Corporate Investment and Stock Market Listing: A Puzzle?

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Corporate Investment and Stock Market Listing: A Puzzle?

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We investigate whether short-termism distorts the investment decisions of stock market-listed firms. To do so, we compare the investment behavior of observably similar public and private firms, using a new data source on private U.S. firms and assuming for identification that closely held private firms are subject to fewer short-termist pressures. Our results show that compared with private firms, public firms invest substantially less and are less responsive to changes in investment opportunities, especially in industries in which stock prices are most sensitive to earnings news. These findings are consistent with the notion that short-termist pressures distort investment decisions. (*JEL* D22, D92, G31, G32, G34)

Economists have long worried that a stock market listing can induce short-termist pressures that distort the investment decisions of public firms. Narayanan (1985), for example, expresses the concern that "American managers tend to make decisions that yield short-term gains at the expense of the long-term interests of the shareholders." More recently, a growing number of commentators blame the sluggish performance of the U.S. economy since the 2008–2009 financial crisis on short-termism.[1]

We are grateful to Sageworks for access to their database on private companies and to Drew White and Tim Keogh of Sageworks for their help and advice regarding their data. Thanks for helpful comments and suggestions go to Bo Becker, Mary Billings, Amy Dittmar, Jesse Edgerton, Yaniv Grinstein, David Hirshleifer (the Editor), Josh Lerner, Gustavo Manso (our AFA discussant), Hamid Mehran, Bruce Petersen, Joshua Rauh, Michael Roberts, Michael Schill, Stanley Zin, and two anonymous referees. We are grateful to Mary Billings for sharing her ERC data with us and the Edward Lowe Foundation for giving us access to the NETS database. Ljungqvist gratefully acknowledges generous financial support from the Ewing M. Kauffman Foundation under the Berkley-Kauffman Grant Program. Supplementary data can be found on *The Review of Financial Studies* web site. Send correspondence to Alexander Ljungqvist, New York University, Stern School of Business, Suite 9-160, 44 West Fourth Street, New York, NY 10012-1126; telephone 212-998-0304; fax: 212-995-4220. E-mail: al75@nyu.edu.

1 Writing in the Harvard Business Review, Barton and Wiseman (2014), global managing director at McKinsey & Co. and CEO of the Canada Pension Plan Investment Board, respectively, argue that "the ongoing short-termism in the business world is undermining corporate investment, holding back economic growth." The Kay Review of U.K. Equity Markets and Long-Term Decision-Making, published in 2012, echoes this sentiment, while similar

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Yet systematic empirical evidence of widespread short-termism has proved elusive, largely because identifying its effects is challenging. A chief challenge is the difficulty of finding a plausible counterfactual for how firms would invest absent short-termist pressures. We address this difficulty by comparing the investment behavior of stock market-listed firms to that of comparable privately held firms, using a novel panel dataset of private U.S. firms covering more than 400,000 firm years over the period 2001–2011. Building on prior work, such as Jensen and Meckling (1976) and Jensen (1989), our key identification assumption is that, on average, private firms suffer from fewer agency problems and, in particular, are subject to fewer short-termist pressures than are their listed counterparts. This assumption is motivated by the fact that private firms are often owner managed and, even when not, are both illiquid and typically have highly concentrated ownership. These features encourage their owners to monitor management more closely to ensure long-term value is maximized (Bhide 1993).

As Holmström (1982), Narayanan (1985), Miller and Rock (1985), Stein (1989), Shleifer and Vishny (1990), and von Thadden (1995) have argued, a focus on a firm's short-term profits or its current share price will distort investment decisions from the first-best if investors have incomplete information about how much the firm should invest to maximize its long-term value. Forgoing positive net present value (NPV) projects boosts current earnings and thereby today's share price by reducing "depreciation charges to earnings or other start-up charges" (Graham, Harvey, and Rajgopal 2005). We extend Holmström (1982) model to show that short-termism induces public-firm managers not only to choose inefficiently low investment levels but also to be less sensitive to changes in investment opportunities than their private counterparts. As in Grenadier and Wang (2005), this occurs even though investors can perfectly observe a firm's actual investment.

Our empirical results are consistent with these two predictions. We first show that private firms invest substantially more than do public ones on average, holding firm size, industry, and investment opportunities constant. This pattern is surprising in light of the fact that a stock market listing gives firms access to cheaper investment capital. Second, we show that private firms' investment decisions are around 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, Cummins, Hassett, and Oliner 2006; Bloom, Bond, and van Reenen 2007). 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 invest more and are

points have been made in *The Economist* ("The profits prophet," October 5, 2013) and the *Financial Times* ("Corporate investment: A mysterious divergence," July 24, 2013).

more sensitive to investment opportunities in the five years before they go public than in those after. Indeed, once they have gone public, their investment sensitivity becomes indistinguishable from that of observably similar, already-public firms. Our results are robust to a variety of matching criteria, to using alternatives measures of investment and investment opportunities, and to instrumenting a firm's listing status with plausibly exogenous variation in the supply of start-up funding across U.S. states and time.

The stylized facts we document are consistent with public-firm managers behaving in a short-termist way, but they may also be consistent with other hypotheses, such as a preference for a quiet life (Bertrand and Mullainathan 2003). To investigate this further, we test a key cross-sectional prediction that is unique to short-termism models: a public-firm manager's incentive to engage in short-termist behavior—and thus the associated distortion in investment behavior—should increase in the sensitivity of his firm's share price to earnings news.

To test this prediction, we follow the accounting literature and measure the sensitivity of share prices to earnings news using "earnings response coefficients" or ERC (Ball and Brown 1968). If short-termism explains the difference in investment sensitivity between public and private firms, then this difference should increase in ERC. This is precisely what we find. When share prices are unresponsive to earnings news (ERC = 0), investment sensitivities are no different, consistent with the absence of an incentive to distort investment to boost the share price. As ERC increases, public firms' investment sensitivity falls significantly while that of private firms remains unchanged. In other words, the difference in sensitivities increases in ERC, and this increase is driven by a change in the behavior of public firms.

This conclusion is supported by auxiliary evidence showing that investment sensitivity is especially low among public firms with high levels of transient (i.e., short-term focused) institutional ownership and those with a propensity to "meet or beat" analysts' earnings forecasts. These cross-sectional patterns are consistent with the notion that short-termist pressures induce public firms to invest myopically.

Our paper makes three contributions. First, our large-sample results complement existing anecdotal and survey evidence of widespread short-termism in the United States. 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. More recently, Graham, Harvey, and Rajgopal (2005, 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. But it is an empirical question of 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.

Second, we document economically important differences in the investment behavior of private and public firms. Almost everything we know about investment at the microlevel is based on evidence from public firms, whose data are readily available but which number only a few thousand.[2] Yet private firms form a substantial part of the U.S. economy. We estimate that in 2010, private U.S. firms accounted for 52.8% of aggregate nonresidential fixed investment, 68.7% of private-sector employment, 58.7% of sales, and 48.9% of aggregate pretax profits. Nearly all of the 5.7 million firms in the United States are private (only 0.06% are listed), and while many are of course small, private firms predominate even among the larger ones: in 2010, for example, 86.4% of firms with 500 or more employees were privately held.[3]

Given that we compare public and private firms of similar size, a potential caveat is that our results may not generalize beyond smaller public firms. Further tests show that this is not the case. The difference in investment levels is actually larger when we compare unmatched public and private firms. In addition, we find 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 in turn are substantially more sensitive to investment opportunities than are the public firms in the other nine deciles.

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

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 mostly rely on debt financing.

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2 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; Cooper and Haltiwanger 2006).

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 use CRSP-Compustat data for U.S. firms listed on the NYSE, AMEX, or Nasdaq. Sales data are from 2007, the most recent year for which they are available.

Before Sageworks became available, studies of private U.S. firms relied on limited samples. Gao and Li (2014) use the CapitalIQ database to compare CEO compensation in public firms and in an unusual set of private firms: those with an SEC registration. The results show that the pay-performance link is stronger in public firms. Because the point of an incentive contract is to overcome an agency problem, these patterns are consistent with the literature's assumption, dating back at least to Jensen (1989), that private firms are subject to fewer agency problems than public firms. They are also consistent with Edgerton's (2012) finding that public firms, compared with observably similar private firms, overuse corporate jets.

We are aware of two single-industry studies comparing the investment behavior of public and private firms in the United States. 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 natural-gas producers are more responsive to changes in natural gas prices than are 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.

Stein (2003) surveys the large empirical literature on the effects of agency problems on investment. We depart from this literature by exploiting variation along the extensive (public/private) margin. Most existing work instead focuses on the intensive margin.[4] For example, Wurgler (2000) and John, Litov, and Yeung (2008) relate investment among public firms to variation in corporate governance, whereas Fang, Tian, and Tice (2014) examine whether public firms with more liquid shares (and thus more footloose investors) are less innovative. Ladika and Sautner (2014) use a quasi-experiment to show that public-firm managers who are given short-term incentives myopically reduce investment, whereas Edmans, Fang, and Lewellen (2014) show that the imminent vesting of public-firm managers' equity incentives is associated with myopic reductions in spending on capital expenditures, R&D, and advertising. 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 but leads to underperformance over longer horizons. 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

[4] Another paper that, like us, focuses on the extensive margin is Baghai (2012), who shows that public firms in the food retail industry respond to short-term performance pressures by increasing prices relative to their private competitors.

(1998) shows that these tendencies are mitigated in the presence of high institutional ownership.

One paper that reaches the opposite conclusion to this body of literature is Bharath, Dittmar, and Sivadasan (2014). They study U.S. manufacturing firms that were taken private and find no evidence of subsequent productivity gains. This suggests that addressing inefficiencies induced by short-termist behavior is not a primary motivation behind the public-to-private transitions in their sample.

2. Sample and Data

According to the census, there were 5,734,538 firms in the United States in 2010.[5] The vast majority are privately held (in 2010, there were only 3,716 U.S. firms with a listing on a U.S. exchange), and even among the very largest private firms, most express no desire to go public.[6] Unless they issue public bonds or have more than 500 shareholders (2,000 shareholders since April 2012), private firms are not subject to SEC disclosure requirements, so little is known about how they invest. Our study is possible only because a new database on private U.S. firms, created by Sageworks, has recently become available.

Like Compustat, Sageworks reports accounting data from income statements and balance sheets along with demographic data, such as NAICS industry codes and geographic location. Unlike Compustat, Sageworks provides data for private firms.[7] Unfortunately, Sageworks masks firm names, though each firm has a unique identifier allowing us to construct a panel.[8] The main drawback of anonymity for our purposes is that we cannot observe transitions from private to public status in the Sageworks database. We will later describe how we assemble a dataset of such transitions from other sources.

Sageworks obtains data not from the private firms themselves, which could raise selection concerns, but from a large number of accounting firms that feed data for all their unlisted corporate clients into Sageworks' database. Selection thus operates at the level of the accounting firm and not of their clients. Sageworks cooperates with most of the largest national accounting firms, as well as from hundreds of regional players, but with proportionately fewer of the many thousand local accountants who service the smallest firms in the United States. As a result, the main selection effect is that Sageworks' coverage is biased

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[5] This figure does not include the self-employed (source: http://www.census.gov/econ/susb).

[6] In Brau and Fawcett's (2006) survey of large private firms, only 10.5% of firms had considered going public.

[7] The firms in Sageworks are either standalone companies or the parent companies of groups of privately held firms.

[8] Commercial users of the Sageworks database only have access to data aggregated by industry and region. This alleviates potential disclosure concerns on the part of the private firms. Only a few academic researchers, ourselves included, have had confidential access to an anonymized version of the underlying firm-by-firm data.

