No Rents from Uncle Sam: Evidence from Private Equity*

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October 2026

Abstract

The US government spends $750 billion a year on contracts, and concerns that much of it is wasted are widespread. A common view is that contractors earn rents at taxpayers' expense. To test whether they do, we study buyouts of federal contractors by private equity sponsors, the owners most likely to find and exploit such rents. We find that targets do not expand their government business: contracting declines after the deal for both targets and comparable contractors, slightly more for targets, and the difference is concentrated among small-business contractors. After the deal, the targets' existing contracts receive funding increases and time extensions no more often than comparable contracts, the targets of politically connected sponsors fare no better, and contractor deals earn no excess returns. Small-business contractor targets, which scale down their government business most, grow at least as fast as other buyouts, suggesting that the retreat does not hurt them. Overall, we find no evidence that contractors earn rents on government work, so savings are unlikely to come from squeezing contractor margins.

Keywords: Government contracting, Public procurement, Private equity, Small business, Government waste

JEL Codes: G23, G24, G34, G38, H57

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"[Fraud is] about 10% of the federal budget, 1 to 2% of GDP. . . . If we can get rid of this fraud, waste, and abuse, we can finance a safer, sounder U.S. without taking on more debt."

–Scott Bessent, U.S. Treasury Secretary, interview with Christopher Rufo, January 2026

1. Introduction

The US federal government spends about $750 billion a year on contracts, which makes it the largest single customer in the economy. A long-standing concern is that much of this spending is wasted, and recent efforts to cut federal spending have targeted contracts on the premise that the government overpays the firms that sell to it. The success of such efforts depends on whether these firms earn excess profits on government work. If they do, the government can save money by squeezing their margins and policing favoritism. If they earn competitive margins, squeezing them would drive firms away rather than save money. Answering this question is difficult, because contractor profits are rarely observed: most federal contractors are private firms that do not disclose their financial statements, and even for public firms, the profits on government work cannot be separated from those on the rest of the business.

Two opposing views describe what firms gain from selling to the government. Under the first, which we call the rent-extraction view, procurement allows well-positioned firms to earn rents. Contracting officers enjoy discretion, competition is often limited, and existing contracts can be renegotiated, so that firms with political access or privileged positions can win contracts and modify them on favorable terms (Goldman, Rocholl, and So, 2013; Boland and Godsell, 2021; Brogaard, Denes, and Duchin, 2021). Under the second, which we call the springboard view, the government is a demanding buyer that keeps margins on its contracts thin (Bajari, Houghton, and Tadelis, 2014; Kang and Miller, 2022), but government work benefits firms in other ways: it provides stable demand, access to finance, and a record of reliable performance that helps firms win other business and grow (Ferraz, Finan, and Szerman, 2015; Goldman, 2020; Hebous and Zimmermann, 2021; Hvide and Meling, 2023). Small-business programs, which reserve a share of federal contracts for small firms, are designed precisely to provide such a springboard.

In this paper, we distinguish the two views by studying what private equity sponsors do with the government business of the federal contractors they acquire. Private equity sponsors are well suited for this test, because if government contracts offered rents, they are the owners most likely to find and exploit them. First, sponsors have strong incentives to raise profits: their compensation

depends on the returns of their deals, they finance their acquisitions with debt, and they must exit their investments within a few years (Jensen, 1989; Kaplan and Strömberg, 2009). Second, a buyout gives the sponsor control of the firm, so it can change the firm's strategy, and sponsors bring the capital and expertise to expand a business when it pays. Third, and most important, sponsors have a record of exploiting government-funded revenue opportunities in other settings, such as for-profit colleges and nursing homes, and the targets of politically connected sponsors create more jobs in election years (Eaton, Howell, and Yannelis, 2020; Gupta, Howell, Yannelis, and Gupta, 2024; Faccio and Hsu, 2017). Finally, unlike the profits of most contractors, the returns that sponsors earn on their deals are sometimes observed. Thus, if government contracts left rents with the firms that sell to the government, sponsors would exploit them. We find no evidence that they do: sponsors neither expand their targets' government business nor earn excess returns on contractor deals.

The two views make different predictions about what sponsors should do with the firms they acquire. If government procurement created opportunities for rent extraction, sponsors would expand their targets' government business, renegotiate their existing contracts on more favorable terms, particularly if they are politically connected, and earn higher returns on contractor deals. By contrast, if government work offers no excess margins, as the springboard view holds, sponsors have no reason to expand it, and contractor deals should earn ordinary returns. The targets that benefited most from government work, such as small firms, should rely on it less once a sponsor provides capital and management. Because their record as reliable government suppliers helps them win other business, this retreat should not hurt them.

To test these predictions, we combine federal procurement records from USAspending.gov for fiscal years 2003 to 2025 with PitchBook data on about 3,600 buyouts of established federal contractors completed between 2008 and 2023. Most targets are private mid-market firms, which make up the bulk of federal contractors; the largest defense contractors are publicly listed and rarely the targets of buyouts. We begin by documenting a feature of federal contracting that creates a measurement problem: firms win new contracts for several consecutive years and then stop, and most of these contracting spells end within a few years. Because sponsors tend to buy contractors during active spells, their targets' government business would decline after the deal even if the new owner changed nothing. As a result, comparing targets with all other contractors attributes the ordinary end of contracting spells to the buyout. When we instead compare targets with similar contractors at the same point in their contracting spells, most of the apparent effect of the buyout disappears.

Using this comparison, we obtain four main results. First, private equity sponsors do not expand the government business of the contractors they buy. Sponsors seek out active contractors with long records of winning government contracts. After the deal, however, they do not use their new ownership to grow these firms' government work. As their contracting spells end, the government business of both targets and comparable contractors declines, with targets scaling it down slightly more. This difference is concentrated among small-business contractors. Moreover, politically connected sponsors are no more likely to buy contractors and, once they do, are no more likely to expand their government business.

Second, small-business contractors, the firms with the most favorable access to government work, scale down their government business more than comparable contractors and are more likely than other targets to stop selling to the government altogether. In effect, they graduate from protected work once a sponsor provides capital and management. This is not driven by a loss of eligibility: few targets lose their small-business status, and most continue to certify themselves as small. Consistent with the springboard view, targets are also more likely to leave fixed-price contracts that attract many bidders, where margins are likely to be thinnest, but less likely to leave work awarded without competition.

Third, sponsors do not obtain more favorable terms on the contracts their targets already hold. After the deal, these contracts receive time extensions and funding increases no more often than those of comparable contractors, and in some specifications less often. The same holds for politically connected sponsors, which would be best placed to secure such modifications. We thus find no evidence of the renegotiation channel through which connected firms earn rents in other settings (Brogaard, Denes, and Duchin, 2021).

Fourth, scaling down government business does not hurt the targets or their sponsors. Using deal-level data from Burgiss and StepStone, which cover a subset of our deals, we find that small-business contractor targets, whose government business declines most relative to comparable contractors, grow at least as fast as non-contractor buyouts by the same sponsor, and in the Burgiss data their profits and enterprise value grow faster. Contractor deals also earn the same returns as comparable buyouts, so sponsors do not earn less when their targets scale down government work. This is what the springboard view predicts: once a sponsor provides capital and management, these firms can rely less on government work without harm to the business.

A natural question is whether sponsors could expand their targets' government business if they wanted to. Several results suggest that they could. Agencies do not appear to shut out investor-

backed firms, and the retreat from government work appears only when an owner takes control: firms backed by growth equity investors, which provide capital without taking control, scale down their government business less than comparable contractors. Nor do sponsors appear to avoid government work as such, for example because of its compliance and disclosure requirements or the cost risk of fixed-price contracts. Instead, as our second result shows, they scale it back selectively. Finally, sponsors appear to learn from experience. Sponsors without earlier contractor deals scale down their targets' government business less than comparable contractors, whereas experienced sponsors scale it down more, and sponsors whose earlier targets maintained their government business earn lower returns on their contractor deals than on their other deals. These results suggest that the absence of expansion reflects an active choice.

Taken together, our results provide no evidence that federal contracting offers rents to the mid-market firms that private equity acquires. Sponsors, the owners most likely to exploit such rents, neither expand their targets' government business nor earn excess returns on contractor deals. Instead, our evidence is consistent with the springboard view: government work helps firms establish themselves as reliable suppliers and grow, and it matters less once a sponsor provides capital and management. These findings also bear on the debate about waste in government spending, suggesting that the savings from reforming procurement are unlikely to come from squeezing contractor margins.

Our paper contributes to several strands of the literature. First, we contribute to the literature on waste in government procurement. Bandiera, Prat, and Valletti (2009) compare the prices that Italian public bodies pay for the same goods and conclude that most of the excess reflects inefficiency rather than benefits to the officials who make the purchases. Liebman and Mahoney (2017) show that expiring budgets lead US agencies to spend on lower-quality projects at the end of the fiscal year, and Decarolis, Giuffrida, Iossa, Mollisi, and Spagnolo (2020) show that the competence of procurement officers affects contract outcomes. Kang and Miller (2022) and Bajari, Houghton, and Tadelis (2014) estimate structural models in which the government captures most of the surplus and renegotiation is costly for contractors. These studies measure waste from the buyer's side, through the prices the government pays and the outcomes it obtains. We take the seller's side. Using the decisions and realized returns of the owners with the strongest incentives to find rents, we ask directly whether contractors earn excess profits on government work, and we find no evidence that they do.

Second, we contribute to the literature on rent extraction in procurement. This literature shows

that political connections can matter in specific settings. Goldman, Rocholl, and So (2013) find that S&P 500 firms connected to the winning party receive more procurement dollars after a change in control of Congress. Boland and Godsell (2021) find that lobbying firms win larger non-competed contracts at the end of the quarter, when officials have the most discretion. Brogaard, Denes, and Duchin (2021) find that connected public firms obtain more favorable renegotiations of their contracts. Building on this evidence, we ask a broader question: whether government work is a source of rents for the private firms that make up most federal contractors.

Third, we contribute to the literature on private equity and government revenue. Prior work shows that private equity owners draw more revenue from government programs in the businesses they acquire, such as for-profit colleges and nursing homes (Eaton, Howell, and Yannelis, 2020; Gupta et al., 2024). This evidence motivates our focus on private equity: if sponsors do not find rents in procurement, few owners would. We extend this literature to federal procurement and show that sponsors do not expand their targets' government business and earn returns similar to those of comparable non-contractor buyouts. More broadly, we add to the literature on how private equity changes the strategy of the firms it acquires (Kaplan, 1989; Davis, Haltiwanger, Handley, Jarmin, Lerner, and Miranda, 2014; Bernstein and Sheen, 2016; Fracassi, Previtero, and Sheen, 2022).

Fourth, we contribute to the literature on the role of government demand and small-business programs. Government contracts provide constrained firms with stability, finance, and opportunities to learn and grow (Ferraz, Finan, and Szerman, 2015; Goldman, 2020; Hebous and Zimmermann, 2021; Hvide and Meling, 2023; di Giovanni et al., 2025), and small-business preferences shift procurement toward eligible firms (Brown, Denes, Duchin, and Hackney, 2024). We show that once a sponsor provides capital and management, small-business contractors reduce their reliance on protected work and grow at least as fast as other buyouts. This suggests that small-business programs serve their intended purpose as a springboard for developing firms, which graduate from protected work once they have other sources of capital and support.

Finally, we contribute to the measurement of how government contracting responds to events. Federal contracting occurs in spells (Cox et al., 2024), and any event that selects firms while they are actively contracting, whether a merger, a change of ownership, or a political shock, will appear to be followed by a decline in government business. In our setting, comparing targets with all contractors overstates the effect of a buyout by a factor of about ten (Section 3.3). Studies of how firms' government business responds to such events therefore need to account for where firms stand in their contracting spells.

2. Two Views of Government Contracting

Concerns about waste in procurement often assume that the government overpays and that the firms selling to it pocket the difference. Evidence from the buyer's side suggests that excess costs often arise for other reasons. Bandiera, Prat, and Valletti (2009) distinguish active waste, which benefits the official who makes the purchase, as a bribe does, from passive waste, which does not benefit the official and arises from a lack of skill or effort, red tape, and cumbersome procedures. In Italian public procurement, they estimate that passive waste accounts for 83% of the total excess cost. Liebman and Mahoney (2017) document a source of passive waste in US federal procurement: expiring budgets lead agencies to spend almost five times the usual weekly amount in the last week of the fiscal year, on information technology projects of markedly lower quality. Neither kind of waste requires that the firms selling to the government earn excess profits: passive waste can raise the price a supplier receives, or be lost in compliance costs. Whether contractors earn excess profits on government work is therefore a separate question, and it is the one we address.

We frame this question around two views of government contracting, which make opposite predictions about how a profit-maximizing owner should treat government work. Under the first, which we call the rent-extraction view, government contracts leave surplus with sellers, who capture it through political access, discretionary awards, and the renegotiation of existing contracts. Under the second, which we call the springboard view, the government is a demanding buyer that keeps margins competitive, but government work benefits firms in other ways: it provides stable demand, access to finance, and a record of reliable performance that helps them win other business and grow. Before turning to the two views, we briefly explain how federal procurement works.

2.1. Federal Procurement

The federal procurement process begins when an agency identifies the need for a product or service and issues a solicitation. Potential bidders submit offers, which the agency evaluates on price and on non-price criteria such as the contractor's ability to perform, its past performance, and its record of integrity. These rules are set out in the Federal Acquisition Regulation (FAR). Most contracts are awarded through this competitive process. A smaller set is awarded without competition to a single qualified contractor, typically when only one source can meet the agency's technical requirements.

Contracts also differ in how they are priced. Under a fixed-price contract, the price is set in

advance, and the contractor bears the risk that its costs exceed the price. Under a cost-plus contract, the government reimburses the contractor's allowable costs and pays an additional fee, so most of the cost risk stays with the government. After a contract is awarded, the agency and the contractor may modify it. A modification can extend the contract's period of performance, which we refer to as a time extension, or increase its amount, which we refer to as a funding increase. Modifications arise from changes in the scope of the work, from problems during execution, and, as we discuss below, possibly from the contractor's ability to renegotiate the terms in its favor.

Small businesses occupy a special place in this system. Federal law sets a goal that 23% of federal contract dollars go to small businesses, and agencies meet it largely by reserving contracts for them. A contracting officer must set aside a contract for small businesses when at least two small firms are expected to bid at a fair price, and some programs, such as the Small Business Administration's 8(a) program for disadvantaged firms, allow contracts to be awarded to eligible firms without competition. Whether a firm counts as small depends on a size standard, based on its employees or revenue, that varies by industry. A firm certifies whether it is small under each industry's standard in its registration in the System for Award Management (SAM), and this self-certification is what allows it to compete for reserved contracts. On each award, the contracting officer also records whether the contractor is small under the standard that applies to the work; the two need not agree (Table A3). We refer to this reserved segment as protected work: a small business bidding for it competes only against other small firms, and sometimes against no one.

Two features of these rules matter for our setting. First, a firm's size is measured together with every entity under common control. When a private equity sponsor acquires a contractor, the contractor's size may therefore be combined with that of the sponsor's other portfolio companies, and an acquired firm must recertify its size after the deal. Second, small-business programs are designed as a temporary form of support. The 8(a) program has a fixed term of nine years, and small-business preferences in general aim to help firms build the capacity to compete without protection.

2.2. The Rent-Extraction View

The rent-extraction view holds that government contracts can be priced above the cost of performance and that firms with the right access capture the difference. Three strands of evidence support this view.

The first strand concerns how contracts are allocated. Goldman, Rocholl, and So (2013) study S&P 500 firms around the 1994 midterm election, in which control of both chambers of Congress

changed. Firms whose boards included former politicians connected to the winning party received significantly more procurement dollars after the election, and firms connected to the losing party received less. Boland and Godsell (2021) show that such influence operates where officials have discretion. In the last two weeks of each quarter, when budget rules push agencies toward non-competed awards, firms that lobby are more likely to win non-competed contracts, and those contracts are about 10% larger and more often cost-plus. There is no such pattern for competed contracts or outside the quarter-end window. Political access, combined with the discretion that contracting officers enjoy, can therefore shape who receives government contracts and on what terms.

The second strand places rents after the award, in the renegotiation of contracts already in place. Brogaard, Denes, and Duchin (2021) show that politically connected public firms obtain more favorable renegotiations of contract amounts and deadlines. When a connected politician dies or leaves office unexpectedly, the probability that the firm's contracts are favorably renegotiated falls by about half, and the firm's stock price declines. In this account, contractors earn rents less by winning contracts than by revising them once they are in place.

The third strand comes from settings with weaker institutions. Mironov and Zhuravskaya (2016) use Russian banking data to show that firms with procurement revenue divert cash around regional elections, consistent with payments to politicians in exchange for contracts, and that in more corrupt regions contracts go to less productive firms. The setting is far from US federal procurement, but it illustrates the view in its most extreme form.

The small-business rules described in Section 2.1 create another potential source of rents. Because protected work is open only to small firms, and some of it is awarded without competition, its prices face less competitive pressure than those in open procurement. A firm with access to protected work may therefore earn higher margins on it, and an owner seeking rents would want to keep that access and bid for more of this work.

The rent-extraction view therefore predicts that a sponsor that acquires a federal contractor will grow its government business relative to comparable contractors, and will keep and expand the target's access to protected work. Sponsors should also obtain more favorable modifications of the target's existing contracts, especially if they are politically connected, and contractor deals should earn higher returns than comparable buyouts.

2.3. The Springboard View

The springboard view is that competition and the government's bargaining position keep margins on government contracts competitive. Firms nonetheless value government work for benefits that private customers rarely offer, such as stable demand, better access to financing, and a record of reliable performance.

Evidence that the government captures most of the surplus comes from structural and institutional studies of procurement. Kang and Miller (2022) estimate a model of federal information technology procurement in which the agency chooses how much competition to solicit and then negotiates terms. Contracts attract few bids because the agency can extract surplus from sellers through negotiation, while administrative hurdles and corruption play a very limited role. In their model, the agency squeezes the profits of high-cost sellers to zero, and most sellers in their sample win only one contract over twelve years. Bajari, Houghton, and Tadelis (2014) show that renegotiation in public construction is costly for contractors rather than a source of rents: adaptation costs account for 7.5% to 14% of the winning bid and are priced into bids, while markups are much smaller. In interviews reported by Josephson, Lee, Mariadoss, and Johnson (2019), government contracting executives describe government work as deliberately kept low-margin and costly to enter, requiring specialized investments and compliance systems.

