	0.000	0.003
	0.032	*0.041*	*0.020*	*0.027*
ROA	0.053	0.095	0.141***	0.158***
	0.063	*0.074*	*0.015*	*0.021*
ROA x pre-IPO			-0.094	-0.070
			0.066	*0.078*
ROA x post-IPO	0.059	0.057	-0.021	-0.001
	0.053	*0.062*	*0.036*	*0.043*
Post-IPO	0.001	-0.004	-0.004	-0.005
	0.010	*0.012*	*0.009*	*0.012*
R^2 (within)	19.4 %	21.1 %	14.8 %	15.9 %
F-test: all coefficients = 0	6.7***	7.3***	18.8***	16.3***
No. observations	963	963	4,525	4,525
No. firms	90	90	420	420

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