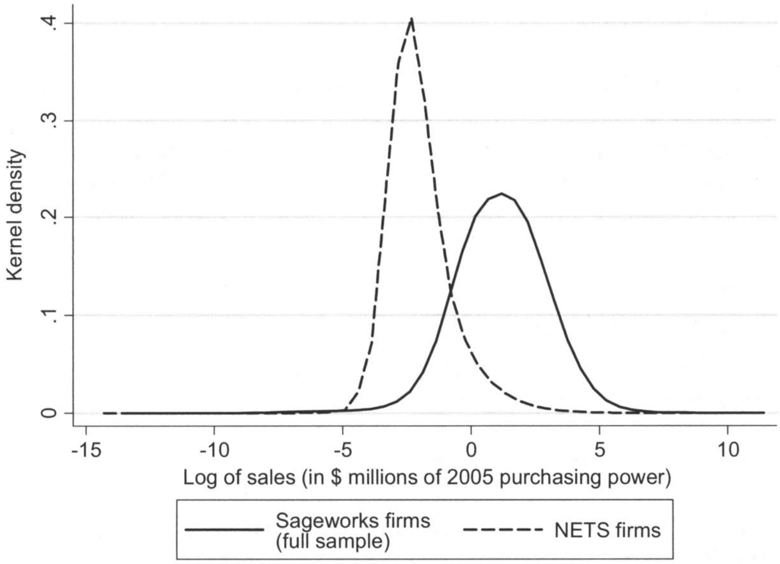


Figure 1
Comparing the size distribution of private firms in Sageworks and in NETS
This graph compares the size distribution of private firms in the full sample of Sageworks and in the National Establishment Time Series (NETS), a database that contains data on employment, estimated sales, location, industry, 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. (Nor does NETS contain data on investment and so cannot be used as a substitute for Sageworks in our investment analysis.) 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.

toward 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. Much of the mass of Sageworks firms is to the right of NETS firms, in terms of 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 an unbalanced ten-year panel.[9] Figure 2 illustrates the growth of the Sageworks database over time.

9 Sageworks is free of survivorship bias. If a firm goes public, dies, or switches to an accounting firm that does not cooperate with Sageworks, its time series will end, but all of its historical data remains in the database.

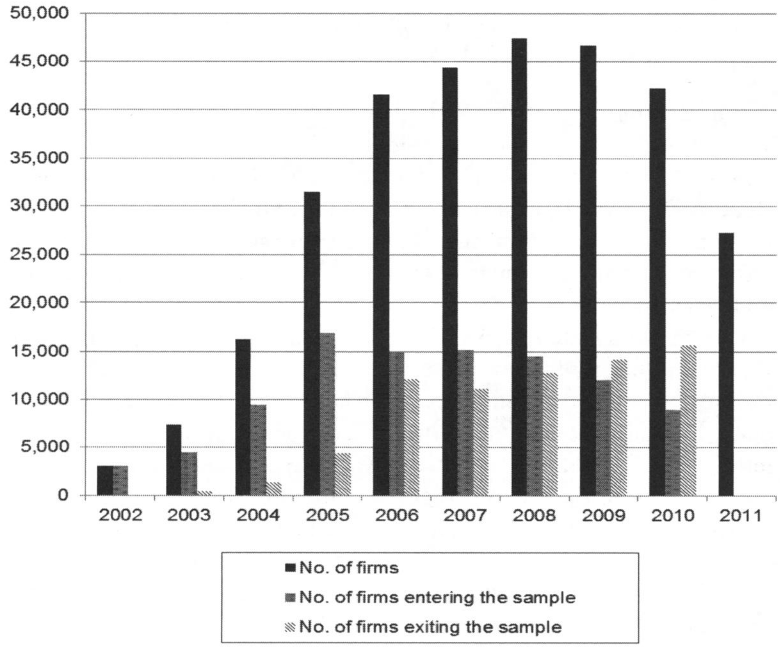


Figure 2
The Sageworks dataset: Distribution by year
This graph illustrates the growth of the Sageworks database over time by showing the distribution by year of the 307,803 firm-year observations in the full Sageworks sample, corresponding to 99,040 unique firms over the period from 2002 to 2011. The figure also reports the number of firms entering and exiting the sample per year.

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 nonprofits, 32,686 firms whose data 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 409,762 firm-years for 99,040 private firms over the period from 2001 to 2011.

To be part of our public-firm sample, a firm has to be in both Compustat and CRSP, be incorporated in the United States and listed on the NYSE, AMEX, or Nasdaq, have valid stock prices in CRSP in three consecutive years, have a CRSP share code of 10 or 11 (which screens out nonoperating entities, such as real estate investment trusts, mutual funds, or closed-end funds), and be neither a financial firm nor a regulated utility. Filtering gives us 34,216 firm-years for 4,360 public firms from 2001–2011.

2.2 Matching

To control for observable differences between public and private firms, we follow prior literature, such as Saunders and Steffen (2011), Michaely and Roberts (2012), and Gao, Harford, and Li (2013) and use a matching procedure. Our 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.

Our baseline match follows Gao, Harford, and Li (2013) in using size and industry. (Our results prove robust to matching on additional characteristics.) Before matching, public firms are much larger than private firms. Table 1 shows that the mean (median) public firm in Compustat has total assets of $2,869.4 million ($392.2 million), compared with $13.5 million ($1.2 million) for the private firms in Sageworks. Figure 3 plots the two size distributions; they overlap only to a limited extent. Gala and Julio (2011) find that investment increases in firm size, so it is important to hold size constant in our tests. Matching on size does so by creating a sample of small public and large private firms. We additionally match on industry, as investment has been shown to vary considerably across industries (Jorgenson 1971), and the public and private firms in our sample are drawn from different industry distributions (see Figure 4).

In the language of the matching literature surveyed by Imbens and Wooldridge (2009), we use a caliper-based nearest-neighbor match adapted to a panel setting. Starting in 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 two (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 remains intact. This allows us to estimate within-firm investment regressions. We 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.

The bottom graph in Figure 3 shows that matching produces nearly identical size distributions. Two standard statistical measures of match quality confirm

[10] Our results are robust to using finer industry classifications and to imposing tighter calipers on the maximum size difference.

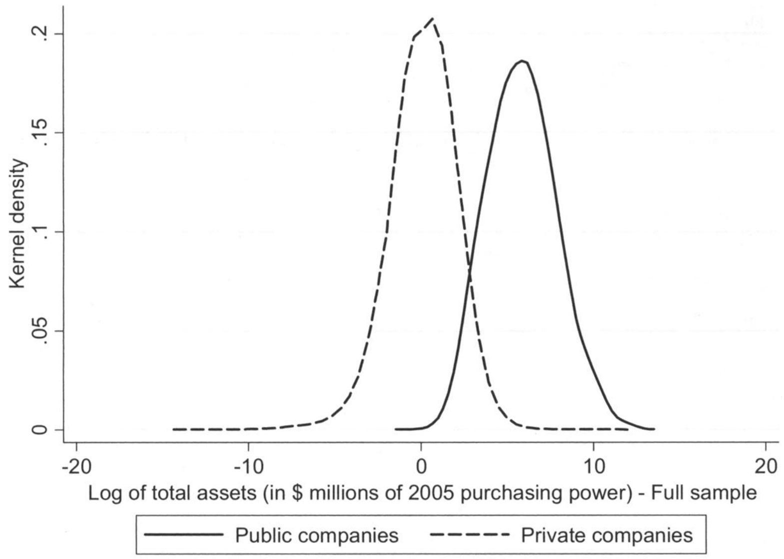
[11] As Smith and Todd (2005) point out, matching with replacement involves a trade-off between bias and efficiency. Bias is reduced as higher quality matches are generated, but efficiency is reduced as fewer distinct observations are used.

Table 1
Descriptive statistics

		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***
	SD	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***
	SD	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
	SD	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***
	SD	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***
	SD	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***
	SD	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***
	SD	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***
	SD	2.057	0.967		2.704	0.827	
Dividend/*TA*	Mean	0.012	0.049	−0.037***	0.012	0.004	0.009***
	Median	0.000	0.000	0.000	0.000	0.000	0.000
	SD	0.060	0.265		0.076	0.032	
Age	Mean	40.7	26.2	14.5***	29.9	36.3	−6.3***
	Median	26.0	24.0	2.0***	22.0	35.0	−7.0***
	SD	36.3	12.8		25.5	16.8	
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	

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 (2001 is used to construct lags). 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 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. 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.

this. The first is Hotelling's T^2test. The data cannot reject the null that the means of total assets are equal across groups ($p=0.276$). The second test of match quality is Rosenbaum and Rubin's (1985) *SDIFFF* test. Although critical values have not yet been derived, Rosenbaum and Rubin suggest that a value of twenty warrants concern about the extent to which the matched groups are



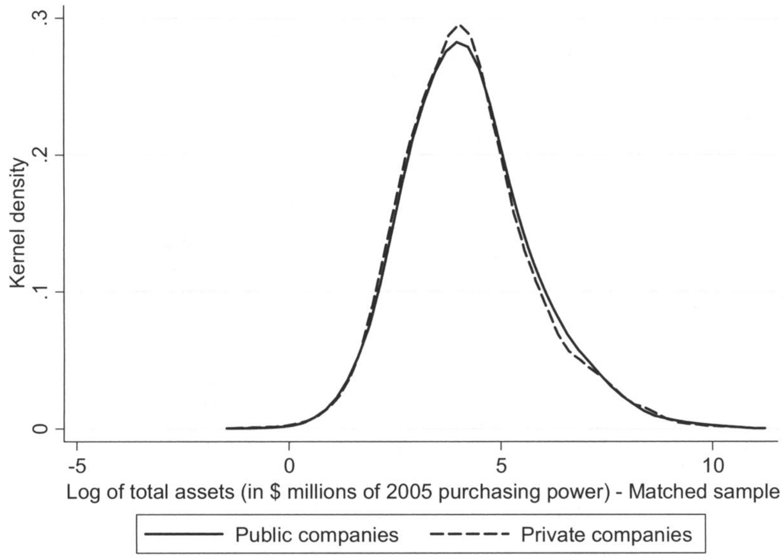


Figure 3
Size distribution of public and private sample firms
The top graph shows the size distribution of the public and private firms in our full samples of Compustat and Sageworks firms. The bottom graph shows the size distribution of the public and private firms in our size-and-industry matched sample. 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.

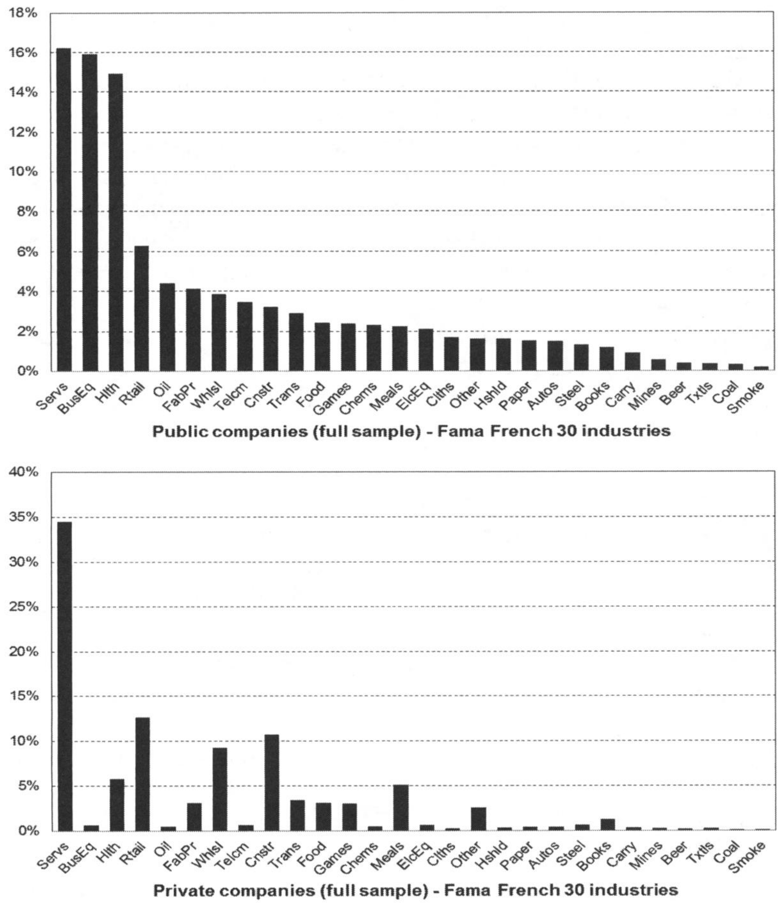


Figure 4
Industry distribution of public and private sample firms
The top and bottom graphs show the industry distribution of the public and private firms in our full samples of Compustat and Sageworks firms, respectively. We use Fama-French (1997) 30 industry definitions, excluding utilities and financial firms.