The springboard view also offers an alternative reading of contract modifications. Calvo, Cui, and Serpa (2019) exploit a dollar threshold above which contracts are subject to closer oversight and find that oversight itself increases delays and cost overruns. Time extensions and funding increases are therefore not, by themselves, evidence of rents: they reflect, at least in part, changes in scope and administrative friction on the government's side. What would point to rents is an increase in such modifications after an owner with strong incentives to exploit renegotiation takes control.

If margins are competitive, why do firms sell to the government? A large literature identifies benefits of government work that operate outside the contract margin. The first is access to finance. Hebous and Zimmermann (2021) find that each dollar of federal purchases raises the capital investment of financially constrained US firms by 10 to 13 cents over four quarters, with no effect on unconstrained firms. Using Spanish credit-register data, di Giovanni et al. (2025) show that lenders treat procurement revenue as more pledgeable than revenue from private customers, so that government contracts help firms grow out of their borrowing constraints.

The second benefit is stability. Goldman (2020) shows that federal contracts insulated US

contractors during the 2008–2009 financial crisis: by 2009, contractors had 15% higher market capitalization, 18% higher capital expenditures, and 26% more bank credit than similar firms. The advantage faded after 2010, and contractors grew somewhat more slowly in the recovery, although these differences are not statistically significant.

The third benefit is reputation and learning. Winning and executing government contracts gives a firm a record of reliable performance, which helps it win further contracts and attract private customers and investors. Ferraz, Finan, and Szerman (2015) compare Brazilian firms that narrowly win and narrowly lose procurement auctions and find that winning raises employment growth, with effects that outlast the contracts and arise partly because winners enter new markets. Hvide and Meling (2023) find that Norwegian startups that narrowly win a procurement auction have about 20% higher sales and employment several years after the contract has ended. Government awards can also certify a firm's quality to private investors: Lerner (1999) finds that recipients of Small Business Innovation Research awards grow faster than matched firms, especially in regions with active venture capital markets, while Howell (2017) shows that early-stage awards raise subsequent venture financing by funding prototypes.

Small-business programs build on these benefits. They reserve work for firms that are expected eventually to compete without protection, and Brown, Denes, Duchin, and Hackney (2024) show that eligibility for small-business preferences shifts procurement substantially toward newly eligible firms. Protected work allows a small firm to establish a record with the government that it could not easily establish in open competition.

Consistent with this view, government work is often a stage that firms pass through rather than a permanent line of business: in the universe of federal contracts, the median firm wins new contracts in only two years (Cox et al., 2024). The springboard view therefore predicts that sponsors should not expand their targets' government business, because it offers no excess margins, and that contractor deals should earn ordinary returns. Because much of the value of government work comes from stable demand, access to finance, and a record of performance, it should matter less once a sponsor provides capital and management. The targets that benefited most from government work, such as small firms, should therefore rely on it less after the buyout, without harm to their growth. Where targets leave government work, they should leave the work on which pricing is most competitive.

2.4. Why Private Equity Separates the Two Views

Comparing contractors with other firms cannot distinguish the two views. Under both, firms seek government business: under the rent-extraction view because it offers rents, under the springboard view because it offers finance, stability, and a path to growth. The two views differ in what a profit-maximizing owner with capital and expertise should do with a contractor's government work once it controls the firm. A buyout allows us to observe this choice, because the sponsor acquires control of the firm and can change its strategy.

If procurement offers rents, private equity is the investor most likely to find and exploit them. Sponsors' compensation depends on the returns of their deals, their acquisitions are financed with debt, and their holding periods are short, all of which give them strong incentives to raise profits (Jensen, 1989; Kaplan and Strömberg, 2009). A growing literature shows that sponsors expand revenue drawn from government programs when the terms are favorable. Eaton, Howell, and Yannelis (2020) show that private equity buyouts of for-profit colleges raise tuition and student borrowing, much of it federally funded, while student outcomes deteriorate. Gupta, Howell, Yannelis, and Gupta (2024) find that private equity ownership of nursing homes raises taxpayer spending per patient while patient outcomes worsen. Faccio and Hsu (2017) show that the targets of politically connected sponsors create more jobs, especially in election years, consistent with an exchange of favors with politicians. Sponsors therefore have the incentive, the expertise, and the capital to expand a profitable government business, and a record of doing so.

Our test rests primarily on what sponsors do with a contractor's government business after they acquire it. Because the profits on contracts won after the deal go to the new owner, a sponsor that saw government work as unusually profitable would expand it. The rent-extraction view predicts this expansion; the springboard view predicts none. Returns provide complementary evidence: under the rent-extraction view, contractor deals should earn more than comparable buyouts bought at similar valuations, and sponsors that grow their targets' government business should be rewarded for doing so.

The two views make their sharpest opposing predictions for small-business contractors. The rent-extraction view predicts that a sponsor would keep and exploit a target's access to protected work. The springboard view predicts the opposite: because a sponsor relaxes exactly the constraints that make government work valuable to small firms, these targets should scale down their government business more than comparable contractors after the buyout, without harm to the growth of the

business. In the rest of the paper, we test these predictions.

3. Data and Empirical Design

To study what private equity sponsors do with the government business of the firms they acquire, we combine federal procurement records with data on private equity deals and their returns. This section describes the data and the sample, explains how we measure a firm's government business, and shows why the choice of comparison group is central to any study of how government contracting responds to a change of ownership.

3.1. Data and Sample

Our data on federal contracts come from USAspending.gov, which reports every contract awarded by federal agencies, together with all later modifications of these contracts. We use the records from fiscal year 2003 through 2025. Reporting became comprehensive in fiscal year 2008,[1] so we study deals completed from 2008 on and use the earlier records only to measure the contracting history of the firms before the deal. For each contract, we observe the contractor, the date of the award and its initial value, whether the price is fixed in advance or covers the contractor's costs plus a fee, how many firms competed for it, and whether the contracting officer classified the contractor as a small business. We also observe subsequent modifications, which extend a contract's period of performance or increase its amount. For each contractor and fiscal year, we count the new contracts it wins and their dollar value, and we track the modifications of its existing contracts separately.

Our data on deals come from PitchBook, which covers buyouts, growth equity investments, and venture capital rounds, together with the sponsors involved and the characteristics of the target. To link the deals to the procurement records, PitchBook provided us with the DUNS numbers of the target companies, the identifier the federal government uses for its contractors.[2] Because we match on the target's own identifier, we follow the government business of the firm the sponsor actually acquired. Contracts won by the seller's other subsidiaries, or by a former parent after a carve-out, are not attributed to the target.

A small number of contractors re-register under a new DUNS number, for instance after a change of legal name or when the government transfers their contracts to a new legal entity. Such a firm

[1] The reporting requirements of the Federal Funding Accountability and Transparency Act (FFATA) of 2006 ensure reliable and comprehensive coverage from fiscal year 2008.

[2] We thank PitchBook for providing the DUNS numbers of the target companies in our sample.

appears to stop contracting under its old DUNS number and to start as a new contractor under a new one. For our analysis, the concern is that a re-registration could make a target appear to stop contracting after the deal. This affects 55 targets in our sample. For these firms, we combine the contracts recorded under the old and new numbers, which has little effect on our results.

For a subset of our deals, we obtain returns from Burgiss, which reports each deal's internal rate of return and total value multiple, as well as the target's revenue, EBITDA, and enterprise value at entry and exit. From each deal's cash flows we also compute its public market equivalent (PME) of Kaplan and Schoar (2005), against a stock index matched to the target's size and industry sector. We construct the return variables, and draw a second sample of deals from StepStone, following Gornall, Gredil, Howell, Liu, and Sockin (2025), and we describe both datasets in Section 5.

Our main sample consists of buyouts completed between 2008 and 2023 of established contractors: firms whose first federal contract in our data was awarded at least three years before the deal. We require this history because, as we explain below, we compare each target with contractors that were in a similar position before the deal.[3] The final sample contains 3,587 buyout targets. It consists primarily of mid-market private firms; the largest defense contractors are publicly listed and are rarely the targets of buyouts. We construct a sample of 565 growth equity targets in the same way. Growth equity investors provide capital but take minority stakes without taking control, so their targets show how government business evolves when a firm receives outside capital but its owners cannot change its strategy.

Table 1 describes the sample. Panel A reports the deals by type. Buyout targets are mature firms, with an average age of 29 years at the deal, compared with 4 years for the targets of venture capital rounds. Panel B describes the buyout sponsors. The median sponsor completes about four deals a year, and on average 9% of its deals involve a target active in federal contracting in the deal year (4% of all their deals pooled). We measure sponsors' connections from the biographies of their professionals in PitchBook: 16% of sponsors are politically connected, 14% are connected to a federal agency that awards contracts, and 8% are connected to Congress.[4]

Panel C compares contractors that are acquired in a PitchBook deal with contractors that are

[3] We also exclude targets that formally changed their status from small business to other than small after the deal (108 targets in our sample, 107 of them observed in the deal year). We discuss these firms in Section 4.3. Including them leaves our results unchanged (Table 5, last rows).

[4] Following Faccio and Hsu (2017), a sponsor is politically connected if the biographies of its professionals mention service in a legislative body, an executive branch position, a political party, or a federal department. A sponsor is connected to a federal agency if one of its professionals previously worked at an agency that awards contracts in our sample, and connected to Congress if the biographies refer to legislative experience only.

never acquired. Acquired contractors win about four times as many new contracts a year and about three times the contract dollars, and they have longer contracting histories: on average, six years have passed since their first contract, compared with less than four years for other contractors. Private capital is therefore drawn to firms with a substantial and established government business rather than to occasional suppliers. This makes our setting well suited to the question we ask as the firms that sponsors acquire have enough government business for its profitability to matter to the new owner.

3.2. Measuring Government Business

Our main outcome measures how much new government business a firm wins relative to other contractors in the same year. In each fiscal year, we rank all contractors that are never acquired by the number of new contracts they win and assign each firm its percentile in that ranking. A firm at the 50th percentile wins as many new contracts as the median never-acquired contractor that year, and a firm that wins no new contract is assigned zero. We construct the same measure using the dollar value of new contracts.

We use ranks rather than levels because government contracting is highly skewed. Among acquired contractors, the average annual number of new contracts exceeds the 90th percentile of the distribution (Table 1, Panel C), so changes in levels would be driven by a handful of large firms and awards. Ranks make firms of very different sizes comparable, limit the influence of the largest awards, and remove changes in total federal spending from one year to the next. In most of the analysis, we focus on new awards, because they capture the new business a firm chooses to pursue and manages to win. We study modifications of existing contracts separately in Section 4.5.

Because small businesses play a central role in federal procurement (Section 2.1), we distinguish them throughout. We classify a target as a small-business contractor if it won more than half of its size-determined contracts in the three fiscal years before the deal year as a small business, that is, on contracts on which the contracting officer recorded it as small. We refer to the remaining targets as other targets. Small-business contractors account for 56% of the targets in our sample. These are the firms with access to the protected segment of government work, and the two views make opposite predictions about what a sponsor should do with that access.

3.3. Contracting Spells and the Comparison Group

Firms rarely sell to the federal government continuously. They typically win new contracts for several consecutive years and then stop, sometimes returning later. We call each run of consecutive years with new awards a contracting spell. Table 2 shows that most spells end within a few years. Among never-acquired contractors, more than 80% of firms in a spell of seven years or more win a new contract the following year, compared with less than half of firms in a spell of one to three years and only about 15% of firms that have been idle for a year or two. The longer a firm has been contracting, the more likely it is to continue. Yet even long spells end, and once a firm stops winning contracts, relatively few return. The same pattern holds for contractors that sell mainly to the Department of Defense. These facts are consistent with Cox et al. (2024), who show that the median federal contractor wins new contracts in only two years. Selling to the government is, for most firms, an episode rather than a permanent line of business.

This feature of government contracting matters for our analysis because targets are far more active than other contractors when they are acquired. In the year before the deal, 79% had won a new contract in the preceding two years, compared with 59% of never-acquired contractors. Similarly, 22% were in a spell of seven years or more, compared with 9% of never-acquired contractors (Table 1, Panel D). A firm chosen while it is actively contracting is likely to see its spell end in the following years, as most spells do. Its government business would therefore decline after the deal even if the new owner changed nothing. Many other contractors, by contrast, are idle and can only become more active. Comparing targets with all contractors, including these idle firms, would therefore attribute the ordinary end of contracting spells to the buyout. The problem is similar to the one Barber and Lyon (1996) identify for firms selected on their recent operating performance, and so is the remedy: firms should be compared with firms that were in a similar position before the event.

We therefore compare each target with its matched contractors: never-acquired contractors that were in a similar position in the year before the deal. A contractor is in a similar position if it is observed in the same year, won a similar number of new contracts in the two preceding years and in the two years before that (none, 1 to 2, 3 to 9, or 10 or more), was in the same tercile of contract dollars over the preceding five years, and was at the same point in its contracting spell (in a spell of 1 to 3, 4 to 6, or 7 or more years, or idle for 1 to 2 or 3 or more years). We then follow the matched contractors forward, exactly as we follow the targets, including those that stop contracting. Because targets and their matched contractors start from the same position, their spells should, absent the

buyout, end at the same rate. Any difference in how their government business evolves after the deal can then be attributed to the change of ownership. Our approach follows the logic of conditional difference-in-differences designs (Abadie, 2005; Callaway and Sant'Anna, 2021).

We measure the effect of a buyout in each year as the change in the target's rank since the year before the deal, minus the same change for its matched contractors. Averaging these differences across targets gives the effect in each year after the deal, and the years before the deal allow us to verify that targets and their matched contractors were on similar paths.

Table 3 shows why the choice of comparison group matters. Columns 1 and 4 compare targets with all contractors that have not been acquired by the same date. Against this benchmark, targets steadily lose government business after the deal: on average from the deal year through the seventh year, their rank declines by about 7 percentile points more than that of all contractors, in both contract counts and contract dollars, and by the seventh year the gap exceeds 14 points. Taken at face value, these estimates would suggest that private equity owners walk away from government work much more than other contractors. Before the deal, however, targets already differ from all contractors, and these differences are statistically significant at the 1% level. This is a first sign that the benchmark is not appropriate.

Columns 2 and 5 show that the decline does not require a buyout. For each target, we draw one of its matched contractors, assign it the target's deal year, and treat it as if it had been acquired. Nothing happened to these pseudo buyouts, yet against all contractors they decline almost as much as the actual targets, by about 6 percentile points in both counts and dollars. The decline in columns 1 and 4 therefore reflects when sponsors buy rather than what they do after buying.

Columns 3 and 6 compare targets with their matched contractors, which were at the same point in their contracting spells and had similar contracting histories in the year before the deal. The two groups follow the same path prior to the deal: the estimates are close to zero and jointly insignificant ($p = 0.94$ for counts and $p = 0.50$ for dollars). After the deal, the rank of targets declines by 0.8 percentile points more than that of their matched contractors in contract counts and by 0.6 points more in contract dollars. These estimates are statistically significant, at the 1% and 5% levels, respectively, but they are economically small, about one-tenth of the gap measured against all contractors. Thus, matching on where targets stand in their contracting spells removes about nine-tenths of the apparent effect of the buyout. We use this comparison throughout the paper.

Table A1 provides additional detail on our matching approach and shows why each of the

conditions we impose matters. Matching on recent contracting activity alone, or on the firm's position in its contracting spell alone or combined with its size or recent awards, leaves targets and their comparison firms on different paths before the deal: under each of these alternatives, the estimates before the deal are jointly significant. Within such broad groups, sponsors pick the more active contractors, so the comparison firms are not in the same position as the targets. Only our main design, which matches on the firm's recent contracts, its contract dollars, and its position in its contracting spell, lines up the paths before the deal. A finer version of the same design, with ten bins of spell length and deciles of recent contracts, does not improve the fit: its estimates before the deal are jointly significant for contract counts ($p = 0.004$).

These results have implications beyond our setting. Because federal contracting occurs in spells, any event that selects firms while they are actively contracting, whether a merger, a change of ownership, or a political shock, will appear to be followed by a decline in government business. In our data, a comparison with all contractors overstates the effect of a buyout by a factor of about ten. Studies of how firms' government business responds to such events therefore need to compare firms at similar points in their contracting spells, or they risk attributing the ordinary end of these spells to the event.

4. Private Equity Ownership and Government Contracting

In this section, we turn our attention to what private equity sponsors do with the government business of the firms they acquire. We begin by asking which contractors sponsors choose to buy. We then follow the targets after the deal and compare them with their matched contractors. This comparison reveals whether sponsors expand their targets' government business, as the rent-extraction view predicts, which targets scale it down most, and what kind of work they leave. Finally, we ask whether sponsors obtain more favorable terms on the contracts their targets already hold.

4.1. Which Contractors Private Equity Buys

We begin by asking whether sponsors seek out federal contractors. Table 4 compares buyouts with other types of private capital investment, such as growth equity and venture capital. The sample includes all of the roughly 187,000 private capital deals in the PitchBook database, including deals for firms that have never sold to the government, and the dependent variable equals one if the deal is a buyout. We measure a firm's government business over the three years before the deal in

three ways: the dollar value of its contracts outside the programs reserved for small businesses, the number of contracts it won as a small business, and the length of its current contracting spell, each as the logarithm of one plus the value. To separate the preferences of private equity sponsors from those of other investors, we interact each measure with indicators for deals with a private equity sponsor and deals with other investors only. All specifications include industry-by-year fixed effects, as well as fixed effects for the type of investor, the firm's age, and the length of its contracting history relative to its age.

Private equity sponsors are drawn to contractors. Among deals with a private equity sponsor, firms with more government business are significantly more likely to be bought out. In column 1, the effect comes from contracts won as a small business, and it is economically large: a firm that won about seven such contracts over the three years before the deal is about 6 percentage points more likely to be acquired in a buyout than a firm with no government business. Because buyouts account for about 32% of the deals in our sample, this is an increase of about one-fifth. Once we control for the firm's earlier private capital deals, such as previous venture capital rounds or buyouts (column 2), the dollar value of its other federal contracts also predicts a buyout. Both effects become somewhat larger when we compare firms with a similar number of employees (column 3). The preference therefore does not simply reflect an interest in larger firms. Column 4 shows that sponsors also favor firms with longer contracting spells: the coefficient on spell length is positive and significant at the 1% level, while the coefficient on small-business contracts falls by about half but remains significant at the 5% level. Sponsors thus seek out firms that have sold to the government for many consecutive years.

This preference is specific to buyout sponsors. For deals with other investors only, every measure of government business enters with a negative coefficient, significant at the 1% level in all specifications. Table A2 shows that firms with more contract dollars outside small-business programs are more likely to receive a growth equity investment (significant at the 1% level in column 1), but that this effect disappears once we compare firms with a similar number of employees (column 2), and that firms with more small-business contracts and longer contracting spells are less likely to receive a venture capital round.

The univariate comparisons in Table 1, Panel D, suggest that sponsors are drawn to active contractors rather than to small-business status as such. In the year before the deal, buyout targets are much more likely than never-acquired contractors to rank in the top third of contractors by contract dollars (58% compared with 33%), but only slightly more likely to be small-business contractors

(56% compared with 52%). Together with the results in Table 4, this indicates that target selection is driven by a firm's record of winning government contracts, whether as a small business or not.

Sponsors' preference for contractors is consistent with both views. Under the rent-extraction view, sponsors buy contractors to exploit the rents on their government work, including the protected access that small-business status provides. Under the springboard view, sponsors buy firms that have used government work to build a reputation for reliable performance, and that need it less once the sponsor provides capital and management. The two views therefore agree on whom sponsors buy, but they disagree on what sponsors should do next, which is the focus of the rest of this section.

4.2. Government Contracting after the Buyout

We now follow the targets after the deal. Figure 1 plots the average change in the percentile rank of targets (solid lines) and of their matched contractors (dashed lines) since the year before the deal, for each year from five years before to seven years after the deal. The left panels measure government business by the number of new contracts, and the right panels by their dollar value. The vertical distance between the two lines is the estimated effect of the buyout, and the shaded band is its 95% confidence interval.

Panel A shows that the effect of the buyout is small compared with the decline that targets share with comparable contractors. Before the deal, targets and their matched contractors follow the same path. After the deal, the government business of both groups declines steadily: by the seventh year, the rank of targets in contract counts has fallen by about 24 percentile points and that of their matched contractors by about 21 points, with almost identical declines in contract dollars. This common decline reflects the end of contracting spells documented in Section 3.3, and it occurs with or without a change of owner. The gap between the two lines, which measures the effect of the buyout, reaches only about three percentile points by the seventh year.

Panels B and C show that this small average effect combines two groups of targets that behave differently. Small-business contractors (Panel B) follow their matched contractors closely until the deal and then scale down their government business more: by the seventh year, their rank in contract counts has fallen by about 31 percentile points, compared with 27 points for their matched contractors. The government business of other targets (Panel C) also declines, by about 16 percentile points by the seventh year, but no faster than that of their matched contractors, which decline by about 15 points. In the first years after the deal, other targets in fact decline somewhat more slowly. We further study the two groups in Section 4.3.

Table 5 reports the corresponding estimates. Each row reports the difference-in-differences estimate for a given year: the change in the targets' rank since the year before the deal, relative to the same change for their matched contractors. At the bottom, the table reports the average effect from the deal year through the seventh year after the deal, the p-value of the joint test that the estimates before the deal are zero, and the change in rank by the seventh year for targets and matched contractors separately.

The estimates for all targets in columns 1 and 2 confirm the pattern in Figure 1. Before the deal, the estimates are all below 0.6 percentile points in absolute value and jointly insignificant ($p = 0.94$ for counts and $p = 0.50$ for dollars), as we would expect if the matched contractors are a good comparison group. In the deal year, targets gain slightly on their matched contractors, by about 1.3 percentile points in contract counts and 1.4 points in contract dollars. From the first year after the deal, the estimates turn negative and grow over time, reaching −3.0 percentile points in contract counts and −2.8 points in contract dollars by the seventh year. On average from the deal year through the seventh year, the rank of targets declines by 0.8 percentile points more than that of their matched contractors in contract counts and by 0.6 points more in contract dollars, significant at the 1% and 5% levels, respectively. These effects are economically small and precisely estimated. The upper end of the 95% confidence interval is −0.4 percentile points for counts and −0.1 points for dollars, which rules out any meaningful expansion of the targets' government business.

This result is central to distinguishing the two views. If government work offered margins above those that firms earn elsewhere, a sponsor that gained control of a contractor would expand its government business. Yet sponsors that seek out contractors with long and active records of government work do not grow these firms' government business after the deal. This finding is consistent with the springboard view: if government work earns no excess margins, a new owner has no reason to pursue more of it.

Growth equity investments provide a useful comparison. Growth equity investors provide capital to younger, expanding firms but take minority stakes without taking control, so unlike buyout sponsors, they cannot change the firm's strategy. Panel B of Table 5 shows that the government business of these firms also declines after the deal, but more slowly than that of their matched contractors. By the seventh year, their rank in contract dollars has fallen by about 14 percentile points, compared with about 21 points for their matched contractors. On average from the deal year through the seventh year, the difference is 4.7 percentile points, significant at the 1% level. The estimate for contract counts is similar, at 4.3 percentile points. We focus on contract dollars

because, in contract counts, growth equity targets were already gaining on their matched contractors before the deal ($p = 0.03$). The retreat from government work therefore appears when an owner takes control of the firm, not merely when it receives outside capital, consistent with the decline reflecting the sponsor's choices. It also suggests that agencies do not shut out investor-backed firms. The slower decline for growth equity targets is also consistent with the springboard view, since these younger firms are still building the record of performance that government work provides.

4.3. Small-Business Contractors

The small average effect masks a clear difference between small-business contractors and other targets. Columns 3 and 4 of Table 5 report the estimates for small-business contractors. Before the deal, these firms follow their matched contractors closely: none of the estimates for contract counts exceeds 0.3 percentile points in absolute value, and the estimates are jointly insignificant ($p = 0.96$ for counts and $p = 0.43$ for dollars). The difference emerges in the first year after the deal, when their rank in contract counts declines by 2.8 percentile points more than that of their matched contractors, significant at the 1% level. The gap widens to about 5 points by the third year and remains between 2.4 and 4.2 points through the seventh year. On average from the deal year through the seventh year, small-business contractors scale down their government business by 2.8 percentile points more than their matched contractors in contract counts and by 2.0 points more in contract dollars, both significant at the 1% level. This effect is more than three times the average effect for all targets.

Other targets behave differently (columns 5 and 6). Their government business also declines after the deal, but in the first years it declines by about 2 percentile points less than that of their matched contractors. This difference fades over time, and by the seventh year the estimates are negative and insignificant. On average from the deal year through the seventh year, the decline of other targets is 1.8 percentile points smaller than that of their matched contractors in counts and 1.3 points smaller in dollars. The additional decline among all targets therefore comes from small-business contractors, which scale down their government business more than comparable contractors.

A possible concern is that small-business contractors scale down their government business because they lose their eligibility for protected work rather than by choice. When a private equity sponsor acquires a firm, the firm's size may be combined with that of the sponsor's other portfolio companies, which could make it too large to bid for contracts reserved for small businesses. Formal reclassifications, however, are rare. Fewer than 3% of targets that continue to contract with the

government formally change their status from small to other than small after the deal, and we exclude these firms from our sample.[5] Most targets continue to be treated as small businesses: among those whose registration we can observe when they win a new contract, two-thirds certify themselves as small on at least one new award, and nearly a third win a contract reserved for small businesses (Table A3). Among small-business contractors, these shares are even higher: 83% certify themselves as small on at least one new award, and about 42% win a contract reserved for small businesses.[6] Small-business contractors thus retain their access to protected work after the buyout, yet choose to rely on it less.

Why, then, do small-business contractors scale down their government business more than other targets? The springboard view offers a natural explanation. For a small firm, government work provides stable demand, revenue that lenders accept as collateral, and a record of reliable performance that helps it win other customers. A private equity sponsor provides capital, access to lenders, and management, and it brings its own reputation to the firm. Once a sponsor provides these benefits, the firm needs government work less and can redirect its resources toward other customers. As we show in Section 5.1, these targets' profits grow faster than those of comparable buyouts, which suggests that this business is at least as profitable as the government work they give up.

Small-business contractors provide the sharpest test of the two views. Under the rent-extraction view, access to protected work is precisely what a rent-seeking owner would want to keep: protected work faces fewer competitors, so a sponsor seeking rents would bid for more of it after the deal. Under the springboard view, small-business programs help firms build capacity and a record of performance, and this support matters less once a sponsor provides capital and management. The evidence supports the springboard view. The firms with the most favorable access to government work, which keep that access after the deal, rely on it less than comparable contractors once they have a private equity owner. In Section 5.1, we show that this does not come at the expense of the business.

[5] Table A3 covers all buyouts of federal contractors in our data. Of the 5,308 deals, 4,302 involve targets that continue to contract with the government in the three years after the deal, and in 126 of these the target formally changes its status from small to other than small. These 126 deals involve 122 firms, of which 81 would otherwise meet our sample criteria. The remaining 27 targets that we exclude change their status in a later filing, through a transfer of their contracts to a new legal entity, or outside the window that Table A3 covers.

[6] Whether a firm controlled by a private equity sponsor remains eligible for small-business awards depends on how the affiliation rules apply to the sponsor's other holdings. We take the firms' certifications and the contracting officers' determinations as given.

4.4. Exit by Type of Work

Beyond how much new business targets win, sponsors face a starker choice: whether to stop selling to the government altogether. Under the rent-extraction view, sponsors would rarely abandon government work. Under the springboard view, they should be most willing to leave the work on which margins are thinnest. Table 6 examines whether targets stop contracting with the government from the deal year through the fourth year after it, and how this decision depends on the kind of work they did before the deal.

The dependent variable equals one if the firm wins no new federal contract from the deal year through the fourth year after the deal. Each target is compared with never-acquired contractors in the same stratum: the same deal year and pre-deal activity cell, and the same pattern of new awards in each of the three years before the deal. These strata are finer than the matched groups of our main analysis, because the outcome is itself an indicator of new awards. The first row of Panel A reports the change in the probability of exit for all targets, and each subsequent row reports how much this change differs for targets with a given type of work before the deal. We classify the type of work using the contracts a firm won before the years on which we match it with comparable contractors. Columns 4 and 5 repeat the analysis for the fourth and fifth years before the deal, to test whether targets with a given type of work were already more likely to stop contracting, and columns 6 to 10 report the same estimates for growth equity targets.

On average, buyout targets are no more likely than comparable contractors to stop selling to the government (the estimate is 0.3 percentage points and insignificant), consistent with the results in Table 5. Whether a target leaves, however, depends on the kind of work it did. In line with their larger decline in new contracts, small-business contractors are 4.5 percentage points more likely than other targets to stop contracting with the government, significant at the 1% level.

Targets are also more likely to leave the work on which margins are likely to be thinnest. Targets whose contracts were fixed-price and drew three or more bids on average are 5.4 percentage points more likely than other targets to stop contracting with the government, significant at the 5% level. Under such contracts, the contractor bears any cost overruns, and competition among several bidders keeps prices low. It is the combination of fixed prices and competition that matters: targets whose work was entirely fixed-price are no more likely to leave than other targets (0.9 percentage points, insignificant), and neither are targets whose contracts drew many bids. This pattern is difficult to reconcile with a reluctance to bear the risk of cost overruns, which would lead sponsors to leave all

fixed-price work. Cost-plus contracts, under which the government rather than the contractor bears cost overruns, are rare among our targets, and their exit rates do not differ significantly from those of other targets.

By contrast, targets whose work was mostly awarded without competition are 2.7 percentage points less likely than other targets to stop contracting, significant at the 10% level. Non-competed contracts are typically awarded to a firm whose capabilities the agency cannot easily find elsewhere, and they often reflect long relationships between the firm and the agency, so a new owner has little reason to abandon them. Keeping this work is also consistent with the rent-extraction view, since non-competed contracts may leave room for rents. Section 4.5 distinguishes the two readings by examining whether sponsors obtain more favorable terms on their targets' existing contracts.

Sponsors thus scale back government work selectively, depending on the type of work. Such selectivity is also difficult to reconcile with an aversion to the compliance and disclosure requirements of government work, which apply to all types of contracts. Several further results suggest that these patterns reflect the change of ownership. None of the differences across types of work is significant before the deal (columns 4 and 5), so targets with different types of work were not already on different paths when they were acquired. Across all targets, the estimate before the deal is small and of the opposite sign: targets were slightly less likely than comparable contractors to stop winning new contracts (0.7 percentage points, significant at the 5% level). If anything, targets were more persistent contractors before the deal, which makes it harder, not easier, to find that they leave government work afterward. Nor do these differences appear for pseudo buyouts (Panel B), never-acquired contractors assigned the deal year of a target in the same position. On average, pseudo buyouts are 1.6 percentage points less likely than the firms they are compared with to stop contracting entirely (significant at the 1% level), but the difference does not vary with the type of work and does not appear before the assigned deal year (column 4). Finally, growth equity targets are 5.9 percentage points less likely than comparable contractors to stop contracting (column 6), consistent with the smaller decline in their government business in Table 5 (Panel B).

4.5. Contract Modifications

Some of the strongest evidence that contractors earn rents concerns not new awards but the renegotiation of existing contracts. Brogaard, Denes, and Duchin (2021) show that politically connected firms obtain more favorable renegotiations of contract amounts and deadlines, and that these renegotiations become less favorable when firms lose their connections. A sponsor seeking

rents could therefore leave a target's new business unchanged and still profit from modifying its existing contracts. Table 7 examines this possibility for two kinds of modifications: time extensions, which lengthen a contract's period of performance, and funding increases, which increase a contract's amount.

Panel A compares the contracts that a target held at the time of the deal with those of its matched contractors. For each contract, we measure whether it receives a time extension or a funding increase from the deal year through the second year after the deal and how much its final value grows relative to its initial value. The unit of observation is the contract. We weight each contract by the inverse of the number of contracts its firm holds, so that every target receives the same total weight regardless of how many contracts it has. After the deal, the targets' contracts are 2.1 percentage points less likely to receive a time extension than those of their matched contractors, significant at the 5% level, and 1.6 percentage points less likely to receive a funding increase, significant at the 10% level. Their final value also grows by about 2% less, significant at the 10% level. These differences emerge only after the change of ownership: for contracts held four years before the deal, none of the differences is significant, and all are close to zero. When we instead weight every contract equally, so that targets with many contracts dominate the estimates, the targets' contracts are modified about as often as those of their matched contractors, and their final value grows slightly more, by 2.2%, significant only at the 10% level (Table A4).

Panel B repeats the comparison at the firm level. For all targets, the probability that at least one existing contract receives a time extension in a given year falls by 1.5 percentage points relative to matched contractors, and the probability of a funding increase falls by 2.6 percentage points, both significant at the 1% level. The estimates for funding increases are already jointly significant before the deal ($p = 0.002$), however, so this decline should be interpreted with caution.

Taken together, sponsors do not appear to obtain more favorable terms on the contracts their targets already hold. Under every measure, these contracts receive time extensions and funding increases no more often than those of comparable contractors, and less often when each target receives the same weight. This also helps explain why targets tend to keep work awarded without competition (Section 4.4). If sponsors kept this work to exploit it, their targets' contracts should receive more favorable terms. They do not, which suggests that targets keep non-competed work because of their capabilities and relationships with agencies rather than for rents.

Overall, private equity owners do not appear to treat government work as a source of rents. They do not expand their targets' government business or obtain more favorable terms on the

contracts their targets already hold, and the firms whose government business declines most relative to comparable contractors are those with the most favorable access to it. In the next section, we ask how the targets and their sponsors fare after the deal.

5. Post-Deal Growth and Sponsor Returns

Section 4 shows that private equity sponsors do not expand their targets' government business and that small-business contractors scale down theirs more than comparable contractors. In this section, we ask how the targets and their sponsors fare. We address two questions. First, does scaling down government business hurt the targets or their sponsors? Second, do contractor deals earn excess returns? Under the rent-extraction view, government work is a source of excess profits, so scaling it down should slow the targets' growth, and contractor deals should earn higher returns than comparable buyouts. Under the springboard view, government work helped these firms establish themselves as reliable suppliers, and this record helps them win other business. Scaling down government work should therefore not hurt the targets, and because government work earns no excess margins, contractor deals should earn returns similar to those of non-contractor deals.

5.1. Post-Deal Growth

We begin with the post-deal growth of the targets. Measuring the performance of buyout targets is difficult, because most targets are private firms that do not disclose their financial statements. We therefore use deal-level data from Burgiss, which are sourced from the records of the limited partners that invest in buyout funds and report the target's revenue, EBITDA, and enterprise value at entry and at exit. Table 8 uses these data to compare the growth of contractor targets with that of buyouts of firms that are not federal contractors. The dependent variables are the change in the logarithm of revenue, the growth of EBITDA, and the change in the logarithm of enterprise value between entry and exit. Because EBITDA can be negative, we measure its growth as the change divided by the average of its entry and exit values, and we also report the change in the logarithm of EBITDA for targets with positive EBITDA at entry and exit. We compare small-business contractor targets and other contractor targets with non-contractor buyouts within sponsor and within sector-year, so that the estimates are not driven by differences in sponsors' skill or in industry conditions.

The firms whose government business declines most do not suffer for it. Panel A shows that small-business contractor targets grow their revenue at the same rate as non-contractor buyouts

by the same sponsor and grow their profits and value faster: their EBITDA grows by about 14 percentage points more over the holding period, and their enterprise value by about 21 log points more, both significant at the 1% level. For other contractor targets, the growth of EBITDA and enterprise value does not differ significantly from that of non-contractor buyouts (the change in log EBITDA, for targets with positive EBITDA at entry and exit, is 0.12 lower, significant only at the 10% level), which suggests that sponsors create as much value in these firms as in their other buyouts. Their revenue grows by about 19 log points less, significant at the 1% level, but this slower growth does not translate into lower profits or value. Panel B repeats the analysis using deal-level data from StepStone. Small-business contractor targets again grow their revenue at the same rate as non-contractor buyouts, whereas other contractor targets grow it by about 13 log points less, significant at the 1% level. The estimates for the EBITDA and enterprise value of small-business contractor targets are also positive, although not statistically significant.

Scaling down government business therefore does not hurt the targets' profits or value. This is the pattern the springboard view predicts: government work helped small-business contractors establish themselves as reliable suppliers, and once they are acquired by a sponsor, they can grow with other customers. Under the rent-extraction view, by contrast, the firms that give up the most government work would give up the most rents, and their growth should suffer.

5.2. Sponsor Returns and Entry Valuations

We next ask whether contractor deals earn higher returns. If government work offered excess margins, contractor deals should earn higher returns than comparable buyouts of firms without government business. Under the springboard view, contractor deals should earn returns similar to those of non-contractor deals.