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. 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, we measure investment in a way that captures both. This can be done by modeling *gross investment*, defined as the annual increase in gross fixed assets (i.e., gross property, plant, and equipment) scaled by beginning-of-year total assets. 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 a firm's investment decisions.

For robustness, we also construct measures of investment that capture intangibles, specifically the annual change in noncurrent assets (gross or net of depreciation) and the change in total assets.[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 because 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, for example, by Lehn and Poulsen (1989), Shin and Stulz (1998), Bloom, Bond, and van Reenen (2007), and Michaely and Roberts (2012).

For robustness purposes, we also use 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 (2013) 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 private firm.

Finally, we explore an exogenous shock to investment opportunities: state-level variation in corporate income tax rates. As Cummins, Hassett, and Hubbard (1994) argue, this allows us to sidestep the need to directly measure investment opportunities. Over our sample period, there are 27 tax cuts and

[12] The change in noncurrent assets captures investment in both fixed and intangible assets. Another form of investment, R&D, is usually expensed and so does not affect intangibles. We cannot model investment in R&D as Sageworks does not break out R&D spending. We will report evidence showing it is highly unlikely that our results are driven by this data limitation.

13 tax increases in a total of 20 states; 380 public and 366 private firms in our matched sample face tax cuts, whereas 188 public and 226 private firms face tax rises. For example, in 2005, Kentucky cut its corporate tax rate from 8.25% to 7%. The average size of tax cuts and tax increases is –0.55 and 0.64 percentage points, respectively.

2.5 Other firm characteristics

Table 1 shows that after we match on size and industry, private firms are younger and have higher ROA, less cash, more debt, more retained earnings, and lower dividends. These patterns are consistent with those documented in prior studies. They reflect 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.[13]

3. Differences in Public and Private Firm Investment Behavior

Our empirical strategy uses private firms as a counterfactual for how stock market-listed firms would invest absent short-termist pressures. The finance literature has long argued that listed firms are prone to agency problems. While a listing provides access to a deep pool of low cost capital, it can also have two detrimental consequences. 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 (Jensen and Meckling 1976). Second, liquidity makes it easy for shareholders to sell their stock at the first sign of trouble rather than actively monitor management—a practice sometimes called the "Wall Street walk." This can weaken incentives for effective corporate governance (Bhide 1993).

Private firms, in contrast, tend to have illiquid and concentrated ownership and so fewer agency problems. Analysis of the Federal Reserve's 2003 Survey of Small Business Finances (SSBF), for example, shows that 94.1% of the larger private firms in the United States have fewer than ten shareholders (most have fewer than three) and that 83.2% are managed by the controlling shareholder.[14]

We begin by documenting that public firms behave in a way that is consistent with the predictions of short-termism models (derived in the

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[13] The purpose of matching is not to eliminate all observable differences between public and private firms but to make firms comparable along the dimensions thought to affect the outcome variable of interest (here: investment). Overmatching on dimensions unrelated to the outcome variable of interest results in samples that are unrepresentative of their respective populations. In other words, we can make matched firms arbitrarily similar to each other on arbitrarily many dimensions, but as we do so, the firms that end up in the matched sample become ever less representative of their respective groups. See Heckman, LaLonde, and Smith (1999) for an exhaustive discussion of this point.

[14] 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.

Internet Appendix): they invest substantially less than private firms and are less responsive to changes in investment opportunities. We verify that these differences in investment behavior are not driven by our sampling or methodological choices or by differences in the role of intangibles, tax treatment, accounting choices, or observable firm characteristics.

3.1 Differences in investment levels

Table 2 establishes our first result: that private firms invest significantly more than public firms on average. The difference is substantial. Row 1 in Table 2, panel A, shows that in the full samples, private firms increase gross fixed assets by 7.5% of total assets a year on average, compared with 4.1% for public firms. Matching on size and industry, as shown in row 2, does not close the gap: private firms continue to outinvest public firms, by 6.8% to 3.7% on average. Figure 5 shows that this is true in eight of the ten 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.

Lower investment among public firms is not due to how we measure investment. Rows 3 and 4 show that private firms continue to outinvest public ones when we augment gross investment in fixed assets with investment in intangibles, such as goodwill or advertising. The gap averages between 2.6 and 4.6 percentage points.[15] Rows 5 and 6 show similar patterns for net (rather than gross) investment: private firms invest 2.9 percentage points more in net fixed assets and 4.4 percentage points more when we include intangibles. In row 7, the gap widens further, to 6.5 percentage points, when we compare growth in total assets. Row 8 shows that matching on finer industry codes, such as 5-digit NAICS, has virtually no effect on our results. Including other characteristics besides size and industry among the matching criteria does not close the gap either. Row 9 controls for lifecycle differences between public and private firms by adding age to our size-and-industry match. This widens the gap to 5.5 percentage points.[16] Row 10 follows Michaely and Roberts (2012) by matching on size, NAICS4 industry, sales growth, ROA, book leverage, and cash. This yields a gap of 2.3 percentage points in favor of private firms. Finally, consistent with Figure 5, rows 11 and 12 show that private firms significantly outinvest their public counterparts, both before and after the 2008–2009 financial crisis.

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[15] Sageworks does not report R&D data, so we cannot test for differences in R&D spending. Moreover, Sageworks firms are anonymous, so we cannot obtain information about their patenting activity. However, Bernstein's (Forthcoming) results suggest that including innovation is unlikely to close the investment gap between public and private firms: he finds that going public leaves a firm's scale of innovation unchanged (as measured by the number of patents), while reducing the novelty of its innovations (as measured by patent citations).

[16] To match on other variables besides industry and size, we construct a propensity score based on size and 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.

Table 2
Unconditional investment levels

Panel A: Means, medians, and standard deviations

			Public firms					Private firms					Public - private firms	
Row	Sample	Investment measure	Mean	Median	SD	No. of obs.	No. of firms	Mean	Median	SD	No. of obs.	No. of firms	Diff. in means	Diff. in medians
1	Full sample	Gross	**0.041**	0.020	0.154	29,718	4,360	**0.075**	0.014	0.303	307,803	99,040	−0.034***	0.006***
Samples matched on														
2	NAICS4, size	Gross	**0.037**	0.015	0.183	11,372	2,595	**0.068**	0.016	0.260	11,372	1,476	−0.031***	−0.001
3	NAICS4, size	Gross NCA	**0.074**	0.020	0.334	11,227	2,563	**0.120**	0.026	0.377	11,227	1,452	−0.046***	−0.006***
4	NAICS4, size	Gross + adv.	**0.052**	0.022	0.201	11,372	2,595	**0.078**	0.021	0.275	11,372	1,476	−0.026***	0.001
5	NAICS4, size	Net	**0.019**	0.000	0.139	11,372	2,595	**0.048**	0.004	0.214	11,372	1,476	−0.029***	−0.004***
6	NAICS4, size	Net NCA	**0.055**	0.000	0.296	11,227	2,563	**0.099**	0.009	0.325	11,277	1,452	−0.044***	−0.009***
7	NAICS4, size	Growth TA	**0.127**	0.034	0.586	11,372	2,595	**0.192**	0.075	0.515	11,372	2,595	−0.065***	−0.041***
8	NAICS5, size	Gross	**0.038**	0.015	0.190	9,884	2,331	**0.070**	0.016	0.265	9,884	1,301	−0.032***	−0.001
9	NAICS4, size, age	Gross	**0.042**	0.020	0.162	20,765	3,518	**0.097**	0.019	0.375	20,765	3,284	−0.055***	0.001
10	NAICS4, size, leverage, cash, sales growth, ROA	Gross	**0.044**	0.019	0.169	16,217	3,491	**0.068**	0.020	0.209	16,217	3,057	−0.023***	−0.001
11	NAICS4, size; 2002–2007	Gross	**0.044**	0.018	0.199	7,005	2,259	**0.077**	0.016	0.269	7,005	913	−0.033***	0.002
12	NAICS4, size; 2008–2011	Gross	**0.026**	0.012	0.152	4,367	1,683	**0.053**	0.017	0.245	4,367	898	−0.027***	−0.005***

Panel B: Investment percentiles

	Investment measure: Gross investment/lagged total assets						
Percentile	25th	50th	60th	70th	80th	90th	95th
Public	0.000	0.015	0.024	0.037	0.060	0.123	0.223
Private	0.000	0.016	0.027	0.046	0.075	0.168	0.358
Difference	0.000	−0.001	−0.003	−0.009	−0.015	−0.045	−0.135

Panel A compares unconditional 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. In rows 1, 2, and 8 through 12, the investment measure is gross investment (the annual change in gross fixed assets—that is, property, plant, and equipment—scaled by beginning-of-year total assets). Rows 3 and 4 capture investment in both gross fixed assets and intangibles. Specifically, row 3 shows the annual change in noncurrent assets (NCA) gross of depreciation, whereas row 4 adds advertising expenses to gross investment in fixed assets. Row 5 uses net investment (the annual change in net fixed assets scaled by beginning-of-year total assets), to which row 6 adds investment in intangibles by modeling the change in noncurrent assets net of depreciation. Row 7 models the growth in total assets (the annual change in total assets scaled by beginning-of-year total assets). Rows 11 and 12 split the sample into two periods: precrisis (2002–2007) and postcrisis (2008–2011). The final two columns report 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. Panel B compares gross investment in fixed assets for public and private firms at the 25th, 50th, 60th, 70th, 80th, 90th, and 95th percentiles of the baseline matched sample. For variable definitions and details of their construction, see Appendix A. Each investment measure is winsorized 0.5% in each tail to reduce the impact of outliers.

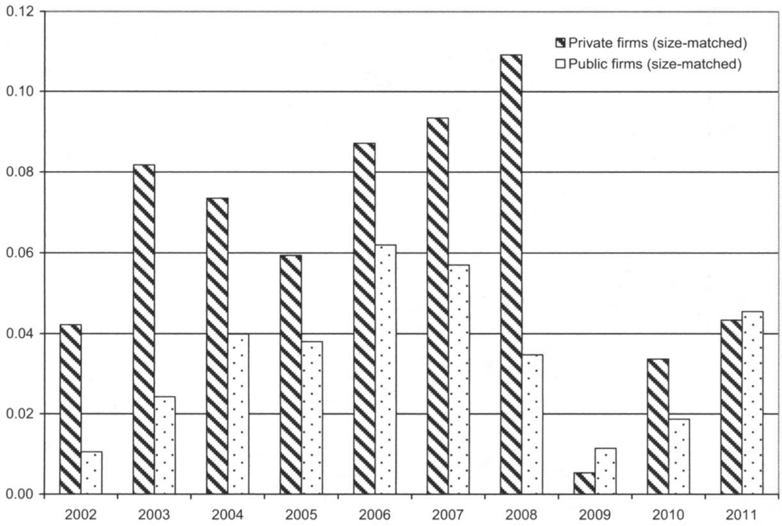

Figure 5
Public and private firm investment levels, 2002–2011
The figure shows the average annual change in gross fixed assets (scaled by total assets) for public and private firms in our size-and-industry matched sample. All pairwise differences are statistically significantly different from zero except for the years 2009 and 2011.

In sum, private firms invest more than do public firms on average, whichever way we measure investment. Medians, on the other hand, differ much less: consistent with the findings of the investment literature, we find that neither the median private nor the median public firm year in our sample involves much investment. This well-known lumpiness in investment explains why work on corporate investment usually focuses on averages rather than on medians (see Thomas 2002 and references therein).

Given that investment tends to be lumpy, it is possible that the difference in average investment levels between public and private firms is driven by outliers.[17] To investigate this possibility, panel B of Table 2 compares gross investment in fixed assets for matched public and private firms at different points in their respective distributions. Private firms outinvest public firms at any point above the median that is, not just in the right tail. At the 80th percentile, for example, public firms invest 6% of assets, whereas private firms invest 7.5% of assets. A Wilcoxon rank-sum test rejects the null that the two distributions are the same ($p<0.001$).

Private firms might outinvest public ones because they face better investment opportunities. To hold investment opportunities constant, we next estimate the

[17] All measures of investment, along with all other continuous variables used in the paper, are winsorized 0.5% in each tail to reduce the impact of outliers. Results are robust to alternative cutoffs.

following investment regression:

$$\frac{I_{it}}{A_{it-1}} = \alpha PUBLIC_i + \beta\left(\frac{S_{it}-S_{it-1}}{S_{it-1}}\right) + \delta\left(\frac{Z_{it}}{A_{it-1}}\right) + \varphi_j + \eta_t + \varepsilon_{it}, \quad (1)$$

where i, j, and t index firms, industries, and fiscal years, respectively, I is one of the investment measures introduced in Table 2, A is total assets, S is sales, Z is operating income before depreciation,[18] and *PUBLIC* is a dummy for whether the firm is listed on the stock market. We remove unobserved industry-level heterogeneity through NAICS4-industry fixed effects and include year effects to control for common trends.[19] Standard errors are clustered at the firm level in the usual manner.

Table 3 reports the results of estimating Equation (1) for each investment measure and each sample used in Table 2. Holding investment opportunities and ROA constant does not alter our findings: private firms outinvest public ones by a similar magnitude as in the unconditional tests in Table 2, and the difference is statistically significant at the 5% level or better in all twelve specifications.

3.2 Differences in investment sensitivity

Tables 2 and 3 show that private firms invest more than do their public counterparts, even after controlling for differences in investment opportunities. This is a novel and perhaps surprising finding, particularly in light of the fact that a common reason to go public is to access cheaper investment capital (Brau and Fawcett 2006). At the same time, public firms have higher payout ratios than do their private counterparts (see Table 1). These patterns are consistent with the notion that public firms inefficiently prioritize high payouts over positive NPV investments due to short-termist pressures. However, they are also consistent with a number of alternative interpretations, such as private firms overinvesting because their managers are closet empire builders.

To help disentangle these possibilities, we examine how public and private firms respond to changes in investment opportunities. According to neoclassical q theory, firms should invest more as their investment opportunities improve, up to the point at which their marginal q equals one. An enduring empirical puzzle is that public firms—the focus of virtually all extant work on corporate investment—appear to have lower sensitivity to investment opportunities than predicted by q theory. How private firms score on this dimension is unknown. Comparing public and private firms' investment sensitivities is thus in and of

[18] 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 conditioning on ROA.

[19] Recall that all firms in Sageworks are unlisted and all firms in Compustat are listed. Thus, when comparing differences in investment levels between public and private firms, we cannot include firm fixed effects.

Table 3
Conditional investment levels

		Matched on										
Sample	Full	NAICS4, size						NAICS5, size	NAICS4, size, age	NAICS4, size, ROA, sales gr., lev., cash	NAICS4, size; 2002–2007	NAICS4, size; 2008–2011
Inv. measure:	Gross	Gross	Gross NCA	Gross + advert.	Net	Net NCA	Growth in total assets	Gross	Gross	Gross	Gross	Gross
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Public	−0.030***	−0.024***	−0.049***	−0.021**	−0.029***	−0.048***	−0.091***	−0.026***	−0.053***	−0.015***	−0.026**	−0.024**
	0.002	*0.009*	*0.016*	*0.009*	*0.009*	*0.015*	*0.021*	*0.010*	*0.013*	*0.004*	*0.012*	*0.010*
Inv. opps.	0.070***	0.059***	0.119***	0.065***	0.049***	0.102***	0.228***	0.060***	0.090***	0.085***	0.071***	0.043***
	0.002	*0.009*	*0.016*	*0.010*	*0.007*	*0.015*	*0.026*	*0.009*	*0.015*	*0.012*	*0.013*	*0.009*
ROA	0.000	0.066***	0.020	0.063**	0.013	0.014	−0.107	0.052**	−0.025	0.008	0.079***	0.026
	0.001	*0.024*	*0.049*	*0.025*	*0.037*	*0.048*	*0.078*	*0.022*	*0.043*	*0.011*	*0.028*	*0.026*
R^2	5.2%	17.3%	13.5%	16.8%	16.2%	13.5%	16.8%	18.0%	12.4%	16.3%	23.5%	12.1%
No. obs.	337,521	22,744	22,454	22,744	22,744	22,454	22,744	19,768	41,530	32,434	14,010	8,734
No. firms	103,400	4,071	4,015	4,071	4,071	4,015	4,071	3,632	6,802	6,548	3,172	2,581

This table reports the results of estimating investment Equation (1), which estimates differences in investment levels between public and private firms holding investment opportunities and profitability constant. Specifically, the table compares conditional investment levels 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. Each column in Table 3 uses the same sample and investment measure as the corresponding row in Table 2, panel A. We use sales growth to proxy for investment opportunities and ROA to measure profitability. For variable definitions and details of their construction, see Appendix A. Each regression includes industry (NAICS4) and year fixed effects (not reported). Heteroscedasticity-consistent standard errors clustered at the firm level are shown in italics beneath 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.

itself interesting. Importantly, it will also shed light on whether public firms' lower investment levels are the result of short-termist pressures: as we show in the Internet Appendix, short-termism induces public-firm managers not only to choose inefficiently low levels of investment but also to be less sensitive to changes in investment opportunities.

We estimate standard investment regressions in the style of the q theory literature as follows:

$$\frac{I_{it}}{A_{it-1}} = \alpha\left(\frac{S_{it}-S_{it-1}}{S_{it-1}}\right) + \beta\left\{PUBLIC_i \times \left(\frac{S_{it}-S_{it-1}}{S_{it-1}}\right)\right\} + \delta\left(\frac{Z_{it}}{A_{it-1}}\right)$$
$$+\phi\left\{PUBLIC_i \times \left(\frac{Z_{it}}{A_{it-1}}\right)\right\} + \mu_i + \eta_t + \varepsilon_{it}. \qquad (2)$$

Unlike Equation (1), Equation (2) includes firm fixed effects. This allows us to focus on investment sensitivities by estimating within-firm variation in investment in response to within-firm variation in investment opportunities. Interacting investment opportunities with the public-firm indicator allows us to compare the investment sensitivities of public and private firms.[20]

3.2.1 Baseline results. The results in Column 1 of Table 4 suggest that public firms' investment decisions are significantly 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. This difference in investment sensitivities is not due to outliers. When we estimate Equation (2) using quantile regressions for each investment decile, shown in Figure 6, we find significantly lower sensitivities among public firms in all but the smallest investment decile. 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. 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.

[20] 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.

Table 4
Sensitivity to investment opportunities

	Dependent variable: Gross investment/lagged total assets									
Proxy for inv. opps.	Sales growth				Ind. q	Pred. q	Tax shock	Sales growth		
									Pre-IPO matching	
Sample	Matched on size and industry	Matched on size and industry	Matched public firms only	All public firms	Matched on size and industry	Matched on size and industry	Matched on size and industry (C Corps only)	Matched on size, sales growth, ROA, cash, leverage & industry	Matched on size and industry	Matched on size, ROA, cash, debt & industry
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Investment opportunities	0.118***		0.028***	0.037***	0.066**	0.272***	−0.042**	0.134***	0.149**	0.077**
	0.029		*0.006*	*0.005*	*0.028*	*0.068*	*0.020*	*0.027*	*0.060*	*0.036*
x public	−0.091***				−0.080**	−0.148**	0.038*	−0.078***	−0.139**	−0.070*
	0.030				*0.035*	*0.064*	*0.021*	*0.029*	*0.060*	*0.036*
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	−0.015	−0.032	−0.014
	0.032	*0.024*	*0.018*	*0.023*	*0.052*	*0.072*	*0.007*	*0.021*	*0.043*	*0.025*
x public	−0.050	−0.075**			−0.013	−0.151**	0.031*	0.032	0.049	0.028
	0.037	*0.031*			*0.055*	*0.072*	*0.018*	*0.039*	*0.053*	*0.040*
R^2 (within)	8.8%	10.4%	4.5%	5.0%	3.7%	8.0%	2.0%	8.3%	8.3%	3.8%
F-test: All coeff. = 0	9.2***	9.0***	11.6***	29.6***	7.6***	9.0***	7.1***	13.5***	4.8***	5.4***
No. observations	22,744	22,744	11,372	29,718	22,744	22,744	19,177	32,434	8,739	8,846
No. firms	4,071	4,071	2,595	4,360	4,071	4,071	3,471	6,548	848	985

This table reports the results of estimating investment Equation (2), which 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 Columns 7 and 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 used in Columns 1, 2, and 5 through 8, see Section 2.2. In Columns 9 and 10, we limit the sample of public firms to firms that went public between 2003 and 2007, which we match up to five private firms (that will remain private) two years before the IPO and then compare their investment after the IPO (see Section 3.2.2 for details). All regressions include firm fixed effects. Because we compare public and private firms that do not change their listing status (or, in the case of Columns 9 and 10, that change their status before the start of our analysis), the level difference in investment between public and private firms is fully absorbed by the firm fixed effects. (See Table 8, panel A, for a within-firm analysis focused on firms that transition from private to public.) Each regression also includes year effects (not reported). Heteroscedasticity-consistent standard errors are shown in italics underneath the coefficient estimates. They are clustered at the firm level in all columns, except Column 7, where they are clustered at the state level. 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).

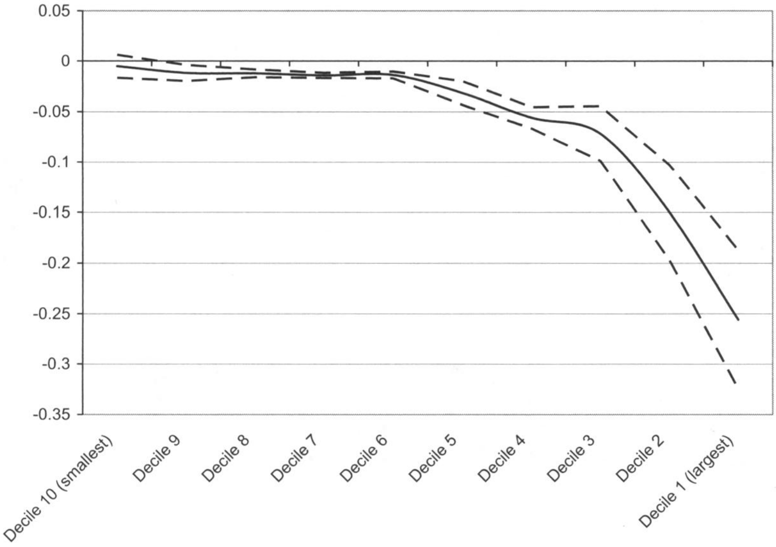

Figure 6
Quantile regressions of investment sensitivity
The figure shows estimates of the difference in investment sensitivities between public and private firms (i.e., the coefficient estimates of the interaction term *Investment opportunities* × *public*) in our baseline matched sample but estimated separately for each investment decile (i.e., at quantiles 5% (decile 10), 15% (decile 9), ..., and 95% (decile 1)). The estimates are obtained from estimating Equation (2) without firm fixed effects (which cannot be combined with quantile regressions) and with a public-firm indicator. Standard errors are obtained by bootstrapping using 500 repetitions each time. The dashed lines represent 95% confidence intervals.