We measure returns in three ways: the internal rate of return; the total value multiple, the ratio of what the sponsor receives to what it invests; and the PME, which discounts the deal's cash flows by the returns of the size- and industry-matched index over the same period, so that a PME above one means the deal outperformed a public investment with the same timing and similar risk (Kaplan and Schoar, 2005; Gredil, Griffiths, and Stucke, 2023). Figure 2 compares the distribution of returns on contractor deals with two benchmarks. The left panels compare contractor deals with the other deals of the same funds, and the right panels compare them with other buyouts that appear in PitchBook. Against the other deals of the same funds, contractor deals appear to perform well, with an average internal rate of return of 19% compared with 12%. This difference, however,

reflects the coverage of our data rather than government work. Deals covered by PitchBook earn higher returns than buyouts in general (Table A5), and contractor deals are by construction covered by PitchBook. Once we hold coverage constant, contractor deals and other buyouts earn the same returns: both have an average internal rate of return of 19%, their average total value multiples are 2.56 and 2.55, and their average PMEs are 1.67 and 1.70. The difference in average internal rates of return is −0.1 percentage points, and its 95% confidence interval rules out an excess return of more than 3.9 percentage points, with standard errors clustered by sponsor.[7]

The evidence in Figure 2 thus suggests that contractor deals earn no excess returns, which is difficult to reconcile with the rent-extraction view. Nor do sponsors earn less on these deals, so the decline in their targets' government business does not reduce their returns.

A possible concern is that sponsors pay more for contractors, so that their returns are ordinary even if government work earns rents. Figure 3 compares the operating margin and the ratio of enterprise value to revenue at entry for contractor targets and other buyouts in PitchBook. Contractor targets are bought at somewhat higher margins, 20.5% compared with 19.5% on average, and at somewhat higher valuations, 3.0 times revenue compared with 2.6. These raw comparisons, however, do not account for differences in industry, region, and timing, which affect both margins and valuations.

Table 9 therefore compares each target with buyouts in the same industry, region, and year. If sponsors paid for rents on government work, valuations should increase with the volume of a firm's government work. To test this, we express each target's operating margin and ratio of enterprise value to revenue at entry as a percentile rank among buyout targets in the same industry, region, and year. We then regress these ranks on indicators for contractor targets grouped by the dollar value (Panel A) or the number (Panel B) of their contracts in the two years before the deal. All specifications include sponsor-by-vintage fixed effects, and columns 2 to 4 add controls for the size of the target and of the investment.

We find no evidence that sponsors pay more for firms with more government business. Most groups of contractors enter at margins and valuations similar to those of other targets. The exception is the heaviest contractors, which sponsors buy at lower valuations. Targets with $1 million or more in contracts enter at valuations about 5 percentile points lower than other targets (Panel A, significant at the 10% level in columns 5 to 7 and at the 5% level in column 8), and targets with

[7] The differences are computed on the returns as plotted in Figure 2, winsorized at the 99th percentile of the pooled sample, without fixed effects: −0.1 percentage points for the internal rate of return (standard error 2.0; 95% confidence interval −4.0 to 3.9), 0.010 for the total value multiple (0.092), and −0.024 for the PME (0.064).

40 or more contracts enter at valuations about 6 to 8 points lower (significant at the 5% level in columns 5 and 7 and at the 10% level in columns 6 and 8) and at margins about 6 to 7 points lower (significant at the 10% level; Panel B). The ordinary returns on contractor deals therefore do not reflect higher purchase prices.

The lower margins of the heaviest contractors raise a further question: do they signal weak firms, or the competitive pricing of government work? Panel C of Table 9 addresses this question by relating deal returns to the target's margin at entry. We include indicators for targets in the bottom and top quartiles of the entry margin rank, together with their interactions with the contractor indicators of Panel B, as well as sponsor, year-by-sector, and investment-size fixed effects. Across all buyouts, low entry margins predict lower returns, consistent with weaker businesses. For example, deals bought at a low margin earn an internal rate of return that is 0.061 log points lower than other deals, significant at the 1% level, and the estimates for the total value multiple and the PME are similar.

For the heaviest contractors, this relation disappears. Among targets with 40 or more contracts, the interactions with the low-margin indicator are positive and large enough to offset the main effect for all three return measures, and deals bought at low and at high margins earn similar returns. Targets with four or fewer contracts show broadly similar patterns. A low margin at entry thus signals a weak business for most buyout targets, but not for the heaviest contractors. This is consistent with the springboard view that the government keeps margins thin, so that a low margin reflects the pricing of government work rather than the quality of the firm.

6. Political Connections and Sponsor Experience

The rent-extraction view gives political connections a central role. If government contracts offered rents, politically connected sponsors would be best placed to capture them, by expanding their targets' government business and earning higher returns on contractor deals. Similarly, sponsors that learned how to expand government business in earlier deals should do so again and be rewarded for it. We examine these predictions using Table 10, which collects the evidence on sponsors' government contracting, and Table 11, which examines their returns.

6.1. Do Connected and Experienced Sponsors Expand Government Business?

We begin by asking which sponsors buy contractors. If political connections helped sponsors extract value from government work, connected sponsors would be more likely to buy contractors. To test this, Panel A of Table 10 examines about 76,000 deals by sponsors that complete at least one buyout of a federal contractor and asks whether the target is an active federal contractor in the deal year, which is the case in about 4% of these deals. We find that political, federal-agency, and congressional connections do not predict contractor deals, either individually or jointly ($p = 0.66$, column 1). Only the indicator for agency connections becomes significant, at about half a percentage point, once we control for the sponsor's deal activity, fundraising, and fund size (columns 2 and 3); the three indicators remain jointly insignificant. Connected sponsors are therefore no more likely to seek out contractors, which is difficult to reconcile with the view that connections help sponsors extract value from government work.

Even if connected sponsors are no more likely to buy contractors, they could still use their connections to grow the government business of the contractors they do buy. Panel B of Table 10 examines this possibility. It repeats the analysis of Table 5 separately for the targets of connected and other sponsors and reports, for each group of targets, the average difference-in-differences estimate from the deal year through the seventh year after the deal. As in Table 5, the government business of all groups declines after the deal as contracting spells end. A negative estimate indicates that the targets scale down their government business more than their matched contractors, and a positive estimate that they scale it down less.

Political connections do not help sponsors expand their targets' government business. Across all targets (columns 1 and 2), the government business of connected sponsors' targets declines only slightly more than that of their matched contractors, by 0.29 percentile points in contract counts and 1.01 points in contract dollars. The estimates for the targets of other sponsors are similar, 1.04 and 0.37 points, and the differences between the two groups are not statistically significant ($p = 0.14$ and $p = 0.20$).

The comparison within each group of targets is more revealing. Among small-business contractors (columns 3 and 4), the targets of connected sponsors scale down their government business more than those of other sponsors, particularly in contract dollars (4.76 compared with 1.08 percentile points, a difference significant at the 1% level), although the estimates for connected sponsors' small-business contractors before the deal are close to jointly significant ($p = 0.057$). Connected

sponsors thus reduce their small-business contractors' reliance on protected work more than other sponsors do. Among other targets (columns 5 and 6), the government business of connected sponsors' targets declines somewhat less than that of their matched contractors, as it does for the targets of other sponsors. Connected sponsors therefore do not use their connections to keep the access to protected work that a rent-seeking owner would value most. Their other targets do scale down their government business slightly less than those of other sponsors, but, as we show below, connected sponsors earn lower returns on contractor deals.

Connected sponsors also do not obtain more favorable terms on their targets' existing contracts. If renegotiation were a source of rents, politically connected sponsors would be best placed to obtain favorable terms (Brogaard, Denes, and Duchin, 2021). Yet Panel C of Table 10 shows that the contracts of their targets receive time extensions no more often than those of matched contractors, and funding increases less often, by 1.7 percentage points, significant at the 1% level.

We next turn to sponsors' experience with contractors. Panel D of Table 10 classifies sponsors by their experience with earlier contractor deals, made three to seven years before the current deal. We distinguish three groups: sponsors whose earlier targets grew or maintained the dollar value of their federal contracts, sponsors whose earlier targets scaled it down, and sponsors with no earlier contractor deal. If expanding government business were profitable, sponsors in the first group would do so again.

Instead, the targets of experienced sponsors scale down their government business more than their matched contractors, whatever happened to their earlier targets: by 1.40 percentile points in contract counts for sponsors whose earlier targets grew or maintained it, and by 2.24 points for sponsors whose earlier targets scaled it down. Only the targets of sponsors new to contractors scale down their government business less than their matched contractors, by 0.90 percentile points. These results suggest that sponsors learn about government work and scale it down once they have experience with contractors.

6.2. Do Connected and Experienced Sponsors Earn More?

Table 11 turns to returns. It asks whether contractor deals earn higher returns, and whether this relation differs for politically connected sponsors and for sponsors with experience in government contracting. We regress the logarithms of the three return measures on an indicator for contractor deals and its interactions with indicators for connected sponsors and with the sponsor's record, defined as in Panel D of Table 10. All specifications control for deals in PitchBook and include

year-by-sector fixed effects. The coefficient on the contractor indicator, which here measures the return difference for contractor deals of unconnected sponsors without earlier contractor deals, is small and insignificant in all columns, consistent with the absence of a return difference in Figure 2. Deals of small-business contractors, whose government business declines most relative to comparable contractors, also earn no less than other contractor deals.

Political connections also do not raise returns on contractor deals. Politically connected sponsors earn a higher internal rate of return on their other deals, by 0.143 log points, but not on contractor deals. On these deals, the interaction of the contractor indicator with political connections is −0.258, more than offsetting this advantage, so connected sponsors earn lower returns on contractor deals than on their other deals. The estimates for the other two return measures are also negative, though insignificant.

Experience with contractors does not raise returns either. Table 11 shows that sponsors whose earlier targets grew or maintained their government business earn significantly lower returns on their current contractor deals than on their other deals, relative to sponsors without earlier contractor deals, by between 0.17 and 0.33 log points depending on the measure, all significant at the 5% level. Compared with the contractor deals of sponsors without such a record, their contractor deals earn about the same (−0.05 to −0.08 log points, insignificant). Because each sponsor's record is set before the current deal, these lower returns do not reflect current deals that went badly and lost contracts as a consequence.

Taken together, these results are consistent with sponsors learning from experience that government work offers no rents. Sponsors that maintained their earlier targets' government business earn lower returns on their contractor deals than on their other deals, and experienced sponsors scale down the government business of their current targets more than comparable contractors. This is the opposite of what the rent-extraction view predicts.

Overall, our evidence suggests that government contracting offers no rents to the firms that private equity acquires. Sponsors do not expand their targets' government business, do not obtain more favorable terms on the contracts their targets already hold, do not pay more for firms with more government business, and earn no excess returns on contractor deals. Small-business contractors, whose government business declines most relative to comparable contractors, grow at least as fast as other buyouts, so scaling down government work does not hurt these firms or their sponsors.

7. Conclusion

Concerns that much of federal spending on contracts is wasted are widespread, but whether this waste takes the form of rents for the firms that sell to the government remains an open question. We study this question using private equity buyouts of federal contractors, since buyout sponsors are the owners most likely to exploit such rents if they exist. We find that sponsors seek out active contractors but do not use their ownership to expand government work. After the deal, the government business of targets declines much like that of matched firms that were not acquired. The targets' existing contracts also receive funding increases and time extensions no more often than comparable contracts, the targets of politically connected sponsors fare no better, and contractor deals earn the same returns as other buyouts.

Small-business contractors, which have the most favorable access to government work, scale it down more than comparable contractors. This retreat does not hurt these firms, which grow at least as fast as other buyouts. In effect, they graduate from protected work once a sponsor provides capital and management. This pattern suggests that government work serves as a springboard that helps firms establish themselves as reliable suppliers, and that small-business programs serve their intended purpose.

Our results also highlight a measurement issue in studying government contracting. Because federal contracting occurs in spells and acquisitions select firms during active spells, comparing targets with all contractors attributes the ordinary end of these spells to the buyout and, in our setting, overstates its effect by a factor of about ten. Studies of how government business responds to such events therefore need to compare firms at the same point in their contracting spells.

Finally, our findings bear on the debate about government spending efficiency. They do not imply that federal procurement is free of waste. They suggest, however, that private mid-market firms like those private equity acquires, which make up most federal contractors, do not earn excess profits on government work. Thus, savings are unlikely to come from squeezing contractor margins, which may drive firms out of government work.

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Table 1
Sample Description

This table describes the deals, sponsors, and federal contractors in the sample. Panel A reports, by deal type, the age of the target (years), the disclosed deal size (millions of US dollars), and the number of new federal contracts won by the target over the three years before the deal, among targets with at least one; N is the number of deals with the variable observed. The deals are the completed PitchBook deals of Table 4 (deal years 2011 to 2026, so that the three years before the deal fall within the years in which the procurement records report the contracting officer's size determination, which start in fiscal year 2008). Panel B describes the 1,223 buyout sponsors, averaging each variable over the sponsor's deals: the number of deals the sponsor completes in the year, the number and share of them that involve a federal contractor (a target active in federal contracting in the deal year, as in Table 10, Panel A), the number of buyout and debt funds raised in the deal year or the four years before and their average size (billions of US dollars), and indicators for sponsors with political, federal-agency, and congressional connections, measured from the biographies of their professionals. Panel C compares federal contractors acquired in a PitchBook deal at some point with contractors that are never acquired, on each firm's average annual contracting over its years in the procurement data (fiscal years 2003 to 2025): new contracts and their initial value, the value of modifications, the initial duration of new contracts, the size of modifications relative to the initial value and the change in duration of modified contracts (both winsorized at the 1st and 99th percentiles), and the years since the firm's first contract. Contract counts are contracts per year, dollar values are millions of US dollars per year, and durations are in months; years without a new contract enter the firm's averages as zero, so the average initial duration is not the duration of a typical contract. Panel D compares the established buyout targets in the year before the deal with never-acquired contractors in the same years, reweighted to the targets' distribution of years; t-statistics are based on the two groups' standard errors. Small-business contractors are targets that won more than half of their size-determined contracts in the three fiscal years before the deal year as small businesses.

Panel A: PitchBook deals

	Target age (years)				Deal size (millions of US dollars)				New federal contracts, three years before the deal (targets with any)			
	N	Mean	25th pct.	75th pct.	N	Mean	25th pct.	75th pct.	N	Mean	25th pct.	75th pct.
Buyouts	53,814	29.06	13.0	37.0	7,316	618.01	30.2	500.0	3,734	62.49	2.0	18.0
Growth equity	11,059	20.48	7.0	26.0	4,214	147.85	9.2	105.0	774	263.67	3.0	19.0
Venture capital	101,210	4.48	2.0	6.0	85,874	22.03	1.8	15.3	2,415	9.69	1.0	6.0
M&A	9,012	22.67	7.0	29.0	5,121	995.97	39.9	533.0	2,003	97.75	2.0	17.0
PIPEs	2,536	26.97	9.0	29.0	2,077	226.37	12.1	180.0	247	43.35	2.0	31.0
IPOs	123	10.57	4.0	13.0	125	287.70	70.0	217.5	12	25.00	7.0	28.0

Panel B: Buyout sponsors

	Mean	SD	10th pct.	25th pct.	Median	75th pct.	90th pct.
Deals completed in the year	7.32	10.00	1.00	2.00	4.17	8.23	15.79
Deals involving a federal contractor in the year	0.38	0.51	0.00	0.00	0.17	0.67	1.00
Share of deals involving a federal contractor	0.09	0.19	0.00	0.00	0.02	0.09	0.25
Funds raised over the preceding five years	2.02	2.49	1.00	1.00	1.25	2.00	3.36
Average fund size (billions of US dollars)	0.65	1.06	0.03	0.10	0.28	0.66	1.64
Politically connected sponsor	0.16						
Sponsor connected to a federal agency	0.14						
Sponsor connected to Congress	0.08						

Table 1
Sample Description (continued)

Panel C: Acquired and never-acquired contractors

	Acquired in a PitchBook deal (9,977 firms)						Never acquired (524,244 firms)					
	Mean	SD	5th	10th	90th	95th	Mean	SD	5th	10th	90th	95th
New contracts per year	12.53	232.13	0.2	0.2	7.2	15.9	2.95	129.58	0.1	0.1	2.6	5.6
Initial value of new contracts per year ($ millions)	0.73	4.97	0.0	0.0	0.9	2.6	0.22	6.92	0.0	0.0	0.1	0.4
Value of modifications per year ($ millions)	0.09	1.44	0.0	0.0	0.1	0.3	0.02	0.65	0.0	0.0	0.0	0.0
Initial duration of new contracts (months)	3.10	3.45	0.1	0.2	7.4	9.4	1.79	4.48	0.0	0.0	4.8	7.0
Modification relative to initial value, modified contracts	0.09	0.25	−0.1	−0.1	0.3	0.6	0.03	0.16	−0.1	0.0	0.1	0.3
Change in duration, modified contracts (months)	1.35	2.99	−2.8	−1.0	5.4	7.8	0.50	2.08	−1.7	−0.3	2.5	4.3
Years since first contract	6.11	3.66	1.2	1.5	12.0	13.0	3.59	2.91	1.2	1.2	8.0	10.1

Panel D: Established buyout targets in the year before the deal

	Buyout targets (1)	Never-acquired contractors in the same years (2)	Difference (3)	t (4)
Won a new contract in the two preceding years	0.794	0.587	0.207	30.6
In a contracting spell of seven years or more	0.219	0.086	0.133	19.2
In the top third by contract dollars over the preceding five years	0.577	0.330	0.247	29.9
Small-business contractor	0.556	0.518	0.039	4.7
Firms	3,587	468,752		

Table 2
Federal Contracting Spells

This table describes how federal contracting spells evolve among contractors that are never acquired. The unit of observation is the firm-year, for fiscal years 2008 to 2022 (so that the three following years are observed). Firms are classified by their status in year y: in a spell of consecutive years with new federal awards of the stated length, or idle (no new award) for the stated number of years. Columns 1 and 6 report the share of firm-years in each status. Columns 2–3 and 7–8 report the probability that the firm wins no new award in the following year and in any of the following three years. Columns 4–5 and 9–10 report the probability that it wins a new award in each of the following three years and in at least one of them. All entries are in percent. Columns 1–5 include all never-acquired contractors; columns 6–10 include contractors whose award dollars come mainly from the Department of Defense.

State in year y	All never-acquired contractors					Contractors selling mainly to the Department of Defense				
	Share of firm-years	No new award in		New award in $y+1$ to $y+3$		Share of firm-years	No new award in		New award in $y+1$ to $y+3$	
		$y+1$	$y+1$ to $y+3$	each year	any year		$y+1$	$y+1$ to $y+3$	each year	any year
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
In a spell of 7 or more years	8.8	16.2	10.2	60.0	89.8	10.0	14.6	8.1	63.8	91.9
In a spell of 4 to 6 years	7.7	23.7	14.8	48.0	85.2	7.9	23.6	13.9	49.3	86.1
In a spell of 1 to 3 years	27.1	54.5	40.1	20.6	59.9	26.8	54.1	38.6	20.9	61.4
Idle for 1 to 2 years	35.7	84.8	73.4	3.9	26.6	35.2	83.1	70.6	4.2	29.4
Idle for 3 or more years	20.6	86.9	72.4	1.9	27.6	20.1	85.5	69.8	2.0	30.2
Firm-years	2,855,729					1,046,107				
Firms	440,287					155,777				

Table 3
Choice of Comparison Group

This table reports event-study estimates of the change in a buyout target's federal contracting from the year before the deal (year −1) to each year from −5 to +7, under three comparisons. The outcome is the firm's percentile rank among never-acquired federal contractors by the number of new contracts (columns 1–3) or the dollar value of new contracts (columns 4–6) won in the fiscal year; firms with no new contract are assigned zero. Columns 1 and 4 compare buyout targets with all contractors not yet acquired by the same date. Columns 2 and 5 repeat this comparison for pseudo buyouts: for each target, one never-acquired contractor drawn from the target's matched contractors is assigned the target's deal year; estimates are combined over 20 draws (Rubin's rule). Columns 3 and 6 compare buyout targets with their matched contractors. Matched contractors are never-acquired contractors that in the year before the deal had the same fiscal year, the same range of new contracts (none, 1 to 2, 3 to 9, or 10 or more) in the two preceding years and in the two years before that, the same tercile of contract dollars over the preceding five years, and the same position in a contracting spell or idle period (in a spell of 1 to 3, 4 to 6, or 7 or more years, or idle for 1 to 2 or 3 or more years); they are followed forward from the year before the deal, including those that stop contracting. The sample is the established buyout targets: targets whose first federal contracting activity is three or more years before the deal year. Targets first active later (entrants) and targets that formally changed from small to other than small after the deal are excluded. Event time is measured from the deal year (year 0); year −1 is the reference year. The average over years 0 to +7 weights each year by its number of targets; in columns 1–2 and 4–5 its standard error omits the covariance across years. The joint test is a Wald test that the estimates for years −5 to −2 are zero. Standard errors are in parentheses: within-cohort sampling errors in columns 1–2 and 4–5, clustered by matched group in columns 3 and 6. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

	Contract counts			Contract dollars		
	Buyout targets vs. all contractors (1)	Pseudo buyouts vs. all contractors (2)	Buyout targets vs. matched contractors (3)	Buyout targets vs. all contractors (4)	Pseudo buyouts vs. all contractors (5)	Buyout targets vs. matched contractors (6)
Year −5	0.20	0.32	0.43	−0.22	−0.05	0.31
	(0.74)	(0.79)	(0.48)	(0.77)	(0.86)	(0.53)
Year −4	1.65**	2.76***	0.05	0.73	2.29***	−0.54
	(0.67)	(0.73)	(0.43)	(0.70)	(0.81)	(0.46)
Year −3	4.21***	5.03***	0.04	3.61***	4.65***	−0.52
	(0.94)	(0.97)	(0.39)	(0.98)	(1.05)	(0.44)
Year −2	0.68	1.72**	0.00	0.50	1.72*	−0.22
	(0.86)	(0.87)	(0.36)	(0.89)	(0.94)	(0.39)
Year 0	−1.94***	−2.93***	1.34***	−1.96***	−3.10***	1.43***
	(0.66)	(0.76)	(0.42)	(0.69)	(0.79)	(0.44)
Year +1	−5.60***	−4.10***	−0.77	−5.49***	−4.38***	−0.59
	(0.91)	(1.03)	(0.51)	(0.92)	(0.97)	(0.50)
Year +2	−6.61***	−4.55***	−1.66***	−6.16***	−4.51***	−1.17**
	(0.85)	(0.90)	(0.57)	(0.84)	(0.95)	(0.56)
Year +3	−8.02***	−6.07***	−1.63**	−7.52***	−6.26***	−1.17*
	(0.99)	(1.08)	(0.68)	(0.98)	(1.08)	(0.66)
Year +4	−9.07***	−7.80***	−0.74	−8.76***	−8.27***	−0.55
	(1.17)	(1.37)	(0.78)	(1.19)	(1.34)	(0.81)
Year +5	−10.68***	−9.15***	−1.00	−9.73***	−9.33***	−0.40
	(1.35)	(1.52)	(0.88)	(1.35)	(1.60)	(0.89)
Year +6	−13.18***	−10.52***	−2.25**	−13.06***	−10.52***	−2.50***
	(1.52)	(1.58)	(0.99)	(1.49)	(1.68)	(0.96)
Year +7	−15.15***	−12.35***	−2.96***	−14.56***	−12.08***	−2.75**
	(1.71)	(2.07)	(1.13)	(1.72)	(2.11)	(1.14)
Average, years 0 to +7	−7.15***	−5.88***	−0.84***	−6.84***	−6.03***	−0.58**
	(0.36)	(0.41)	(0.23)	(0.36)	(0.41)	(0.23)
Joint test, −5 to −2 (p)	0.000	0.000	0.937	0.000	0.000	0.500
Targets (year 0)	3,574	3,419	3,578	3,574	3,419	3,578
Observations (year 0)	3,207,140	3,207,140	927,098	3,207,140	3,207,140	927,098

Table 4
Which Contractors Private Equity Buys

This table reports linear probability models of whether a completed PitchBook deal is a buyout. The sample is the completed PitchBook deals of 2011 to 2026 with at least one investor that has ten or more PitchBook deals, including deals for firms that never sold to the government (whose contracting measures are zero). The target's government business is measured over the three years before the deal: the dollar value of its initial awards on which the contracting officer determined it to be other than small (contract dollars outside small-business programs), the number of its initial awards on which it was determined small (contracts won as a small business), both winsorized at the 95th percentile among contractors, and, in column 4, the length in years of its current contracting spell in the year before the deal. Each measure enters as the logarithm of one plus its value, divided by 100, so that a coefficient is the change in the probability, in percentage points, for a one-unit change in the logarithm of one plus the measure. Each is interacted with an indicator for deals with a private equity sponsor (an investor of a financial-sponsor type, with ten or more PitchBook deals and a fund raised in the deal year or the four years before) and with an indicator for deals with other investors only. The deal-history controls are the numbers of the target firm's own PitchBook deals in the five years before the deal, by type (venture capital, buyout, growth equity, recapitalization, M&A, PIPE, and IPO). Employee-count fixed effects use six PitchBook employee bins. Contracting-experience (experience) fixed effects are five levels of the length of the firm's federal contracting history relative to its age. The last rows report the share of deals that are buyouts (the sample mean of the dependent variable, in percent) and the number of deals. t-statistics, based on standard errors clustered by industry and year, are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

	Deal is a buyout			
	(1)	(2)	(3)	(4)
PE-sponsor deal × contract dollars outside small-business programs	0.146	0.260***	0.444***	0.256***
	(1.68)	(3.44)	(5.73)	(3.99)
PE-sponsor deal × contracts won as a small business	3.002***	3.010***	3.097***	1.498**
	(5.52)	(5.71)	(6.11)	(2.86)
PE-sponsor deal × length of current contracting spell				3.588***
				(6.29)
Other investors × contract dollars outside small-business programs	−1.336***	−1.341***	−1.146***	−0.939***
	(−9.35)	(−9.86)	(−8.94)	(−7.24)
Other investors × contracts won as a small business	−4.610***	−5.467***	−5.881***	−4.209***
	(−4.67)	(−5.73)	(−6.36)	(−4.53)
Other investors × length of current contracting spell				−3.450***
				(−5.87)
Firm's earlier private capital deals (5 years)	No	Yes	Yes	Yes
Employee-count fixed effects	No	No	Yes	Yes
Industry-by-year, age, investor-type, and experience fixed effects	Yes	Yes	Yes	Yes
Share of deals that are buyouts (%)	32.0	32.0	32.0	32.0
Observations	187,216	187,216	187,216	187,216

Table 5
Government Contracting after the Buyout

This table reports event-study estimates of the change in federal contracting of buyout targets (Panel A) and growth equity targets (Panel B) relative to their matched contractors, from the year before the deal (year −1) to each year from −5 to +7. The outcome is the firm's percentile rank among never-acquired federal contractors by the number of new contracts (odd columns) or the dollar value of new contracts (even columns) won in the fiscal year; firms with no new contract are assigned zero. Matched contractors are never-acquired contractors that in the year before the deal had the same fiscal year, the same range of new contracts (none, 1 to 2, 3 to 9, or 10 or more) in the two preceding years and in the two years before that, the same tercile of contract dollars over the preceding five years, and the same position in a contracting spell or idle period (in a spell of 1 to 3, 4 to 6, or 7 or more years, or idle for 1 to 2 or 3 or more years); they are followed forward from the year before the deal, including those that stop contracting. Columns 1–2 include all targets, columns 3–4 small-business contractors, and columns 5–6 other targets. Small-business contractors are targets that won more than half of their size-determined contracts in the three fiscal years before the deal year as small businesses; other targets are the remaining established targets. The rows below the estimates report the average effect over years 0 to +7 (each year weighted by its number of targets); the p-value of the joint test that the estimates for years −5 to −2 are zero; and the change since year −1 at year +7 for targets and for matched contractors separately. The sample is the established buyout targets: targets whose first federal contracting activity is three or more years before the deal year. Targets first active later (entrants) and targets that formally changed from small to other than small after the deal are excluded. The block *Including targets that changed from small to other than small* repeats the average effect and the joint test with the established targets that formally changed from small to other than small after the deal added back (108 targets, 107 of them observed in year 0, of which 95 are small-business contractors). Growth equity targets are restricted in the same way as buyout targets. Panel B reports all growth equity targets only, without the split by small-business status. Event time is measured from the deal year (year 0); year −1 is the reference year. Standard errors, clustered by matched group, are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

	All targets		Small-business contractors		Other targets	
	Counts (1)	Dollars (2)	Counts (3)	Dollars (4)	Counts (5)	Dollars (6)
Panel A: Buyout targets						
Year −5	0.43	0.31	−0.27	−0.28	1.32*	1.08
	(0.48)	(0.53)	(0.66)	(0.71)	(0.70)	(0.78)
Year −4	0.05	−0.54	−0.29	−0.94	0.47	−0.04
	(0.43)	(0.46)	(0.60)	(0.64)	(0.61)	(0.67)
Year −3	0.04	−0.52	−0.05	−0.73	0.15	−0.26
	(0.39)	(0.44)	(0.59)	(0.65)	(0.48)	(0.55)
Year −2	0.00	−0.22	−0.23	−0.33	0.28	−0.10
	(0.36)	(0.39)	(0.53)	(0.57)	(0.47)	(0.50)
Year 0	1.34***	1.43***	0.56	0.85	2.37***	2.20***
	(0.42)	(0.44)	(0.58)	(0.62)	(0.57)	(0.62)
Year +1	−0.77	−0.59	−2.81***	−1.94***	1.83***	1.17*
	(0.51)	(0.50)	(0.70)	(0.70)	(0.71)	(0.69)
Year +2	−1.66***	−1.17**	−4.36***	−3.19***	1.77**	1.41*
	(0.57)	(0.56)	(0.78)	(0.79)	(0.80)	(0.77)
Year +3	−1.63**	−1.17*	−5.08***	−4.21***	2.78***	2.71***
	(0.68)	(0.66)	(0.91)	(0.92)	(0.95)	(0.90)
Year +4	−0.74	−0.55	−2.38**	−1.65	1.66	1.08
	(0.78)	(0.81)	(1.03)	(1.09)	(1.11)	(1.11)
Year +5	−1.00	−0.40	−3.07***	−1.97	2.06	1.94
	(0.88)	(0.89)	(1.18)	(1.20)	(1.28)	(1.29)
Year +6	−2.25**	−2.50***	−3.92***	−2.72**	0.18	−2.12
	(0.99)	(0.96)	(1.37)	(1.35)	(1.39)	(1.37)
Year +7	−2.96***	−2.75**	−4.15**	−3.49**	−1.39	−1.75
	(1.13)	(1.14)	(1.62)	(1.61)	(1.62)	(1.67)
Average, years 0 to +7	−0.84***	−0.58**	−2.83***	−2.01***	1.78***	1.32***
	(0.23)	(0.23)	(0.32)	(0.33)	(0.33)	(0.33)
Joint test, years −5 to −2 (p)	0.937	0.500	0.962	0.427	0.327	0.703
Change since year −1 at year +7						
Buyout targets	−24.38	−24.61	−30.72	−30.09	−16.39	−17.69
Matched contractors	−21.42	−21.86	−26.57	−26.60	−15.00	−15.95
Targets (year 0)	3,578	3,578	1,990	1,990	1,588	1,588
Observations (year 0)	927,098	927,098	543,222	543,222	583,404	583,404
Including targets that changed from small to other than small						
Average, years 0 to +7	−0.91***	−0.64***	−2.82***	−2.00***	1.74***	1.27***
	(0.23)	(0.23)	(0.31)	(0.32)	(0.33)	(0.33)
Joint test, years −5 to −2 (p)	0.990	0.326	0.769	0.184	0.405	0.721
Targets (year 0)	3,685	3,685	2,085	2,085	1,600	1,600

Table 5
Government Contracting after the Buyout (continued)

	All growth equity targets	
	Counts (1)	Dollars (2)
Panel B: Growth equity targets		
Year −5	−2.37*	−0.17
	(1.26)	(1.43)
Year −4	−1.20	−0.47
	(1.09)	(1.17)
Year −3	−2.04**	−1.74
	(1.02)	(1.16)
Year −2	−1.39	−0.89
	(0.91)	(1.01)
Year 0	2.60***	2.52**
	(1.00)	(1.16)
Year +1	3.87***	3.40***
	(1.14)	(1.31)
Year +2	4.65***	5.56***
	(1.19)	(1.43)
Year +3	3.80***	5.81***
	(1.34)	(1.55)
Year +4	4.58***	5.20***
	(1.57)	(1.68)
Year +5	6.11***	5.16***
	(1.80)	(1.97)
Year +6	5.63***	4.58**
	(2.08)	(2.18)
Year +7	5.34**	7.09***
	(2.27)	(2.33)
Average, years 0 to +7	4.31***	4.66***
	(0.50)	(0.57)
Joint test, years −5 to −2 (p)	0.032	0.547
Change since year −1 at year +7		
Growth equity targets	−14.48	−13.50
Matched contractors	−19.81	−20.59
Targets (year 0)	565	565
Observations (year 0)	283,838	283,838

Table 6
Exit from Federal Contracting

This table reports how the probability that a target wins no new federal contract from the deal year through year +4 changes after the deal, relative to never-acquired contractors in the same stratum (treatment year, pre-deal activity cell, and the firm's exact pattern of new awards in each of years −3 to −1, with the pattern active in all three years split by spell length into 3, 4 to 6, and 7 or more years: ten states). These strata are finer than the matched groups of Tables 3 and 5, because the outcome is itself an indicator of new awards. Panel A reports the estimates for buyout targets (columns 1–5) and for growth equity targets (columns 6–10). Panel B repeats the construction for pseudo buyouts: for each target, one never-acquired contractor in the target's stratum assigned the target's deal year. The first row of each panel is the change for all targets. Each following row is estimated on the targets whose type of work before the deal can be classified, and its coefficient is the difference between targets with that type of work and the other classified targets. Types of work are measured over years −8 to −6, outside both the years on which targets are matched to never-acquired contractors (−3 to −1) and the placebo years (−5 and −4), so that they are not constructed from the same awards that define the comparison. A target's type of work requires at least three priced (or competition-coded) initial awards in the window: *entirely fixed-price* if every priced award is firm-fixed-price; *some cost-plus contract dollars* if any award dollars are cost-plus; *mostly awarded without competition* if more than half of its competition-coded awards were not competed; *three or more bidders on average* if its awards drew at least three offers on average; and *small-business contractors* if more than half of its size-determined awards in the window were won as a small business. The placebo columns repeat the construction for years −5 and −4, before the matched pattern. The buyout sample is the established buyout targets, excluding targets that formally changed from small to other than small after the deal; growth equity targets are restricted in the same way. Targets is the number of targets in each row. *t*-statistics, based on standard errors clustered by firm and stratum, are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

	Buyout targets					Growth equity targets				
	Years 0 to +4			Placebo, years −5 and −4		Years 0 to +4			Placebo, years −5 and −4	
	Estimate (1)	*t* (2)	Targets (3)	Estimate (4)	*t* (5)	Estimate (6)	*t* (7)	Targets (8)	Estimate (9)	*t* (10)
Panel A: Targets										
All targets	0.003	(0.45)	3,568	−0.007**	(−2.20)	−0.059***	(−4.32)	506	−0.005	(−0.59)
Entirely fixed-price contracts	0.009	(0.55)	1,208	−0.013	(−1.50)	−0.059	(−1.55)	154	0.011	(0.49)
Some cost-plus contract dollars	−0.031	(−1.38)	203	0.013	(1.02)	0.125**	(2.32)	39	−0.001	(−0.06)
Mostly awarded without competition	−0.027*	(−1.73)	852	−0.004	(−0.45)	−0.059	(−1.52)	109	0.026	(1.06)
Three or more bidders on average	0.023	(1.25)	426	0.004	(0.38)	0.032	(0.77)	68	0.004	(0.14)
Fixed-price contracts with three or more bidders	0.054**	(2.01)	204	−0.004	(−0.33)	−0.028	(−0.44)	27	−0.040	(−0.89)
Small-business contractors	0.045***	(3.10)	1,524	−0.006	(−0.67)	−0.022	(−0.50)	212	−0.045	(−1.44)
Panel B: Pseudo buyouts										
All pseudo buyouts	−0.016***	(−3.07)	3,281	−0.004	(−1.45)	−0.025*	(−1.76)	462	−0.005	(−0.60)
Entirely fixed-price contracts	−0.020	(−1.37)	1,084	−0.001	(−0.10)	−0.034	(−0.86)	145	−0.009	(−0.36)
Some cost-plus contract dollars	−0.017	(−0.85)	216	0.009	(0.58)	0.080	(1.40)	25	−0.022	(−0.63)
Mostly awarded without competition	0.005	(0.29)	721	−0.003	(−0.33)	0.055	(1.35)	97	−0.041*	(−1.68)
Three or more bidders on average	−0.018	(−1.21)	498	−0.001	(−0.06)	−0.001	(−0.02)	72	−0.003	(−0.13)
Fixed-price contracts with three or more bidders	−0.002	(−0.11)	263	0.008	(0.62)	−0.001	(−0.01)	40	−0.002	(−0.06)
Small-business contractors	−0.007	(−0.38)	1,612	−0.015	(−1.35)	0.026	(0.65)	219	−0.020	(−0.71)