3.2.2 Sampling, measurement, and methodological choices. Our baseline 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. To see whether public firms really invest differently than private firms, we examine the impact of our sampling, measurement, and methodological choices. First, our investment sensitivities model may simply be better specified for private firms. To investigate this, we estimate the model in the sample of matched public firms only. Column 3 reveals that the model for public firms is as good as those in published work that uses public U.S. firms (see, e.g., 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 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 7 assigns the public firms in the full sample to one of ten size deciles based on total

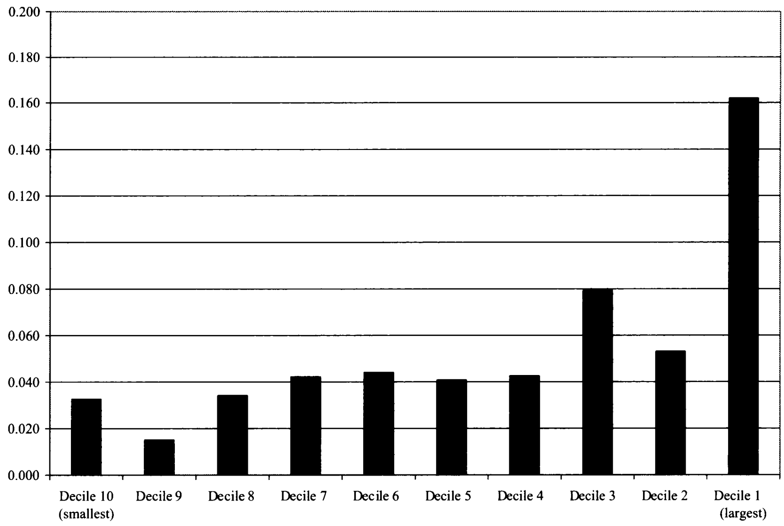

Figure 7
Public-firm investment sensitivities by size decile, 2002–2011
The figure shows estimates of the coefficient on investment opportunities for each size decile from an investment regression that interacts the firm's size decile with sales growth, using data for the Compustat universe over the 2002-2011 period. The investment regression is otherwise specified exactly as in Table 4, 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.

assets and plots the estimated investment sensitivity for each decile.[21] 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 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 0.162, nearly 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 sample firms).[22]

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 4 over consecutive ten-year windows starting in 1970 using Compustat data for all publicly traded firms. Figure 8 shows the resulting

[21] 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.

[22] See Grullon, Hund, and Weston (2013) for related evidence that the investment behavior of the largest public firms differs markedly from that of smaller public firms.

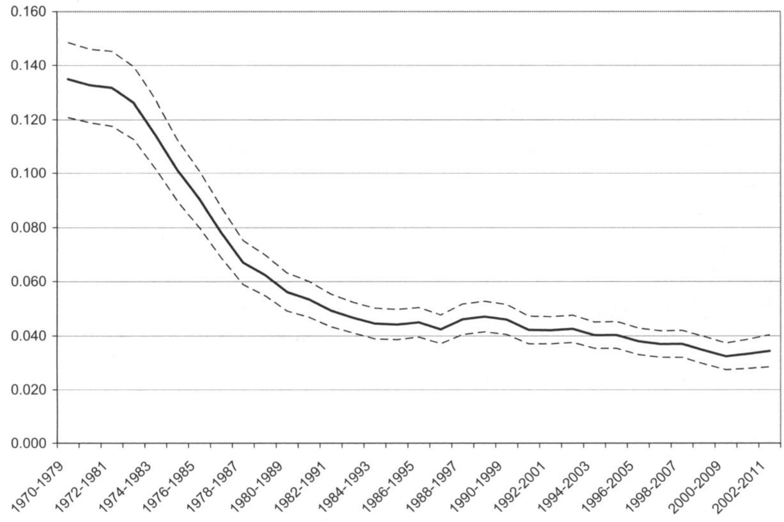

Figure 8
Public-firm investment sensitivities for consecutive ten-year windows, 1970–2011
The figure shows coefficient estimates and 95% confidence intervals for the coefficient on sales growth in standard investment regressions estimated over consecutive ten-year windows starting in 1970 using Compustat 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. 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. The investment regression is specified exactly as in Table 4, Column 4.

coefficient estimates, along with 95% confidence intervals. This reveals that the low public-firm investment sensitivity shown in Table 4 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; they have not been as high as the 0.118 point estimate we find for private firms from 2002–2011 for at least two decades.

Fourth, our results might be driven by our use of sales growth to proxy for investment opportunities. A potential concern is that a revenue increase may allow financially constrained firms to invest more. If private firms are more likely to be financially constrained than are public firms, this might explain their higher sensitivity to sales growth.[23] On closer inspection, this seems unlikely: Farre-Mensa and Ljungqvist (2014) find no evidence of financial constraints

[23] However, the evidence in Figure 7 runs counter to this concern. It shows that investment sensitivity is highest among the largest public firms, which are arguably least likely to be financially constrained.

among large private firms, such as those in our matched sample.[24] We can further alleviate this concern by testing whether our results are robust to using alternative measures of investment opportunities. Tobin's q cannot be computed for private firms for obvious reasons, but we can use either an industry-based q 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 measure investment opportunities altogether. The experiment exploits exogenous variation in corporate tax rates at the state level (see Heider and Ljungqvist [Forthcoming] for further details). A cut in a state's corporate 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 return on investment and so to their investment opportunities. Using a difference-in-differences approach, Column 7 shows that tax changes have a negative effect on the investment of private firms, indicating that they increase investment spending in response to tax cuts and reduce it in response to tax increases ($p=0.040$).[25] Public firms, on the other hand, barely respond to tax changes at all ($p=0.591$).

The tax test is subject to two potential confounds. First, states levy taxes on all corporate activities within their borders, regardless of where a firm is headquartered. If public firms more often operate in multiple states, their investment decisions will be less sensitive to a tax change in their home state. This could potentially explain their lower tax sensitivity. Using hand-collected data on the geography of public firms' operations, Table IA.1 in the Internet Appendix shows that this potential confound does not drive the differential tax sensitivity of public and private firms. Second, private firms' investment may be sensitive to tax changes not because their investment opportunities change but because their after-tax cash flows change. If private firms are more financially constrained than public firms, tax changes could affect their ability to invest differently even if their sensitivity to investment opportunities remained unchanged. As Table IA.1 shows, tax-induced cash flow changes do not appear to drive our results.

Fifth, our matching criteria may be too parsimonious. In particular, our baseline match might compare the best-performing private firms with publicly traded "laggards." To address this concern, we follow Michaely and Roberts (2012) and augment our matching criteria to include size, sales growth, ROA,

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24 Farre-Mensa and Ljungqvist (2014) show that the top quartile by size of Sageworks private firms are not financially constrained. Because 96% of the private firms in our matched sample come from this quartile, it is unlikely that our results are driven by differences in financial constraints.

25 This test limits private firms to C Corps, as other legal forms are not subject to corporate taxes in most states. The Internet Appendix exploits this fact as an indirect validity test of our tax identification strategy. If corporate tax changes capture changes in firms' investment opportunities rather than, say, reflecting changes in a state's economic environment, they should have no effect on the investment behavior of non-C Corps. This is precisely what we find; see Column 4 in Table IA.1.

book leverage, cash, and industry. Among other things, this allows us to analyze the investment behavior of public and private firms with very similar performance: ROA averages 8.7% for public firms and 7.5% for private firms. Column 8 shows that our conclusions remain unchanged.

An alternative way to address the concern that we have matched private firms to public firms that are underperformers or otherwise unrepresentative is to perform the match while the public firms are still private. Column 9 matches public firms two years before their IPO to up to five private firms in the same NAICS2 industry that will remain private and are closest in size. Column 10 matches on a propensity score based on size, ROA, book leverage, and cash, again within industry. (For details of these matches, see Appendix A.) We then compare the investment behavior of the public firms after their IPO to that of their still private matches. Matching prior to the IPO leaves our conclusions unchanged: the difference in investment sensitivity between public and private firms is 13.9 percentage points in Column 9 ($p=0.021$) and 7.0 percentage points in Column 10 ($p=0.053$).

Sixth, Table IA.2 in the Internet Appendix shows that our results are robust to several variations of the matching algorithm (e.g., matching without replacement). The gap in investment sensitivities ranges from 6.1 percentage points to 10.9 percentage points in favor of private firms and is consistently highly statistically significant. The table also provides estimates for the full samples of public and private firms (i.e., without any attempt at matching). We report both unweighted results and results weighting each firm by the reciprocal of a propensity score based on size and industry. Angrist and Pischke (2008) suggest this weighting approach as a closely related alternative to matching on the propensity score. The unweighted results provide a lower bound for the difference in investment sensitivities between public and private firms, as the full sample of private firms contains a large number of quite small companies that invest very little. But even in this case, we still find that public-firm investment is significantly less sensitive to investment opportunities than is private-firm investment. The weighted results, where small private firms with a small chance of listing are downweighted, are in line with those we obtain when matching, thus reinforcing the notion that our conclusions are not driven by our matching choices.

3.3 Differences in firm characteristics

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. Next, we consider if it reflects differences in firm characteristics that are not a direct result of firms' listing status but could correlate with it. 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. For instance, Jovanovic and Rousseau (2014) 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 life cycle 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 life cycle 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 life cycles, whereas firms with high *RE/TA* ratios tend to be more mature. Second, we use firm age as a direct proxy for maturity.

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 ($p < 0.001$). Matching on age in addition to size and industry yields similar results, with an estimated difference in sensitivities of –0.080 ($p = 0.004$). These point estimates are similar to our baseline estimate of –0.091. In combination with the findings in Tables 2 and 3, this suggests that life cycle differences play a limited role in explaining the differences in investment behavior between public and private firms.

3.3.2 Differences in the importance of intangibles. So far, our investment sensitivity analysis has focused on investment in fixed assets. But as discussed in Section 3.1, firms also invest in intangibles. Might systematic differences in the types of assets public and private firms invest in account for the observed difference in fixed investment sensitivities? Columns 3 and 4 in Table 5 suggest not. They show that private-firm investment continues to be more sensitive to changes in investment opportunities when we include goodwill (which is captured by noncurrent assets) or advertising. If anything, the gap is slightly larger (in absolute value) than our baseline estimate of −0.091.

Another important intangible is R&D. Brown, Fazzari, and Petersen (2009) link IPOs to subsequent increases in R&D, suggesting that the stock market is an important source of funding for R&D projects. Omitting R&D from the dependent variable would 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. Although 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 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

Table 5
Economic differences between public and private firms

	Dependent variable: Investment/lagged total assets									
	Lifecycle effects		Including intangibles							
	Matched on size, industry and *RE/TA*	Matched on size, industry, and age	Gross noncurrent assets	Advertising	R&D	Only C or S Corps	Net rather than gross investment	Net noncurrent assets	Growth in total assets	Additional controls
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Investment opportunities	0.106***	0.113***	0.176***	0.133***	0.119***	0.137***	0.086***	0.128***	0.311***	0.118***
	0.019	*0.027*	*0.043*	*0.035*	*0.029*	*0.035*	*0.023*	*0.035*	*0.081*	*0.029*
...x public	−0.070***	−0.080***	−0.110**	−0.102***	−0.082***	−0.107***	−0.063***	−0.071*	−0.164**	−0.091***
	0.019	*0.027*	*0.044*	*0.036*	*0.030*	*0.036*	*0.023*	*0.023*	*0.083*	*0.029*
ROA	−0.014	0.052**	−0.009	0.096***	0.090***	0.091***	−0.032	−0.025	−0.124	0.088***
	0.012	*0.023*	*0.063*	*0.036*	*0.032*	*0.027*	*0.051*	*0.066*	*0.126*	*0.031*
...x public	0.048*	−0.024	0.000	−0.054	−0.219***	−0.053*	0.055	0.015	−0.218	−0.053
	0.026	*0.035*	*0.070*	*0.041*	*0.040*	*0.032*	*0.053*	*0.070*	*0.144*	*0.037*
Cash holdings										0.107***
										0.030
Book leverage										−0.180**
										0.076
R^2 (within)	6.2%	10.7%	7.4%	9.2%	9.1%	9.8%	7.9%	6.6%	13.3%	10.9%
F-test: All coeff. = 0	16.5***	14.8***	9.3***	7.5***	10.7***	10.6***	8.0***	9.4***	19.1***	9.8***
No. observations	51,734	41,530	22,454	22,744	22,744	21,608	22,744	22,454	22,744	22,744
No. firms	6,988	6,802	4,015	4,071	4,071	3,846	4,071	4,015	4,071	4,071

As in Table 4, 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 life cycle effects. In Column 1, we control for life cycle 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 life cycle stage (see DeAngelo, DeAngelo, and Stulz 2006). Column 2 controls for life cycle 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 impute founding dates from the NETS database, which offers comprehensive coverage of the universe of U.S. firms; see Appendix A for details. Columns 3–5 allow for a broader range of assets in which firms can invest. Column 3 models the annual change in noncurrent assets gross of depreciation (which includes both fixed assets and goodwill), whereas row 4 adds advertising expenses to gross investment in fixed assets. Column 5 includes R&D spending in the dependent variable for public firms (R&D is unavailable for private firms). Column 6 restricts the sample to incorporated firms (i.e., C and S Corps). In Column 7, we change the dependent variable from gross to net investment (i.e., the change in net fixed assets over beginning-of-year total assets). Column 8 adds to this investment in intangibles by modeling the change in noncurrent assets net of depreciation. Column 9 uses growth in total assets as the dependent variable. In Column 10, we test whether the baseline results presented in Table 4, Column 1, are robust to observed heterogeneity in cash holdings and book leverage. For variable definitions and details of their construction, see Appendix A. Heteroscedasticity-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.

investment opportunities through R&D, only marginally smaller than the point estimate of –0.091 when we omit R&D. 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. Although this is possible, there is no compelling reason to think it is likely.

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. This is in line with the results of our analysis of differences in investment levels.

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. Whereas 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 ($p=0.003$). Columns 7 and 8 model net rather than gross investment, with or without intangibles. In either case, we continue to find that private firms are more sensitive to changes in investment opportunities than are public firms. The same is true in Column 9, which models the growth in total assets.

3.4 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 the observed differences in investment behavior. For example, firms with more cash or less debt might more easily take advantage of better investment opportunities. Omitting cash holdings and leverage would then bias our results, though as Table 1 shows, the effect would likely go in the other direction: private firms actually have less cash and more debt than do 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 ($p=0.002$). In fact, the coefficient for the difference in investment sensitivity between public and private firms is indistinguishable from our baseline estimate of –0.091.

4. Short-Termism and Investment Behavior: Cross-Sectional Evidence

The evidence in Section 3 is consistent with the predictions derived in the Internet Appendix, that short-termist pressures induce public-firm managers to invest both less and in a way that is less sensitive to investment opportunities. A key hypothesis underlying this prediction is that private firms are less prone to agency problems than public ones and so are insulated from the short-termist pressures to which public firms find themselves exposed. We start this section by providing evidence consistent with this commonly assumed premise. We then report cross-sectional evidence that fits short-termism models but that is not predicted by other explanations for the observed differences in investment behavior, such as the notion that public-firm managers have a preference for a quiet life (Bertrand and Mullainathan 2003).

4.1 Is it plausible that private firms have fewer agency problems?

As Sageworks provides no ownership data, we cannot directly compare ownership structures between our public and private firms. Fortunately, Sageworks reports each firm's legal form and as a point of law, three legal forms (sole proprietors, LLCs, and partnerships) correlate strongly with concentrated ownership and little separation between ownership and control and hence little occasion for an agency problem to arise. Sole proprietors 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 are C and S Corps. These could theoretically have dispersed owners: C Corps can have an unlimited number of owners, whereas S Corps can have up to 100. (Virtually all 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 suffers from greater distortions than 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 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.

Columns 2 and 3 focus on sole proprietors, which are free of agency costs by definition. Column 2 compares these to all other private firms, whereas 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 proprietors, LLCs, and partnerships to C and S Corps, using the entire sample (Column 4) or a size-and-industry matched sample (Column 5). Each of these models tells the same story: investment sensitivities

Table 6
Investment sensitivities by legal form

	Dependent variable: Gross investment/lagged total assets				
		Sole proprietors		Sole prop. + LLC + partnership + LLP	
	All private firms	versus all other private firms	matched to similar private firms	versus all private C and S Corps	matched to similar private C and S Corps
	(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

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. Because Sageworks does not report ownership information, we use legal form as a proxy for ownership concentration instead. Sole proprietors, 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 hand. 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 proprietors which, by definition, have a single owner. In Column 2, we compare the investment behavior of sole proprietors to that of all other private firms, whereas 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 proprietors 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. Heteroscedasticity-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.

do not vary significantly within the sample of private firms, in contrast to the large difference we found between public and private firms. Because a nontrivial 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 are consistent with agency considerations, and in particular short-termism, being a key driver of the investment differences between public and private firms documented in Section 3. We can derive further predictions of how short-termism impacts the investment behavior of public firms by putting some structure on the nature of short-termism. Short-termism models typically assume 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. Because the stock price equals the present discounted value of the firm's future free cash flows, he will try to boost it by manipulating investors' expectations of future cash flows. He does so by reporting higher current cash flows, in the hope that investors will increase their expectations of future cash flows in response. The mechanism in short-termism models is not fraudulent accounting but underinvestment: in line with the survey evidence of Graham, Harvey, and Rajgopal (2005), a short-termist manager forgoes positive NPV projects, resulting in lower investment levels and lower sensitivity to changes in investment opportunities.

While investors observe actual investment, they have incomplete information about which projects the firm should invest in to maximize its long-term value, and as a result, they do not know the extent of underinvestment. But investors understand managers' incentives and so realize that high current reported cash flows will in fact lead to lower cash flows in the future. In response, they "discount" reported cash flows. Yet the short-termist manager still underinvests in equilibrium. The reason is akin to the prisoners' dilemma: if investors assumed no underinvestment, the manager would inflate current cash flows by cutting investment; given that investors will, therefore, assume underinvestment, the manager is better off forgoing positive NPV projects and thus actually underinvesting.

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26 The manager may care about the current stock price because he intends to sell some of his stockholdings (as suggested by Stein 1989 and confirmed empirically by Bhojraj et al. 2009) or because he fears losing his job in the event of a hostile takeover (Shleifer and Vishny 1990; Stein 1988). Another possibility is that some shareholders may incentivize the manager to pursue short-termist actions that increase the firm's stock price in the short term (albeit at the expense of long-run fundamental value) in the hope of selling the stock in the future to potentially overoptimistic investors (see Bolton, Scheinkman, and Xiong 2006 for such a model).

4.3 Testable implications

Short-termism models in the Stein (1989) tradition predict that the extent of a firm's investment distortion depends on the sensitivity of its share price to its current earnings per share. This parameter, which Stein calls α_0, captures the extent to which investors base their expectations of the firm's future earnings on its current earnings and thus the effect that news about current earnings has on stock prices. This yields two cross-sectional implications. First, we expect no distortion for a public firm whose current earnings are uninformative about future earnings and so have no effect on its share price ($\alpha_0 = 0$). In this case, the manager cannot manipulate investors' expectations, so there is no point investing myopically. Second, as α_0 increases, current earnings become more and more informative about future earnings, increasing the incentive to manipulate investors' expectations (and the firm's share price) by underinvesting, leading to greater distortions.[27] 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 proxy for α_0 using earnings response coefficients estimated at the industry level (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. We take this sensitivity as being outside the firm's control and ask how it affects a firm's response to changes in investment opportunities. If short-termism drives the gap in investment sensitivity between public and private firms, the level of α_0 will affect public and private firms asymmetrically: the larger α_0, the greater a short-termist manager's incentive to manipulate investors' expectations and thus the greater the difference in investment sensitivity between public and private firms. This is true even if ERC, our empirical proxy, picks up factors besides α_0 (say, growth opportunities or firm maturity), as both public and private firms' investment behavior should respond to such factors. A differential response to ERC, on the other hand, is a unique prediction of short-termism models.

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 or 48 industry groups. We then include a full set of interaction terms involving lagged industry ERCs

27 While CAPEX is not expensed, it does depress earnings by increasing depreciation. Graham, Harvey, and Rajgopal (2005) report survey evidence that managers avoid missing earnings-per-share (EPS) targets by cutting CAPEX to reduce "depreciation charges to earnings." Almeida, Fos, and Kronlund (2013) emphasize that, in addition to increasing the numerator in EPS by reducing depreciation, cutting CAPEX frees up cash for share repurchases, which can reduce the denominator in EPS.

in our matched-sample investment sensitivity regression. This "triple-diff" approach controls for any effects of ERC on investment that are common to public and private firms, thus allowing us to identify the effect that ERC has on the difference in investment sensitivity between public and private firms.

Table 7 reports the results. In row 1, using Fama-French thirty 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), whereas 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 sensitivities between public and private firms is small (0.021) and not statistically significant (p=0.676), consistent with the predicted absence of distortions for $\alpha_0 = 0$. As ERC increases, public firms' investment sensitivity falls significantly (p=0.034), whereas that of private firms is unchanged (p=0.812). In other words, as illustrated in Figure 9, the difference in investment sensitivities between public and private firms increases in ERC, and this increase is driven by a change in the behavior of public firms.[28] Row 2 shows similar results for Fama-French forty-eight industries.

4.5 Interpretation and auxiliary evidence

Table 7 is consistent with the notion that short-termism plays a key role in explaining the observed differences in investment behavior. Our findings that public and private firms exhibit similar investment sensitivities when $\alpha_0 = 0$ and that the difference in sensitivities increases in α_0 fit models of short-termism but, as far as we know, are not predicted by other hypotheses that may theoretically explain the observed differences in investment behavior, such as a preference for a quiet life.[29]

Figures 10 and 11 provide auxiliary evidence consistent with the notion that short-termist pressures are an important driver of the low investment sensitivity of public firms. Figure 10 examines how the investment sensitivity of public firms varies with ownership by "transient" institutional investors. Bushee (2001) shows that transient investors have a strong preference for short-term earnings. As a result, we expect short-termist pressures to be higher, and

[28] 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.

[29] Another agency problem that might distort the investment decisions of public firms is empire building. However, if this were a prevalent distortion among public firms, they would invest more than private ones, the opposite of what we find in Tables 2 and 3. An alternative hypothesis that might a priori explain why public and private firms invest differently is the possibility that private firms are financially constrained. However, recall from Section 3.2.2 that we found no support for the notion that private firms' higher investment and investment sensitivity are the result of financial constraints. Other channels that may explain the observed differences in investment behavior between public and private firms include the idea that public firms are weighed down by rules and regulations designed to protect minority shareholders or that private firms are capital inefficient or inexperienced at making investment decisions. None of these alternative channels seems to predict our Table 7 findings.

Table 7
Cross-industry variation in short-termism

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*		

Short-termism models predict that the difference in investment sensitivities between public and private firms is zero for ERC = 0 and then increases in ERC, where ERC measures the sensitivity of a public firm's stock price to earnings news. To test these predictions, we include a full set of interaction terms involving ERC in our baseline investment sensitivity equation from Table 4. Our definition of ERC follows the accounting literature; see Appendix A for details. 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 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.