Table 7
Contract Modifications

This table compares how the federal contracts of buyout targets and of their matched contractors are modified after the deal. *Time extension* indicates a modification that extends the contract's period of performance, *funding increase* a modification that increases its obligations, and the *log of final to initial obligations* measures how much the contract's value grows. Panel A is at the contract level: one row per contract that a target or a matched contractor of its stratum (up to 50 per stratum, reweighted to the targets' shares) held at the deal, that is, awarded before the deal year with a period of performance running into it; outcomes are measured over years 0 to +2. Each contract is weighted by the inverse of the number of contracts its firm holds, so that every target receives the same total weight. Estimates are from OLS regressions on a target indicator with stratum-by-award-year fixed effects; the placebo columns repeat the construction for the contracts held four years earlier (outcomes over years −4 to −2). Probabilities are in units of probability (0.01 is one percentage point); t-statistics, clustered by stratum, are in parentheses; the contract counts give target contracts in brackets. Panel B is at the firm-year level: the outcomes are indicators that, in the year, a modification of one of the firm's existing contracts extends its period of performance or adds obligations to it, built from every action in the USAspending archives (defined from 2008, so the 2008 deal cohort has no base year); the row reports the average change over years 0 to +7 relative to matched contractors, in percentage points, with its standard error clustered by matched group in parentheses, the p-value of the joint test that the estimates for years −5 to −2 are zero, and the number of targets. Matched contractors are never-acquired contractors that in the year before the deal had the same fiscal year, the same range of new contracts (none, 1 to 2, 3 to 9, or 10 or more) in the two preceding years and in the two years before that, the same tercile of contract dollars over the preceding five years, and the same position in a contracting spell or idle period (in a spell of 1 to 3, 4 to 6, or 7 or more years, or idle for 1 to 2 or 3 or more years); they are followed forward from the year before the deal, including those that stop contracting. The sample is the established buyout targets: targets whose first federal contracting activity is three or more years before the deal year. Targets first active later (entrants) and targets that formally changed from small to other than small after the deal are excluded. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

Panel A: Contracts held at the deal (contract level)

	Years 0 to +2		Placebo: years −4 to −2	
	Estimate (1)	Contracts (2)	Estimate (3)	Contracts (4)
Time extension	−0.021**	128,741	−0.004	109,231
	(−2.10)	[9,268]	(−0.38)	[7,818]
Funding increase	−0.016*	128,741	−0.009	109,231
	(−1.65)	[9,268]	(−0.90)	[7,818]
Log of final to initial obligations	−0.018*	122,792	−0.004	105,514
	(−1.89)	[9,034]	(−0.36)	[7,639]
Targets with contracts held at the deal		1,526		

Panel B: All targets (firm-year level)

	Time extension			Funding increase		
	Average, years 0 to +7 (1)	Pre-deal p (2)	Targets (3)	Average, years 0 to +7 (4)	Pre-deal p (5)	Targets (6)
All targets	−1.53***	0.525	3,487	−2.56***	0.002	3,487
	(0.40)			(0.38)		

Table 8
Post-Deal Growth

This table compares the growth over the holding period of buyouts of federal contractors with that of non-contractor buyouts. The dependent variables, between entry and exit, are the change in log revenue (column 1); the growth of EBITDA measured as the change divided by the average of its entry and exit values, which is defined when EBITDA is negative and bounded by ±2 (column 2); the change in log EBITDA, for deals with positive EBITDA at entry and exit, winsorized at the 1st and 99th percentiles (column 3); and the change in log enterprise value (column 4). The rows are indicators for small-business contractor targets and other contractor targets of the established sample; non-contractor buyouts are the omitted group. Small-business contractors are targets that won more than half of their size-determined contracts in the three fiscal years before the deal year as small businesses; other targets are the remaining established targets. Contractor targets whose first federal contracting activity is two or fewer years before the deal (entrants) and contractor deals outside the event-study sample carry their own indicators, which are not reported, so that neither enters the omitted group. All regressions include sponsor and year-by-sector fixed effects. Panel A uses Burgiss and Panel B StepStone deal-level data. *t*-statistics, based on standard errors clustered by sponsor, are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

Panel A: Burgiss

	(1) Change in log revenue	(2) EBITDA growth (midpoint)	(3) Change in log EBITDA	(4) Change in log enterprise value
Small-business contractor targets	−0.001	0.144***	0.114*	0.212***
	(−0.03)	(2.78)	(1.91)	(2.60)
Other contractor targets	−0.190***	−0.072	−0.121*	−0.027
	(−4.62)	(−1.29)	(−1.95)	(−0.38)
Small-business minus other targets	0.189	0.216	0.235	0.238
t-statistic	3.28	3.00	2.87	2.32
Fixed effects		Sponsor and year-by-sector		
Deals: small-business contractor targets	295	310	293	202
Deals: other contractor targets	237	253	238	138
Deals: entrant contractor targets (not reported)	132	139	134	80
Deals: other contractor deals (not reported)	335	340	318	205
Deals: non-contractor buyouts	9,041	8,981	7,663	5,613
Observations	10,040	10,023	8,646	6,238

Panel B: StepStone

	(1) Change in log revenue	(2) EBITDA growth (midpoint)	(3) Change in log EBITDA	(4) Change in log enterprise value
Small-business contractor targets	0.005	0.091	0.017	0.096
	(0.10)	(1.41)	(0.25)	(1.35)
Other contractor targets	−0.134***	0.003	−0.047	−0.003
	(−3.18)	(0.05)	(−0.65)	(−0.04)
Small-business minus other targets	0.139	0.088	0.065	0.099
t-statistic	2.02	0.99	0.63	1.04
Fixed effects		Sponsor and year-by-sector		
Deals: small-business contractor targets	156	160	158	151
Deals: other contractor targets	127	127	122	129
Deals: entrant contractor targets (not reported)	66	67	64	65
Deals: other contractor deals (not reported)	131	130	119	129
Deals: non-contractor buyouts	4,941	4,875	4,359	4,990
Observations	5,421	5,359	4,822	5,464

Table 9
Entry Margins, Valuations, and Returns

This table compares the operating margins, valuations, and returns of contractor targets with those of other buyouts. In Panels A and B, the dependent variables are the target's operating margin at entry (columns 1–4) and its ratio of enterprise value to revenue at entry (columns 5–8), each expressed as a percentile rank (0 to 100) among buyouts in the same industry (GICS sub-industry), region, and deal year. The regressors are indicators for contractor targets grouped by the dollar value (Panel A) or the number (Panel B) of their federal contracts in the two years before the deal, and an indicator for deals in PitchBook. All columns include sponsor-by-vintage fixed effects; columns 2–4 and 6–8 add fixed effects for quartiles of entry revenue and of the investment. In Panel C, the dependent variables are the deal's returns, $\ln(1.1 + \text{IRR})$, $\ln(0.1 + \text{total value multiple})$, and $\ln(0.1 + \text{PME})$, regressed on the contractor indicators of Panel B, an indicator for deals in PitchBook, indicators for deals bought at a low or a high entry margin (odd columns) or entry valuation (even columns), and their interactions with the contractor indicators. Low and High are the bottom and top quartiles of the entry rank among buyouts in the same industry, region, and year (rank below 25 and above 75). The rows *Low = High* report, for non-contractor deals and for each contractor group, the p-value of the test that deals bought at low and at high entry margins (or valuations) earn the same returns, (Low + Low × group) = (High + High × group). Panel C includes sponsor, year-by-sector, and investment-size quartile fixed effects. The sample is the buyout deals of the Burgiss data. t-statistics, based on standard errors clustered by sponsor, are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

Panel A: Contract dollars in the two years before the deal

	Operating margin at entry				Enterprise value to revenue at entry			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Contract dollars below $100,000	2.611	2.063	2.717	2.086	2.546	1.838	2.642	1.850
	(1.56)	(1.26)	(1.62)	(1.30)	(1.45)	(1.14)	(1.52)	(1.17)
Contract dollars $100,000 to $1 million	1.452	0.850	1.582	0.951	−2.217	−2.229	−2.159	−2.095
	(0.63)	(0.37)	(0.69)	(0.42)	(−0.91)	(−0.93)	(−0.90)	(−0.92)
Contract dollars $1 million or more	−3.123	−3.227	−3.276	−3.496	−5.110*	−5.189*	−5.313*	−5.584**
	(−1.00)	(−1.04)	(−1.07)	(−1.16)	(−1.66)	(−1.74)	(−1.78)	(−1.99)
Deal in PitchBook	−0.777	0.396	−0.907	0.453	−6.078***	−3.909***	−6.119***	−3.646***
	(−0.69)	(0.37)	(−0.83)	(0.44)	(−5.38)	(−3.52)	(−5.40)	(−3.31)
Largest minus smallest group	−5.73	−5.29	−5.99	−5.58	−7.66	−7.03	−7.95	−7.43
Contractor groups equal (p)	0.257	0.312	0.226	0.264	0.029	0.050	0.021	0.030
Observations	10,167	10,167	10,167	10,167	7,804	7,804	7,804	7,804
Within R^2	0.003	0.004	0.003	0.004	0.010	0.008	0.010	0.009
Sponsor-by-vintage fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Entry-revenue quartile fixed effects		Yes		Yes		Yes		Yes
Investment-size quartile fixed effects			Yes	Yes			Yes	Yes

Panel B: Number of contracts in the two years before the deal

	Operating margin at entry				Enterprise value to revenue at entry			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
4 or fewer contracts	1.452	0.965	1.512	0.952	2.415	1.933	2.429	1.873
	(0.80)	(0.55)	(0.83)	(0.55)	(1.21)	(1.03)	(1.22)	(1.01)
5 to 39 contracts	3.552*	2.877	3.564*	2.747	−1.186	−2.050	−1.185	−2.257
	(1.68)	(1.39)	(1.70)	(1.35)	(−0.55)	(−1.04)	(−0.55)	(−1.19)
40 or more contracts	−6.656*	−6.472*	−6.407*	−6.095*	−7.651**	−6.352*	−7.445**	−5.715*
	(−1.74)	(−1.71)	(−1.71)	(−1.66)	(−2.15)	(−1.83)	(−2.11)	(−1.72)
Deal in PitchBook	−0.739	0.419	−0.868	0.476	−6.044***	−3.875***	−6.084***	−3.609***
	(−0.65)	(0.39)	(−0.79)	(0.46)	(−5.35)	(−3.50)	(−5.38)	(−3.28)
Largest minus smallest group	−8.11	−7.44	−7.92	−7.05	−10.07	−8.28	−9.87	−7.59
Contractor groups equal (p)	0.044	0.063	0.053	0.083	0.025	0.054	0.028	0.061
Observations	10,167	10,167	10,167	10,167	7,804	7,804	7,804	7,804
Within R^2	0.003	0.004	0.003	0.005	0.010	0.008	0.010	0.009
Sponsor-by-vintage fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Entry-revenue quartile fixed effects		Yes		Yes		Yes		Yes
Investment-size quartile fixed effects			Yes	Yes			Yes	Yes

Table 9
Entry Margins, Valuations, and Returns (continued)

Panel C: Returns and entry margins and valuations

	IRR		TVM		PME	
	(1)	(2)	(3)	(4)	(5)	(6)
4 or fewer contracts	−0.024	0.012	−0.172***	−0.050	−0.131**	−0.052
	(−0.57)	(0.30)	(−2.60)	(−0.70)	(−2.11)	(−0.76)
5 to 39 contracts	0.020	0.000	0.044	0.027	0.038	0.049
	(0.42)	(0.00)	(0.56)	(0.29)	(0.52)	(0.54)
40 or more contracts	−0.128	−0.133	−0.196	−0.139	−0.144	−0.114
	(−1.35)	(−1.09)	(−1.37)	(−0.93)	(−1.04)	(−0.76)
Deal in PitchBook	0.050**	0.050**	0.173***	0.163***	0.092**	0.092**
	(2.20)	(2.09)	(4.77)	(4.14)	(2.56)	(2.40)
Low entry margin	−0.061***		−0.114***		−0.096***	
	(−2.92)		(−3.33)		(−3.04)	
Low entry margin × 4 or fewer contracts	0.119		0.438***		0.350***	
	(1.59)		(4.17)		(3.53)	
Low entry margin × 5 to 39 contracts	−0.007		0.046		0.107	
	(−0.07)		(0.28)		(0.69)	
Low entry margin × 40 or more contracts	0.336***		0.498**		0.427**	
	(2.81)		(2.38)		(2.26)	
High entry margin	0.001		0.019		0.028	
	(0.03)		(0.76)		(1.25)	
High entry margin × 4 or fewer contracts	0.101*		0.290***		0.209**	
	(1.80)		(3.07)		(2.33)	
High entry margin × 5 to 39 contracts	−0.003		0.000		−0.014	
	(−0.03)		(0.00)		(−0.11)	
High entry margin × 40 or more contracts	0.280**		0.192		0.166	
	(2.31)		(0.83)		(0.73)	
Low entry valuation		0.028		0.109***		0.112***
		(1.33)		(3.25)		(3.53)
Low entry valuation × 4 or fewer contracts		0.037		0.086		0.141
		(0.42)		(0.65)		(1.17)
Low entry valuation × 5 to 39 contracts		−0.088		−0.079		−0.056
		(−0.72)		(−0.43)		(−0.33)
Low entry valuation × 40 or more contracts		0.198		0.328*		0.316
		(1.47)		(1.66)		(1.56)
High entry valuation		−0.021		−0.048*		−0.037
		(−1.27)		(−1.87)		(−1.50)
High entry valuation × 4 or fewer contracts		0.020		0.090		0.067
		(0.32)		(0.81)		(0.68)
High entry valuation × 5 to 39 contracts		0.089		0.167		0.007
		(1.23)		(1.13)		(0.05)
High entry valuation × 40 or more contracts		0.224		−0.213		−0.253
		(1.52)		(−0.74)		(−0.98)
Fixed effects	Sponsor, year-by-sector, and investment-size quartile					
Low = High (p)						
Non-contractor deals	0.007	0.025	0.001	0.000	0.001	0.000
4 or fewer contracts	0.571	0.471	0.900	0.260	0.881	0.070
5 to 39 contracts	0.493	0.287	0.607	0.654	0.985	0.629
40 or more contracts	0.960	0.834	0.489	0.007	0.568	0.006
Observations	10,166	8,100	10,822	8,535	10,821	8,535
R^2	0.087	0.086	0.093	0.100	0.100	0.103
Within R^2	0.004	0.003	0.008	0.008	0.005	0.006

Table 10
Sponsors and Government Contracting

This table collects the evidence on sponsors and government contracting. Panel A reports linear probability models, at the sponsor–deal level, of whether the target is an active federal contractor in the deal year, for the 75,978 deals (2008 to 2024) of sponsors with at least one contractor buyout, ten or more PitchBook deals, and a buyout fund of recorded size raised in the deal year or the four years before. The regressors are indicators for politically connected sponsors, sponsors connected to a federal agency, and sponsors connected to Congress, measured from the biographies of their professionals. Columns 2 and 3 control for the sponsor's deal activity (log of deals in the year), fundraising (log of buyout and debt funds raised in the deal year or the four years before), and average fund size; column 3 adds the numbers of the target firm's own PitchBook deals in the five years before, by type; all columns include indicators for listed sponsors and listed targets and deal-type, target-age, and industry-by-year fixed effects; t-statistics, clustered by industry and year, are in parentheses. The joint test is a Wald test that the three connection indicators are zero; the share of deals involving a contractor is the sample mean of the dependent variable, in percent. Panels B and D report the average change in federal contracting of buyout targets relative to their matched contractors over years 0 to +7, separately by type of sponsor, with the p-value of the joint test that the estimates for years −5 to −2 are zero and the number of targets. The outcome is the firm's percentile rank among never-acquired federal contractors by the number of new contracts (counts) or the dollar value of new contracts (dollars) won in the fiscal year; firms with no new contract are assigned zero. Columns 1–2 include all targets, columns 3–4 small-business contractors, and columns 5–6 other targets. The row *Connected minus not connected* is estimated in one regression that stacks the two groups of targets with their matched contractors, interacts every event-time indicator with an indicator for connected sponsors, and clusters standard errors by matched group; a matched group that contains targets of both connected and other sponsors is kept as one cluster, so that the clustering absorbs the overlap. Panel C reports, for the targets of politically connected sponsors, the average change over years 0 to +7, in percentage points, in the probability that a modification extends the period of performance of an existing contract (time extension) or adds obligations to it (funding increase), relative to matched contractors, as in Table 7, Panel B. Panel D classifies sponsors by the record of their earlier contractor targets, acquired three to seven years before the deal: earlier targets whose federal contract dollars grew or held (the average over those deals of the growth of total obligations from years −2 and −1 to years +2 and +3 of their own deal, measured as the change divided by the average of the two levels, at or above zero; the growth is the targets' own, not relative to their matched contractors), earlier targets whose contract dollars declined, and no earlier contractor deal that can be measured. Matched contractors are never-acquired contractors that in the year before the deal had the same fiscal year, the same range of new contracts (none, 1 to 2, 3 to 9, or 10 or more) in the two preceding years and in the two years before that, the same tercile of contract dollars over the preceding five years, and the same position in a contracting spell or idle period (in a spell of 1 to 3, 4 to 6, or 7 or more years, or idle for 1 to 2 or 3 or more years); they are followed forward from the year before the deal, including those that stop contracting. The sample is the established buyout targets: targets whose first federal contracting activity is three or more years before the deal year. Targets first active later (entrants) and targets that formally changed from small to other than small after the deal are excluded. In Panels B to D, standard errors clustered by matched group are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

Panel A: Which sponsors buy contractors

	Target is an active federal contractor in the deal year		
	(1)	(2)	(3)
Politically connected sponsor	−0.002	−0.002	−0.002
	(−0.73)	(−1.01)	(−0.78)
Sponsor connected to a federal agency	0.003	0.005**	0.005*
	(1.18)	(2.16)	(1.98)
Sponsor connected to Congress	0.001	0.002	0.001
	(0.38)	(0.96)	(0.76)
Sponsor deal activity, fundraising, and fund size	No	Yes	Yes
Listing status of the sponsor and the target	Yes	Yes	Yes
Firm's earlier deals (5 years)	No	No	Yes
Joint test of the three connection indicators (p)	0.664	0.121	0.204
Share of deals involving a contractor (%)	3.8	3.8	3.8
Observations	75,978	75,978	75,978

Table 10
Sponsors and Government Contracting (continued)

Panel B: Government business of the targets of connected and other sponsors

	All targets		Small-business contractors		Other targets	
	Counts (1)	Dollars (2)	Counts (3)	Dollars (4)	Counts (5)	Dollars (6)
Politically connected sponsor						
Average effect, years 0 to +7	−0.29	−1.01**	−3.57***	−4.76***	2.48***	2.14***
	(0.42)	(0.40)	(0.61)	(0.60)	(0.55)	(0.53)
Joint test, years −5 to −2 (p)	0.463	0.188	0.755	0.057	0.370	0.439
Targets (year 0)	1,140	1,140	502	502	638	638
Sponsor not politically connected						
Average effect, years 0 to +7	−1.04***	−0.37	−2.53***	−1.08***	1.35***	0.79*
	(0.28)	(0.29)	(0.37)	(0.38)	(0.42)	(0.42)
Joint test, years −5 to −2 (p)	0.556	0.648	0.981	0.812	0.017	0.676
Targets (year 0)	2,433	2,433	1,485	1,485	948	948
Connected minus not connected						
Difference in the average effect	0.76	−0.64	−1.04	−3.67***	1.12	1.35**
	(0.51)	(0.50)	(0.70)	(0.70)	(0.70)	(0.68)
p-value	0.135	0.199	0.141	0.000	0.110	0.048

Panel C: Contract modifications for the targets of politically connected sponsors

	Time extension			Funding increase		
	Average, years 0 to +7 (1)	Pre-deal p (2)	Targets (3)	Average, years 0 to +7 (4)	Pre-deal p (5)	Targets (6)
Targets of politically connected sponsors	0.52	0.161	1,119	−1.74***	0.219	1,119
	(0.73)			(0.65)		

Panel D: Government business by sponsor record

	All targets		Small-business contractors		Other targets	
	Counts (1)	Dollars (2)	Counts (3)	Dollars (4)	Counts (5)	Dollars (6)
Earlier contractor targets grew or held government business						
Average effect, years 0 to +7	−1.40***	−1.35***	−2.65***	−1.80***	0.65	−0.60
	(0.49)	(0.49)	(0.62)	(0.62)	(0.81)	(0.79)
Joint test, years −5 to −2 (p)	0.578	0.496	0.296	0.440	0.595	0.209
Targets (year 0)	743	743	458	458	285	285
Earlier contractor targets lost government business						
Average effect, years 0 to +7	−2.24***	−1.40***	−4.19***	−3.05***	0.24	0.70
	(0.35)	(0.36)	(0.50)	(0.50)	(0.48)	(0.51)
Joint test, years −5 to −2 (p)	0.710	0.849	0.601	0.420	0.871	0.981
Targets (year 0)	1,440	1,440	812	812	628	628
No record of earlier contractor deals						
Average effect, years 0 to +7	0.90**	0.69*	−1.45***	−1.01*	3.70***	2.71***
	(0.38)	(0.37)	(0.52)	(0.54)	(0.54)	(0.51)
Joint test, years −5 to −2 (p)	0.706	0.844	0.518	0.552	0.356	0.507
Targets (year 0)	1,390	1,390	717	717	673	673

Table 11
Sponsors and the Returns on Contractor Deals

This table reports regressions of deal returns, ln(1.1 + IRR) (columns 1–2), ln(0.1 + total value multiple) (columns 3–4), and ln(0.1 + PME) (columns 5–6), on an indicator for buyouts of federal contractors and its interactions with indicators for politically connected and federal-agency connected sponsors, with indicators for small-business contractors and for contractors that never won a contract as a small business, and (even columns) with the sponsor's record with earlier contractor targets, defined as in Table 10, Panel D. All specifications include an indicator for deals in PitchBook, the log of the sponsor's deals in years −7 to −3, and year-by-sector fixed effects. The sample is the buyout deals of the Burgiss data. A target is a small-business contractor if more than half of its size-determined initial awards in the three fiscal years before the buyout were won as a small business, and a never-small-business contractor if none was and none was set aside; both indicators are zero for targets that cannot be classified, which include most contractor deals. On deals that are not contractor deals, the two indicators are nonzero only for targets that held federal contracts in a different period, so their main terms are not interpreted. The sponsor-record indicators equal one on all PitchBook-matched deals of sponsors with that record, so their interactions with the contractor indicator compare the contractor deals of these sponsors with their other deals, relative to the same comparison for sponsors without earlier contractor deals. Because every indicator is interacted, the coefficient on the contractor indicator is the return difference for contractor deals of unconnected sponsors (in even columns, without earlier contractor deals) whose targets are not classified as small-business or never-small-business contractors. *t*-statistics, based on standard errors clustered by deal and sponsor, are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

	IRR		TVM		PME	
	(1)	(2)	(3)	(4)	(5)	(6)
Contractor deal	0.048	0.057	0.021	0.039	0.007	0.012
	(1.37)	(1.30)	(0.36)	(0.55)	(0.13)	(0.18)
Contractor deal × earlier targets grew or held		−0.169**		−0.325**		−0.270**
		(−2.17)		(−2.35)		(−2.06)
Contractor deal × earlier targets lost		0.019		0.098		0.166
		(0.30)		(0.87)		(1.44)
Earlier contractor targets grew or held		0.122**		0.245**		0.188*
		(2.12)		(2.20)		(1.75)
Earlier contractor targets lost		0.015		−0.066		−0.112
		(0.27)		(−0.65)		(−1.09)
Politically connected sponsor	0.143***	0.116*	0.212	0.193	0.152	0.160
	(2.69)	(1.93)	(0.89)	(0.77)	(0.64)	(0.63)
Agency-connected sponsor	−0.076	−0.119	−0.058	−0.116	−0.013	−0.043
	(−0.70)	(−0.99)	(−0.37)	(−0.64)	(−0.09)	(−0.25)
Politically and agency-connected sponsor	−0.043	−0.008	−0.129	−0.071	−0.109	−0.068
	(−0.34)	(−0.06)	(−0.46)	(−0.24)	(−0.39)	(−0.23)
Contractor deal × politically connected sponsor	−0.258***	−0.241***	−0.338	−0.331	−0.261	−0.285
	(−3.35)	(−2.77)	(−1.33)	(−1.23)	(−1.02)	(−1.05)
Contractor deal × agency-connected sponsor	0.043	0.077	0.051	0.099	0.056	0.072
	(0.33)	(0.54)	(0.29)	(0.51)	(0.35)	(0.39)
Contractor deal × politically and agency-connected sponsor	0.143	0.120	0.127	0.084	0.046	0.028
	(0.92)	(0.73)	(0.40)	(0.26)	(0.15)	(0.09)
Small-business contractor	−0.249	−0.256	−0.186	−0.156	−0.241	−0.189
	(−0.82)	(−0.86)	(−0.46)	(−0.40)	(−0.72)	(−0.59)
Never-small-business contractor	0.212	0.158	0.403	0.289	0.259	0.178
	(1.34)	(1.15)	(1.50)	(1.23)	(1.27)	(0.94)
Contractor deal × small-business contractor	0.244	0.247	0.237	0.205	0.253	0.197
	(0.79)	(0.82)	(0.57)	(0.51)	(0.74)	(0.60)
Contractor deal × never-small-business contractor	−0.183	−0.137	−0.426	−0.323	−0.265	−0.200
	(−1.08)	(−0.91)	(−1.47)	(−1.25)	(−1.16)	(−0.94)
Log of the sponsor's deals, years −7 to −3	0.010	0.010	0.002	0.002	0.009	0.009
	(0.74)	(0.74)	(0.14)	(0.14)	(0.71)	(0.72)
Controls	Deal in PitchBook; year-by-sector fixed effects					
Observations	21,376	21,376	24,592	24,592	24,592	24,592
Within R^2	0.003	0.003	0.005	0.005	0.003	0.004

Figure 1
Government Contracting after the Buyout

This figure plots the change in federal contracting of buyout targets and of their matched contractors from the year before the deal (year −1) to each year from −5 to +7, with the deal year at 0. The outcome is the firm's percentile rank among never-acquired federal contractors by the number of new contracts (left panels) or the dollar value of new contracts (right panels) won in the fiscal year; firms with no new contract are assigned zero. Solid lines are buyout targets and dashed lines their matched contractors. Matched contractors are never-acquired contractors that in the year before the deal had the same fiscal year, the same range of new contracts (none, 1 to 2, 3 to 9, or 10 or more) in the two preceding years and in the two years before that, the same tercile of contract dollars over the preceding five years, and the same position in a contracting spell or idle period (in a spell of 1 to 3, 4 to 6, or 7 or more years, or idle for 1 to 2 or 3 or more years); they are followed forward from the year before the deal, including those that stop contracting. The vertical distance between the two lines is the estimated effect of the buyout reported in Table 5, and the shaded band is its 95% confidence interval, based on standard errors clustered by matched group. Panel A includes all targets, Panel B small-business contractors (targets that won more than half of their size-determined contracts in the three fiscal years before the deal year as small businesses), and Panel C all other targets. The sample is the established buyout targets: targets whose first federal contracting activity is three or more years before the deal year. Targets first active later (entrants) and targets that formally changed from small to other than small after the deal are excluded.

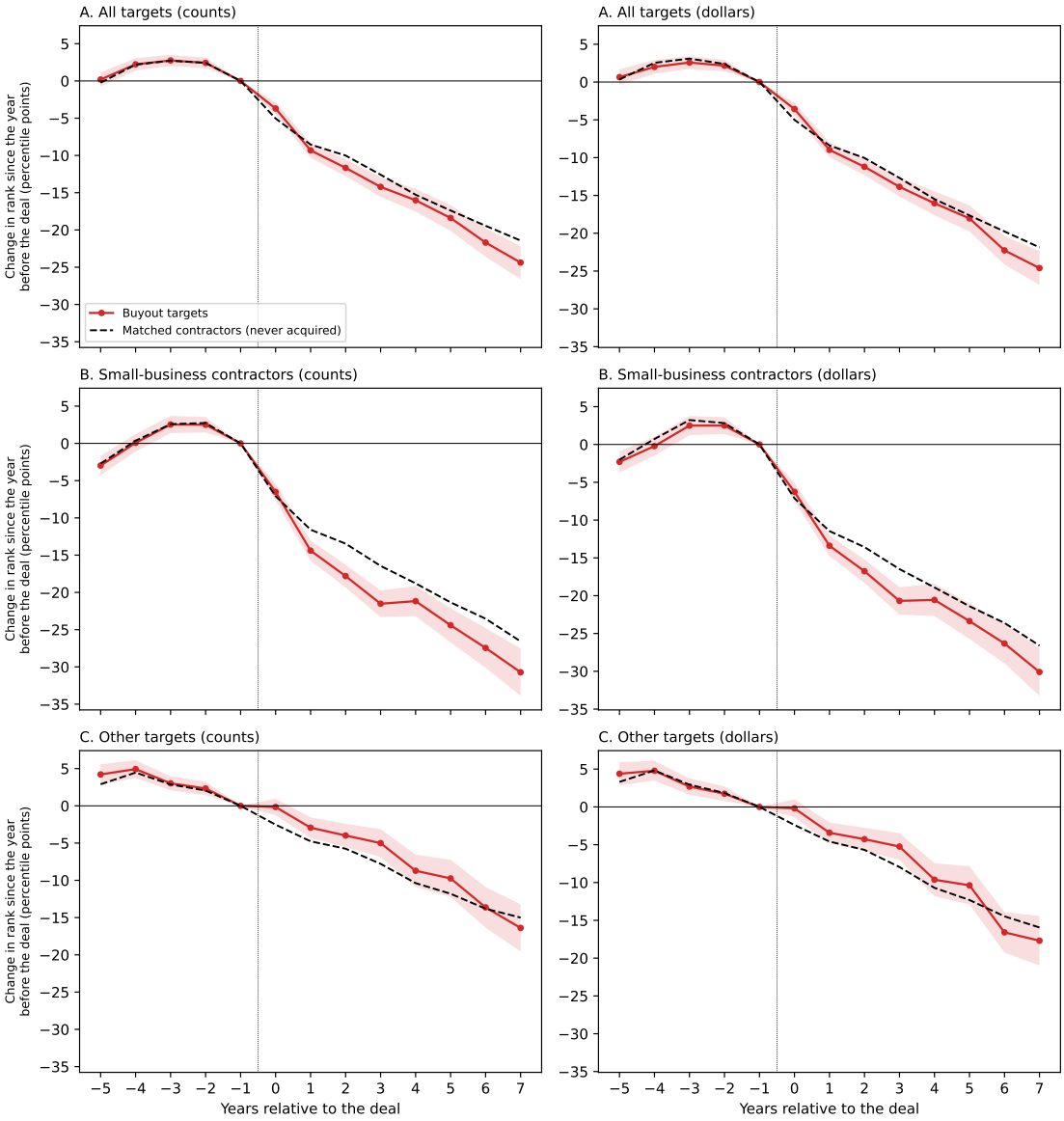
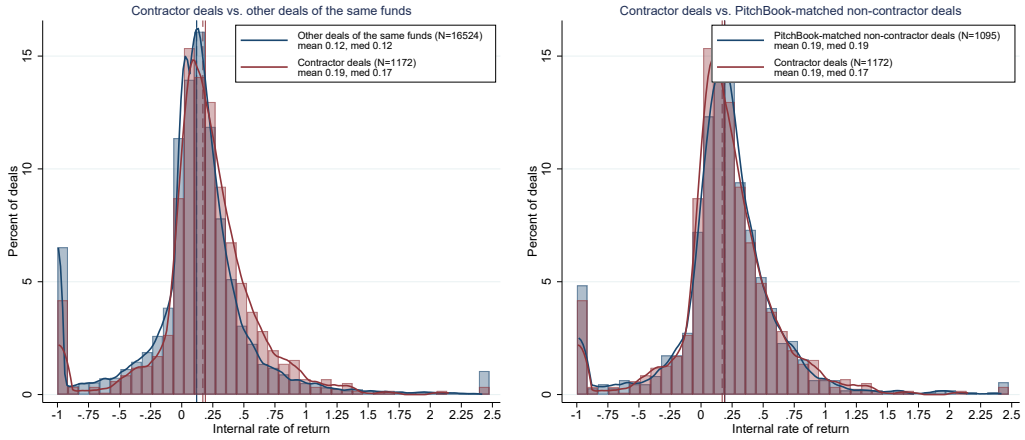


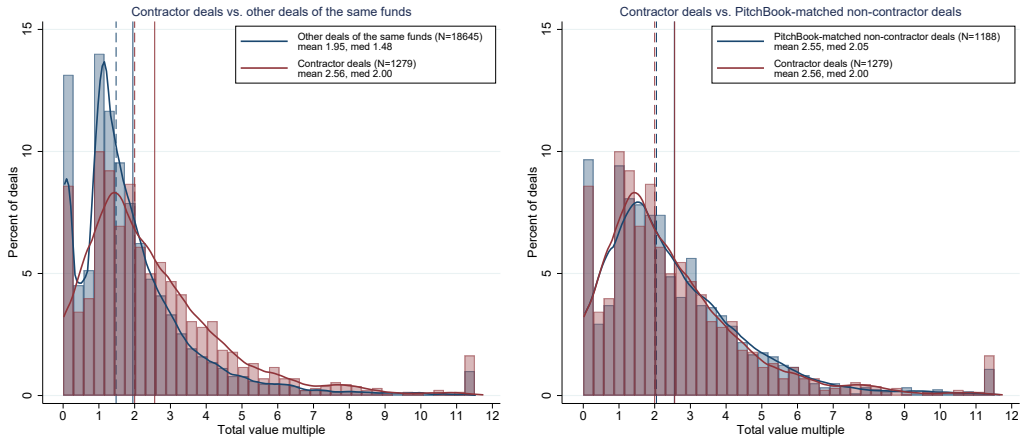
Figure 2
Returns on Contractor Deals

This figure compares the distributions of deal-level returns on buyouts of federal contractors with two benchmarks, in the Burgiss data: the internal rate of return (Panel A), the total value multiple (Panel B), and the PME (Panel C), each winsorized at the 99th percentile of the pooled sample. The left panels compare contractor deals with the other deals of the same funds; the right panels compare them with buyouts that are matched to PitchBook but are not contractor deals. The legends report the number of deals, the mean, and the median of each group; solid vertical lines mark the means and dashed lines the medians.