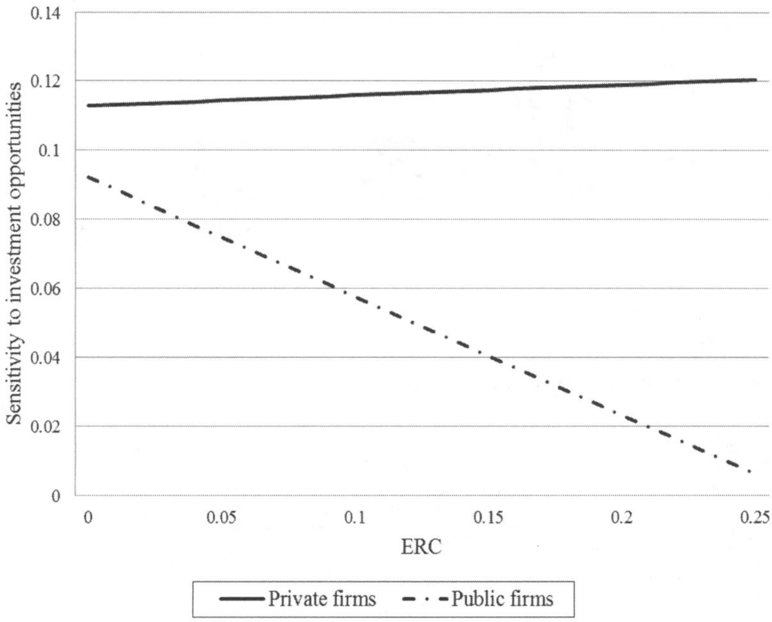

Figure 9
Cross-industry variation in short-termism
Short-termism models predict that the difference in investment sensitivities between public and private firms is zero for $\alpha_0 = 0$ and then increases in α_0, where α_0 measures how sensitive a public firm's stock price is to its current cash flows. This figure uses the estimates reported in Table 7 to illustrate how the difference in investment sensitivities between public and private firms changes with α_0. As detailed in Table 7, we follow the accounting literature and use the earnings response coefficient (ERC) in the firm's industry to capture a firm's stock price sensitivity.

thus investment sensitivity to be lower, among public firms that are owned by more transient shareholders. Figure 10 shows that investment sensitivity is indeed significantly lower the more transient owners a public firm has ($p = 0.056$).

Figure 11 examines whether low investment sensitivity among public firms stems from a desire to avoid negative earnings surprises. Bartov, Givoloy, and Hayn (2002) argue that habitually meeting or beating analysts' earnings forecasts signals that firms manage their reported earnings to avoid negative earnings surprises. Figure 11 shows that public firms exhibit considerably lower investment sensitivities the more prone they are to meet or beat forecasts. This is consistent with firms passing up potentially profitable investment projects in an effort to manage their earnings in line with analysts' forecasts.

Our evidence is thus consistent with the predictions of short-termism models, suggesting that myopic incentives play a first-order role in explaining the observed differences in investment behavior. That said, we caution against interpreting our findings as evidence that short-termism is the only driver of

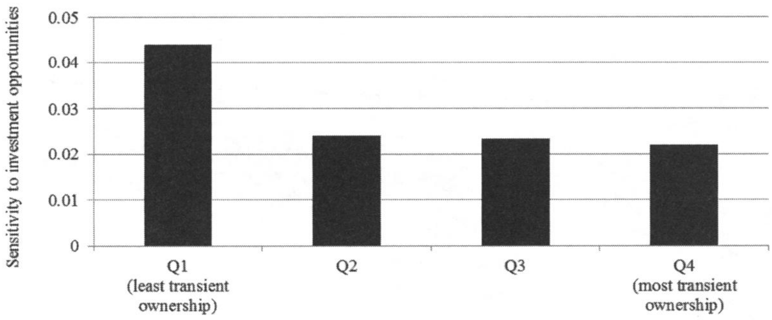

Figure 10
Variation in transient institutional ownership among public firms
The figure shows estimates of the sensitivity of investment to investment opportunities for all public firms in the full sample sorted into quartiles according to the fraction of their shares that is owned by transient institutional shareholders at the beginning of the fiscal year. Data on institutional ownership come from Thomson-Reuters' Institutional Holdings (13F) Database. We use Brian Bushee's classification of institutional owners according to their investment horizon (transient, quasi-indexer, and dedicated), available on his website. Bushee (2001) shows that high levels of ownership by transient institutions are associated with significant over- (under-) weighting of the near-term (long-term) earnings component of firm value. The figure plots the coefficient on investment opportunities obtained from estimating the Table 4, Column 4 regression separately for each transient institutional ownership quartile.

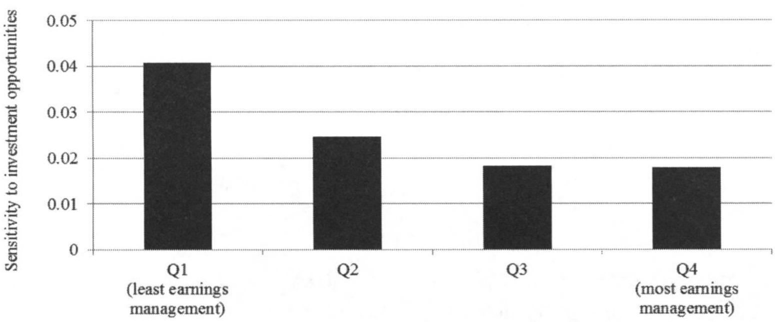

Figure 11
Variation in earnings management among public firms
The figure shows estimates of the sensitivity of investment to investment opportunities for public firms sorted into quartiles according to their propensity to meet-or-beat earnings forecasts, which prior work indicates signals the presence of earnings management (Bartov, Givoloy, and Hayn 2002). Specifically, we assign each public firm-year in the full sample to one of four quartiles based on the fraction of the previous 20 quarters in which the firm met or exceeded analysts' earnings expectations. (For 45% of the observations, we do not have enough of a history of earnings forecasts to perform this classification.) The figure plots the coefficient on investment opportunities obtained from estimating the Table 4, Column 4 regression separately for each earnings management quartile.

our findings: other differences between public and private firms may play an incremental role in explaining why public firms invest less and in a way that is less sensitive to investment opportunities compared with matched private firms.

5. Endogeneity of a Firm's Listing Status

While our results are consistent with the notion that public firms' investment decisions are affected by short-termist pressures, the empirical strategy used in Sections 3 and 4 cannot establish whether these pressures are a result of a firm's listing status or whether they reflect potentially unobservable differences between the types of firms that tend to go public and those that tend to remain private. For example, a private firm whose manager is more prone to succumb to short-termist pressures might also be more likely to go public. We address this concern in two ways. First, we show that our results are robust to exploiting within-firm variation in listing status. Second, we use an instrumental variable approach.

The results suggest that a firm's listing status does impact its investment behavior. That said, we recognize that the empirical strategies used in this section are imperfect substitutes for what would, ideally, be a trial that varies firms' listing status randomly.

5.1 Within-firm variation in listing status

Following Michaely and Roberts (2012), we consider an alternative sample: firms that go public. If the observed difference in investment behavior is indeed the result of a change in managers' investment horizons, we should see behavior changing as a firm transitions from private (i.e., concentrated) to public (i.e., dispersed) ownership. This set-up differs from our previous tests in that it examines how a given firm's behavior changes as it transitions from private to public status.

Of course, most firms go public precisely 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 an increase in 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. That said, the concern remains that an IPO is not a natural experiment, so we stop short of claiming causality.

Our IPO dataset consists of the 90 nonfinancial and nonutility firms that went public between 1990 and 2007 for the sole purpose of allowing existing shareholders to cash out, as opposed to raising equity to fund operations or investment plans or to repay debt. (See Table IA.3 in the Internet Appendix for the firms' names, dates, and circumstances.) We collect five years of post-IPO data from Compustat and hand-collect up to five years of pre-IPO data from IPO prospectuses and 10-K filings available in the SEC-Edgar and Thomson Research databases. Because this sample does not involve Sageworks data, we can collect data on R&D as well as on CAPEX from the income and cash flow statements.

To capture possible changes in investment levels, Columns 1 and 3 of Table 8, panel A, include an indicator variable set equal to one in the years following a firm's IPO. To capture possible changes in investment sensitivities, Columns 2 and 4 interact this indicator with investment opportunities. Whether we measure investment as CAPEX (Columns 1 and 2) or the sum of CAPEX and R&D (Columns 3 and 4), we find that both a firm's investment level and its sensitivity to investment opportunities significantly decline after it goes public. Thus, firms appear to alter their investment behavior once they are public, even though their IPOs are of the kind that only affects their ownership structure.

Investment behavior could change for macroeconomic reasons and not because of the IPO itself. To control for this, Columns 5 through 8 combine data from the IPO sample with data for already-public firms matched on size, age, and industry. While we cannot rule out that the two groups of firms may differ in various unobserved ways, the results continue to tell the same story: both investment levels and investment sensitivities decline significantly after the IPO. In fact, before they go public, IPO firms have significantly greater investment sensitivities than do matched already-public firms; once they are public, however, their sensitivities are not only significantly lower than before but become indistinguishable from those of public firms. This suggests that the observed differences in investment behavior we saw in our baseline specifications are unlikely to be driven by time-invariant differences between public and private firms.

5.2 Instrumental variable analysis

Our findings leave open the possibility that unobservable differences between public and private firms might simultaneously affect the decision to go public or stay private and firms' investment behavior. To alleviate this concern at least partially (eliminating it is impossible), we complement our matching and IPO tests by following an instrumental variable (IV) approach.

The ideal instrument provides exogenous variation in a firm's listing status without directly affecting investment. Our IV exploits differences in the supply of start-up funding across U.S. states and time. We define VC supply as the total number of firms receiving first-round venture capital funding in a firm's headquarter state two years after the firm was founded, scaled by the number of firms in the state that were less than three years old at the time.[30] VC funding data are available from 1973, so we exclude all firms founded before 1971 from the IV analysis. We also exclude firms founded after 1999 to ensure that VC supply is measured before 2002, the start of our sample period.

The motivation for our IV rests on two facts: VCs tend to invest locally (Lerner 1995) and, as Ozmel, Robinson, and Stuart (2013) note, "it is well established that firms with more venture funding are at greater risk of going

[30] According to Chemmanur, Krishnan, and Nandy (2011), the median VC-backed firm is around two years old when it first receives VC funding.

Table 8
Changes in investment behavior around IPOs

Panel A: Least squares estimates

	Dependent variable: Investment/lagged total assets							
	Own difference				Diff-in-diff with matched controls			
	investment w/o R&D		investment w/ R&D		investment w/o R&D		investment w/ R&D	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Investment opportunities	0.052***	0.081***	0.080***	0.121***	0.026***	0.017***	0.043***	0.032***
	0.018	*0.027*	*0.022*	*0.034*	*0.007*	*0.006*	*0.009*	*0.008*
Investment opp. x pre-IPO						0.064**		0.090***
						0.027		*0.035*
Investment opp. x post-IPO		−0.063**		−0.085**		0.001		0.004
		0.032		*0.042*		*0.019*		*0.026*
ROA	0.080	0.050	0.120*	0.090	0.140***	0.171***	0.170***	0.195***
	0.056	*0.065*	*0.064*	*0.076*	*0.025*	*0.014*	*0.029*	*0.020*
ROA x pre-IPO						−0.121*		−0.105
						0.066		*0.079*
ROA x post-IPO		0.099*		0.101		−0.022		−0.004
		0.056		*0.066*		*0.036*		*0.043*
Post-IPO	−0.009**	−0.019**	−0.015***	−0.022*	−0.009**	−0.019**	−0.016***	−0.022*
	0.004	*0.009*	*0.005*	*0.012*	*0.004*	*0.009*	*0.005*	*0.012*
R^2 (within)	8.8%	10.4%	11.8%	13.1%	9.3%	10.2%	10.9%	11.5%
F-test: All coefficients = 0	13.4***	13.2***	14.4***	15.2***	57.4***	45.7***	51.9***	33.6***
No. observations	963	963	963	963	4,525	4,525	4,525	4,525
No. firms	90	90	90	90	420	420	420	420

In this table, we estimate within-firm changes in investment behavior around the IPOs of firms that go public for the sole purpose of allowing some of their existing shareholders to cash out. Table IA.3 in the Internet Appendix 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. Each regression is estimated using least squares with firm fixed effects. Columns 1, 3, 5, and 7 show how investment levels change around the IPO, whereas Columns 2, 4, 6, and 8 focus on changes in the sensitivity of investment to investment opportunities. Columns 1 through 4 report own-difference results for the IPO sample. Specifically, Columns 1 and 3 include a post-IPO indicator set equal to one in the years following a firm's IPO; Columns 2 and 4 interact investment opportunities and ROA with the post-IPO indicator. Columns 5 through 8 combine data from the IPO sample with data from a matched control sample of already-public firms. To be eligible for matching, a public firm must be in both Compustat and CRSP, be incorporated in the United States and listed on the NYSE, AMEX, or Nasdaq exchanges, have valid stock price data in CRSP, and have a CRSP share code of 10 or 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. Columns 6 and 8 interact investment opportunities and ROA with separate indicators for pre- and post-IPO. The 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 sensitivities that are significantly different from those of their matched controls either before or after going public, respectively. As in Columns 5 and 7, we 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 in Columns 5 through 8.) For variable definitions and details of their construction, see Appendix A. Heteroscedasticity-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.

Table 8
(continued)

Panel B: Instrumental variable analysis

Dependent variable:	Public firm?	Gross investment/lagged assets			
	IV first stage (probit)	IV second stage		OLS	
	(1)	(2)	(3)	(4)	(5)
Investment opportunities	−0.059*	0.064***	0.163***	0.064***	0.130***
	0.032	*0.012*	*0.043*	*0.012*	*0.035*
... x public			−0.146***		−0.092***
			0.055		*0.036*
ROA	−0.120	0.038***	0.072***	0.038***	0.072***
	0.093	*0.007*	*0.021*	*0.007*	*0.016*
...x public			0.047		−0.057*
			0.137		*0.034*
Public		−0.038*	0.027	−0.025**	−0.001
		0.021	*0.045*	*0.010*	*0.010*
Instrument: VC supply at founding	9.774***				
	1.364				
F-test: VC supply at founding = 0	51.4***				
R^2	13.6%	19.1%	18.7%	19.2%	21.5%
F-test: all coefficients = 0	113.6**	45.9***	99.0***	5.6***	5.5***
No. observations	10,188	10,188	10,188	10,188	10,188
No. firms	1,983	1,983	1,983	1,983	1,983

This table uses an instrumental variable (IV) approach to examine whether the observed differences in investment behavior between public and private firms reflect unobserved differences that endogenously determine listing status. Our instrument for listing status exploits differences in the supply of venture capital funding across U.S. states and time. Specifically, we define VC supply as the total number of firms receiving first-round VC funding in a firm's headquarter state two years after the firm was founded, scaled by the number of firms in the state that were less than three years old at the time. (Appendix A provides further details on the construction of this and all other variables included in the analysis.) Before matching, we exclude all firms founded before 1971 from the IV sample, so that VC supply is first measured in 1973, the year that the National Venture Capital Association was created to represent the interests of the fledgling VC industry. We also exclude firms founded after 1999 to ensure that VC supply is measured before 2002, the start of our sample period. We then match public and private firms founded between 1971 and 1999 on size and industry. Column 1 shows the first-stage results from a probit model of a firm's listing status on the VC supply at the time of founding, sales growth, and ROA (the control variables used in our baseline investment equations (1) and (2)), as well as industry (NAICS4) and year fixed effects. Column 2 and 3 show the second-stage results corresponding to estimating investment Equations (1) (levels) and (2) (sensitivities). Following Wooldridge (2010), 939), the second stage is estimated by GMM using as instruments the first-stage predicted probabilities as well as their interactions with sales growth and ROA. As VC supply does not vary within-firm, Column 3 includes industry (NAICS4) fixed effects (instead of firm fixed effects) alongside a public-firm indicator. For comparison, Columns 4 and 5 report the results of estimating the specifications in Columns 2 and 3 without instrumenting a firm's listing status. Column 4 differs from our baseline investment level specification in Table 3 in that it limits the sample to firms founded between 1971 and 1999. Column 5 differs from our baseline investment sensitivity specification in Table 4 in that it includes industry rather than firm fixed effects and limits the sample to firms founded between 1971 and 1999. Heteroscedasticity-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 firm-level variables are winsorized 0.5% in each tail to reduce the impact of outliers.

public." As a result, we expect firms in states with greater supply of VC funding at the time of their founding to be more likely to obtain VC funding and thus to go public subsequently. The first-stage results, shown in Table 8, panel B, confirm this prediction: VC supply at founding is a positive and strongly significant predictor of a firm's listing status many years later, with an F-statistic of 51.4.

The exclusion restriction is that conditional on covariates, the supply of VC funding in a firm's home state two years after it was founded only affects the firm's investment behavior for our 2002–2011 sample period through its impact on the firm's listing status. The chief concern is that neither firms' nor VCs' location choices are random.

Although it is possible that firms keen to obtain VC funding choose to locate in areas with ample supply of VC funding, this would only violate the exclusion restriction if a firm's preference for VC funding early in its life cycle also correlated with its investment decisions much later in its life. The fact that the median firm in our IV sample is twenty-two years old helps alleviate this concern: the motives that drove a firm's location decision two decades earlier should have little effect on a firm's investment decisions today.

Similarly, VCs likely prefer to operate in states with better investment opportunities, and these in turn likely affect the investment behavior of firms located in those states. Our instrument deals with this by exploiting time variation in VC supply:[31] even if VC supply does correlate with a state's investment opportunities, these opportunities are likely to have changed from when a firm was founded to when we measure its investment. Our approach is thus similar to that of Acemoglu, Johnson, and Robinson (2009) and Glaeser, Kerr, and Kerr (Forthcoming), who like us rely on separation in time to motivate the exclusion restriction. That said, we recognize that persistence in state-level investment opportunities would violate the exclusion restriction. As exclusion restrictions are inherently untestable, the usual caveat applies.

Columns 2 and 3 of Table 8, panel B, show the second-stage results for the investment-level Equation (1) and the investment sensitivity Equation (2), respectively.[32] After instrumenting a firm's listing status, public firms continue to invest less than matched private firms, with an average difference of 3.8 percentage points ($p = 0.073$), and they do so in way that continues to be less sensitive to investment opportunities, with an average difference of –0.146 ($p = 0.008$).[33] A comparison to the equivalent naïve OLS estimates, reported in Columns 4 and 5, shows that these point estimates are somewhat larger (in absolute magnitude) with than without instrumenting listing status.

Taken together, these results suggest that the endogeneity of a firm's listing status is unlikely to be a first-order driver of the observed differences in investment behavior between public and private firms.

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31 VC supply varies considerably over time: the correlation between a state's VC supply in 1985 (the year when VC supply is measured for the median firm in our sample) and 2002 (our first panel year) is only 0.24.

32 Because VC supply does not vary within-firm, Equation (2) is estimated with industry fixed effects instead of firm fixed effects.

33 Note that owing to the presence of interaction effects in Column 3, the public-firm indicator is not informative: it captures the mean difference in investment levels for public and private firms with zero sales growth and zero ROA.

6. Conclusions

We investigate whether short-termism distorts the investment decisions of public firms. To do so, we compare the investment behavior of public and private firms, matched primarily on size and industry. Building on prior work, our key identification assumption is that, on average, private firms are subject to fewer short-termist pressures than are their listed counterparts. The data support this assumption.

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 most sensitive to earnings news. These findings are consistent with the notion that short-termist pressures increase the hurdle rate that public-firm managers use to evaluate investment projects, resulting in lower investment levels and a lower sensitivity to changes in investment opportunities. Remarkably, our findings hold even during the recent financial crisis, when private firms presumably became (even) more financially constrained compared to public firms.

Our findings highlight short-termist pressures as a potentially important cost of a stock market listing. A number of authors have emphasized other costs and benefits of being public, such as costs associated with disclosure requirements or benefits due to a reduction in financing costs, greater scope for risk sharing, and the opportunity to attract better qualified human capital. Our paper thus can be seen as part of a larger research agenda that tries to understand the trade-offs associated with the going-public decision against the background of companies' waning interest in a stock market listing in the United States, where the number of listed firms has more than halved since 1997.

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 change in gross fixed assets—i.e., gross property, plant, and equipment—(Compustat data item *ppegt* or its Sageworks equivalent, *GrossFixedAssets*) scaled by beginning-of-year nominal total assets.

Gross investment in noncurrent assets (*NCA*) is the change in total assets (Compustat data item *at* or its Sageworks equivalent, *TotalAssets*) minus the change in current assets (Compustat data item *act* or its Sageworks equivalent, *TotalCurrentAssets*) minus the change in net fixed assets (Compustat item *ppent* or its Sageworks equivalent, *NetFixedAssets*) plus the change in gross fixed assets (Compustat data item *ppegt* or its Sageworks equivalent, *GrossFixedAssets*), all scaled by beginning-of-year nominal total assets.

Gross investment + advertising is the change in gross fixed assets plus advertising expenses (Compustat data item *xad* or its Sageworks equivalent, *Advertising*) scaled by beginning-of-year nominal total assets.

Net investment is the annual change in net fixed assets—i.e., net property, plant, and equipment—(Compustat item *ppent* or its Sageworks equivalent, *NetFixedAssets*) scaled by beginning-of-year nominal total assets.

Net investment in noncurrent assets (*NCA*) is the change in total assets (Compustat data item *at* or its Sageworks equivalent, *TotalAssets*) minus the change in current assets (Compustat data item *act* or its Sageworks equivalent, *TotalCurrentAssets*) scaled by beginning-of-year nominal total assets.

Growth in total assets (*TA*) is the change in total assets 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: $Sales_{it}/Sales_{it-1}-1$ (using Compustat item *sale* or its Sageworks equivalent, *Sales*).

Predicted q is computed as follows. Following Campello and Graham (2013), we regress each public firm's Tobin'sq(Compustat items $prcc\_f \times cshpri + pstkl + dltt + dlc - txditc$ divided by beginning-of-year total assets, *at*) on the firm's sales growth, return on assets (ROA, defined as operating income before depreciation scaled by beginning-of-year total assets), net income before extraordinary items, book leverage, and year and industry fixed effects (using three-digit NAICS industries). We then use the regression coefficients to generate predicted $q$for each firm, both public and private.

Industry q is the lagged size-weighted mean of Tobin'sq(Compustat items $prcc\_f \times cshpri + pstkl + dltt + dlc - txditc$ 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 $tax\ rate_t - tax\ rate_{t-1}$, 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 Appendixes A and B of Heider and Ljungqvist (Forthcoming). 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.

Dividends/TA is dividends (Compustat item *dvc+dvp* or its Sageworks equivalent, *Dividends*), scaled by total assets.

Age is the number of 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 United States. 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 matches to cases where there is a unique Sageworks firm and a unique NETS firm in a given ZIP code and NAICS5 industry. We next predict the age of those Sageworks firms without a unique match to NETS as follows. We regress the age of those Sageworks firms with a unique match to NETS on a full set of three-digit NAICS industry dummies, size, and size interacted with the industry dummies and use the estimated coefficients to predict the age of all those Sageworks firms without a unique match to NETS.

Pre-IPO matching in Columns 9 and 10 of Table 4 involves matching public and private firms, whereas the public firms are still private. Specifically, we match public firms two years before they go public to private firms in the Sageworks database. Given the two-year horizon, we focus on IPOs that took place in or after 2003. This ensures that matching first occurs in 2001, the first year for which we have Sageworks data. To ensure that we can follow each firm for at least five years before the 2011 end of our panel, we exclude IPOs that took place after 2007. We identify 310 IPOs between 2003 and 2007 that can be matched to a Sageworks firm and that pass our data filters. IPO dates are obtained from Jay Ritter's Web site (http://bear.warrington.ufl.edu/ritter/ipodata.htm), from Compustat (Compustat variable *ipodate*), and from Thomson Reuters' SDC database.

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 ten 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 Web page. 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.)

VC supply at founding is the number of firms receiving first-round VC funding in a sample firm's headquarter state two years after the firm was founded, scaled by the number of firms in the state that were less than three years old at the time. Firms founded before 1973 are excluded from the IV analysis so that VC supply is only measured for firms founded in or after 1973, the year that the National Venture Capital Association was created. Data on VC funding come from VentureXpert. State-level data on the number of firms less than three years old come from the Longitudinal Business Database and are made available by the Census Bureau on its Web site. We measure VC supply in the first headquarter state reported in Compustat or Sageworks. We use Heider and Ljungqvist's (Forthcoming) headquarter state data to correct for the fact that Compustat reports a firm's current (as opposed to historic) headquarter state.

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