Panel A: Internal rate of return



Panel B: Total value multiple



Panel C: PME

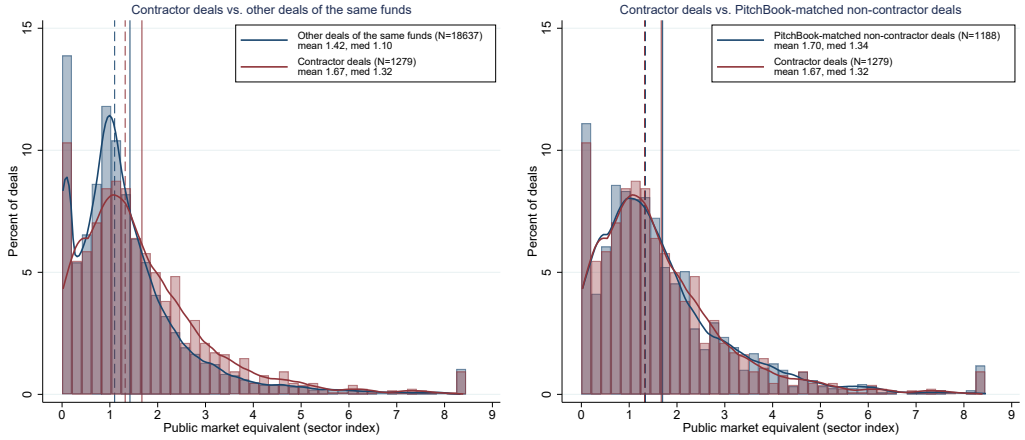
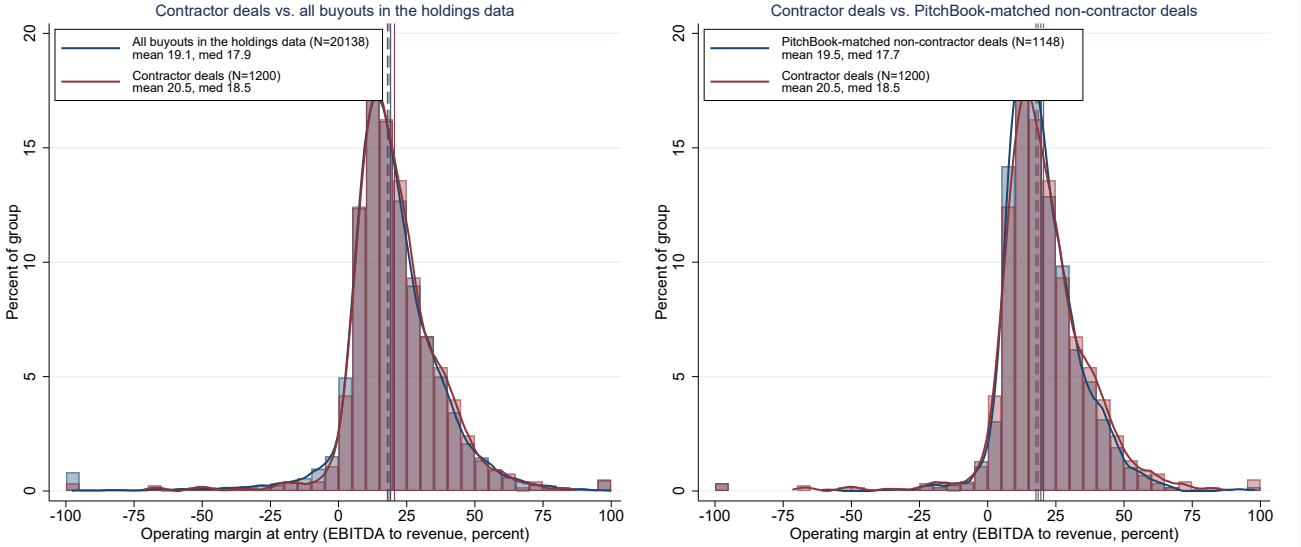


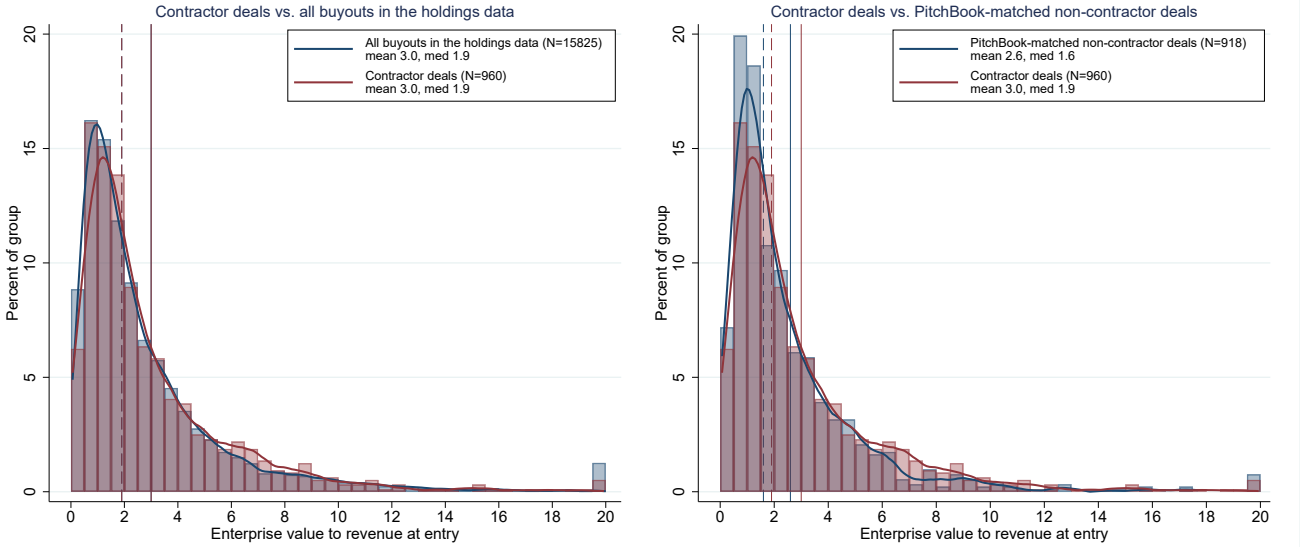
Figure 3
Entry Margins and Valuations

This figure compares the operating margin at entry (Panel A) and the ratio of enterprise value to revenue at entry (Panel B) of buyouts of federal contractors with two benchmarks, in the Burgiss data. The left panels compare contractor targets with all buyouts in the holdings data; the right panels compare them with buyouts that are matched to PitchBook but are not contractor deals. The legends report the number of deals, the mean, and the median of each group; solid vertical lines mark the means and dashed lines the medians. Operating margins are shown from −100% to 100% and ratios of enterprise value to revenue up to 20; the bars at the ends of the axis hold the deals beyond these limits.

Panel A: Operating margin at entry



Panel B: Enterprise value to revenue at entry



Appendix

Table A1
Alternative Definitions of Matched Contractors

This table reports the average effect over years 0 to +7, with its standard error clustered by matched group, and the p-value of the joint test that the estimates for years −5 to −2 are zero, for the established buyout targets, under alternative definitions of the matched contractors. Each row defines the matched group differently: by treatment year and the pre-deal activity cell (the number of new contracts in the two preceding years and the two years before that, and the tercile of contract dollars over the preceding five years); by treatment year and three or five contracting states in the year before the deal (idle or in a spell of a given length); by three states and the size tercile or the recent-award bin; and by the cell, ten bins of spell length, and deciles of recent contracts. The last row is the main design of Tables 3 and 5. The outcome is the firm's percentile rank among never-acquired federal contractors by the number of new contracts (counts) or the dollar value of new contracts (dollars) won in the fiscal year; firms with no new contract are assigned zero. The sample is the established buyout targets: targets whose first federal contracting activity is three or more years before the deal year. Targets first active later (entrants) and targets that formally changed from small to other than small after the deal are excluded. Event time is measured from the deal year (year 0); year −1 is the reference year. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

	Contract counts			Contract dollars		
	Average	Standard error	Pre-deal p	Average	Standard error	Pre-deal p
Year × pre-deal activity cell	-2.71^{***}	(0.53)	0.000	-2.52^{***}	(0.53)	0.000
Year × three contracting states at −1	-1.34^{**}	(0.54)	0.000	-1.29^{**}	(0.52)	0.000
Year × five contracting states at −1	-1.22^{**}	(0.57)	0.000	-1.22^{**}	(0.53)	0.002
Three states × size tercile at year −1	−0.57	(0.54)	0.000	-1.00^{*}	(0.52)	0.000
Three states × recent-award bin	-0.92^{*}	(0.54)	0.000	-0.99^{**}	(0.49)	0.000
Cell × spell length (10 bins) × count decile	−0.57	(0.41)	0.004	−0.48	(0.45)	0.194
Main design (Tables 3 and 5)	-0.84^{***}	(0.23)	0.937	-0.58^{**}	(0.23)	0.500

Table A2
Deal Type and Federal Contracting: Growth Equity and Venture Capital

This table reports linear probability models of whether a completed PitchBook deal is a growth equity investment (columns 1–2) or a venture capital round (column 3). The sample is the completed PitchBook deals of 2011 to 2026 with at least one investor that has ten or more PitchBook deals, including deals for firms that never sold to the government (whose contracting measures are zero). The target's government business is measured over the three years before the deal: the dollar value of its initial awards on which the contracting officer determined it to be other than small (contract dollars outside small-business programs), the number of its initial awards on which it was determined small (contracts won as a small business), both winsorized at the 95th percentile among contractors, and, in columns 2 and 3, the length in years of its current contracting spell in the year before the deal. Each measure enters as the logarithm of one plus its value, divided by 100, so that a coefficient is the change in the probability, in percentage points, for a one-unit change in the logarithm of one plus the measure. Each is interacted with an indicator for deals with a private equity sponsor (an investor of a financial-sponsor type, with ten or more PitchBook deals and a fund raised in the deal year or the four years before) and with an indicator for deals with other investors only. The deal-history controls are the numbers of the target firm's own PitchBook deals in the five years before the deal, by type (venture capital, buyout, growth equity, recapitalization, M&A, PIPE, and IPO). Employee-count fixed effects use six PitchBook employee bins. Contracting-experience (experience) fixed effects are five levels of the length of the firm's federal contracting history relative to its age. The last rows report the share of deals of the column's type (the sample mean of the dependent variable, in percent) and the number of deals. Each column repeats the regression of Table 4 on the same sample, with the column's deal type as the dependent variable. t-statistics, based on standard errors clustered by industry and year, are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

	Deal is a growth equity investment		Deal is a venture capital round
	(1)	(2)	(3)
PE-sponsor deal × contract dollars outside small-business programs	0.166^{***}	-0.029	-0.124^{**}
	(2.97)	(−0.55)	(−2.26)
PE-sponsor deal × contracts won as a small business	0.505^{*}	0.324	-1.141^{***}
	(1.85)	(1.10)	(−4.28)
PE-sponsor deal × length of current contracting spell		0.544^{*}	-1.126^{**}
		(2.08)	(−2.90)
Other investors × contract dollars outside small-business programs	-0.145^{*}	-0.315^{***}	-0.224^{**}
	(−1.94)	(−4.58)	(−2.63)
Other investors × contracts won as a small business	-0.753^{**}	−0.324	-2.392^{***}
	(−2.45)	(−0.80)	(−4.40)
Other investors × length of current contracting spell		−0.273	0.440
		(−0.61)	(0.96)
Firm's earlier private capital deals (5 years)	Yes	Yes	Yes
Employee-count fixed effects	No	Yes	Yes
Industry-by-year, age, investor-type, and contracting-experience fixed effects	Yes	Yes	Yes
Share of deals of the column's type (%)	6.1	6.1	55.1
Observations	187,216	187,216	187,216

Table A3
Small-Business Status after the Deal

This table describes the small-business status of contractor targets after the deal. The sample is the PitchBook deals of targets with federal contracts whose 36 months after the deal are covered by the procurement archives, by deal type. Panel A reports how many targets remain active in federal contracting after the deal and how many file a size re-representation (a formal change of the size status recorded on an existing contract) or a novation (a transfer of a contract to a new legal entity). Panels B and C report the direction of the first such filing: the size recorded on the contract before the filing and on the filing. Panel D describes the active deals with neither filing: whether they win new standalone contracts, only orders under existing agreements, only new contract vehicles (agreements under which orders are later placed), or only modifications; and, for deals from 2015 on, whose size certification in the System for Award Management (SAM) is observable when they win a new award, whether they certify themselves as small under the award's industry code on at least one new award, whether the contracting officer also determined them small on that award, and whether the award was set aside for small businesses. The last two columns of Panel D report the same rows for buyouts of small-business contractors: targets that won more than half of their size-determined contracts in the three fiscal years before the deal year as small businesses. Percentages are of the row's base: deals observed for 36 months (active), active deals (filings), deals with a filing (Panels B and C), active deals with neither filing (activity), and deals with an observable certification (certification rows).

	Buyouts		Growth equity		Venture capital		Buyouts of small-business contractors	
	Deals	%	Deals	%	Deals	%	Deals	%
Panel A: Federal contracting activity in the 36 months after the deal								
Deals observed for 36 months after the deal	5,308		1,264		5,892			
Active in federal contracting after the deal	4,302	81.0	1,091	86.3	4,971	84.4		
with a size re-representation	227	5.3	64	5.9	53	1.1		
with a novation	273	6.3	74	6.8	56	1.1		
with either	465	10.8	121	11.1	107	2.2		
active, with neither filing	3,837	89.2	970	88.9	4,864	97.8		
Panel B: First size re-representation after the deal								
small → other	126	55.5	31	48.4	19	35.8		
small → small	38	16.7	16	25.0	28	52.8		
other → other	48	21.1	11	17.2	3	5.7		
other → small	11	4.8	5	7.8	3	5.7		
no prior size → other	3	1.3	1	1.6	0	0.0		
no prior size → small	1	0.4	0	0.0	0	0.0		
Deals with a re-representation	227	100.0	64	100.0	53	100.0		
Panel C: First novation after the deal								
small → other	10	3.7	1	1.4	0	0.0		
small → small	67	24.5	10	13.5	20	35.7		
other → other	46	16.8	9	12.2	2	3.6		
other → small	3	1.1	2	2.7	0	0.0		
no prior size → other	91	33.3	28	37.8	12	21.4		
no prior size → small	56	20.5	24	32.4	22	39.3		
Deals with a novation	273	100.0	74	100.0	56	100.0		
Panel D: Deals without re-representation or novation								
Active deals with neither filing (Panel A)	3,837		970		4,864		2,120	
won a new standalone contract	2,910	75.8	774	79.8	4,005	82.3	1,567	73.9
new orders under existing agreements only	320	8.3	78	8.0	176	3.6	204	9.6
new contract vehicles only	29	0.8	13	1.3	41	0.8	11	0.5
modifications of existing contracts only	578	15.1	105	10.8	642	13.2	338	15.9
Deals with a SAM size certification observable on a new award (2015 or later)	1,300		365		1,165		745	
certified small (SAM) on at least one new award	880	67.7	256	70.1	1,091	93.6	619	83.1
... and determined small by the contracting officer	799	61.5	232	63.6	1,016	87.2	580	77.9
... and won a set-aside	406	31.2	118	32.3	392	33.6	309	41.5
dropped the small certification between the deal and the award	161	12.4	23	6.3	46	3.9	135	18.1
determined small while certified not small	231	17.8	58	15.9	35	3.0	117	15.7

Table A4
Contract Modifications, Every Contract Weighted Equally

This table repeats Table 7, Panel A, weighting every contract equally, so that targets with many contracts dominate the estimates. One row per contract that a buyout target or a matched contractor of its stratum (up to 50 per stratum, reweighted to the targets' shares) held at the deal; outcomes over years 0 to +2; the placebo columns repeat the construction for contracts held four years earlier. *Time extension*, *funding increase*, and the *log of final to initial obligations* are defined as in Table 7. OLS on a target indicator with stratum-by-award-year fixed effects. The sample is the established buyout targets: targets whose first federal contracting activity is three or more years before the deal year. Targets first active later (entrants) and targets that formally changed from small to other than small after the deal are excluded. t-statistics, clustered by stratum, are in parentheses; target contracts in brackets. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

	Years 0 to +2		Placebo: years −4 to −2	
	Estimate (1)	Contracts (2)	Estimate (3)	Contracts (4)
Time extension	−0.003	128,741	−0.038	109,231
	(−0.24)	[9,268]	(−1.58)	[7,818]
Funding increase	0.014	128,741	−0.043*	109,231
	(0.93)	[9,268]	(−1.84)	[7,818]
Log of final to initial obligations	0.022*	122,792	−0.012	105,514
	(1.68)	[9,034]	(−0.72)	[7,639]
Targets with contracts held at the deal		1,526		

Table A5
Returns Data

This table describes the deal-level returns data (Burgiss in Panels A to C, StepStone in Panel D): the internal rate of return; the total value multiple; distributions to paid-in capital; the PME against the size- and industry-matched sector index and against the Russell 2000; the holding period in years; the amount invested; the ratio of enterprise value to EBITDA at entry; and, over the holding period, the change in log revenue and the growth of EBITDA measured as the change divided by the average of its entry and exit values. Panel A includes all buyout deals in the returns data, Panel B the deals of funds that hold at least one buyout matched to PitchBook, and Panel C the buyout deals matched to PitchBook. Panel D reports the buyout deals in the StepStone data matched to PitchBook, which report neither distributions nor holding periods.

	Mean	SD	10th	25th	Median	75th	90th	N
Panel A: All buyout deals in the returns data								
IRR	0.192	1.653	−0.383	−0.001	0.122	0.287	0.549	25,400
Total value multiple	2.466	1.976	0.130	0.968	1.454	2.400	3.890	28,978
Distributions to paid-in capital	1.740	1.798	0.000	0.000	0.450	1.930	3.420	26,933
PME, sector index	1.407	1.369	0.083	0.599	1.089	1.759	2.854	28,932
PME, Russell 2000	1.338	1.263	0.081	0.611	1.051	1.673	2.685	28,921
Years held	5.960	3.196	2.341	3.554	5.418	7.833	10.253	13,162
Invested (millions of US dollars)	162.91	303.19	9.00	22.50	61.26	171.76	403.37	20,884
Enterprise value to EBITDA at entry	15.94	26.24	5.32	7.14	9.83	14.37	23.86	11,950
Change in log revenue	0.547	0.811	−0.204	0.081	0.410	0.876	1.496	14,290
EBITDA growth (midpoint)	0.271	0.947	−1.078	−0.148	0.343	0.854	1.425	14,272
Panel B: Deals of funds with a PitchBook-matched buyout								
IRR	0.187	1.470	−0.423	−0.002	0.127	0.291	0.550	17,696
Total value multiple	2.571	1.994	0.130	0.980	1.500	2.520	4.040	19,924
Distributions to paid-in capital	1.898	1.816	0.000	0.000	0.530	2.000	3.560	18,556
PME, sector index	1.432	1.385	0.083	0.603	1.109	1.811	2.922	19,916
PME, Russell 2000	1.372	1.287	0.080	0.616	1.079	1.729	2.762	19,916
Years held	5.918	3.135	2.409	3.573	5.410	7.748	10.174	9,188
Invested (millions of US dollars)	185.65	338.92	9.76	25.00	70.00	200.00	465.00	14,611
Enterprise value to EBITDA at entry	15.65	25.40	5.35	7.19	9.86	14.24	23.17	9,190
Change in log revenue	0.543	0.811	−0.213	0.078	0.412	0.873	1.492	10,978
EBITDA growth (midpoint)	0.279	0.934	−1.027	−0.139	0.353	0.852	1.402	10,981
Panel C: PitchBook-matched buyout deals								
IRR	0.219	0.892	−0.188	0.040	0.186	0.374	0.628	2,267
Total value multiple	2.768	2.373	0.380	1.120	2.010	3.410	5.150	2,467
Distributions to paid-in capital	1.882	2.886	0.000	0.000	0.880	2.960	4.730	2,334
PME, sector index	1.686	1.486	0.171	0.711	1.322	2.230	3.503	2,467
PME, Russell 2000	1.723	1.455	0.192	0.782	1.366	2.306	3.530	2,467
Years held	5.911	2.939	2.500	3.844	5.435	7.587	9.999	1,466
Invested (millions of US dollars)	231.78	404.11	18.58	40.00	95.85	264.38	565.65	1,888
Enterprise value to EBITDA at entry	12.76	16.94	5.59	7.19	9.51	12.93	18.41	1,847
Change in log revenue	0.477	0.620	−0.132	0.103	0.403	0.786	1.218	2,059
EBITDA growth (midpoint)	0.368	0.783	−0.564	0.028	0.424	0.840	1.266	2,134
Panel D: PitchBook-matched buyout deals in the StepStone data								
IRR	0.292	0.390	−0.022	0.080	0.242	0.438	0.723	1,657
Total value multiple	2.886	2.411	0.937	1.290	2.250	3.600	5.784	1,663
PME, sector index	2.036	1.727	0.563	0.968	1.564	2.575	4.014	1,485
PME, Russell 2000	2.070	1.709	0.601	0.982	1.644	2.565	3.981	1,485
Invested (millions of US dollars)	224.86	426.18	16.93	35.86	87.35	242.48	577.99	1,597
Enterprise value to EBITDA at entry	11.59	10.74	5.78	7.19	9.33	12.39	17.28	932
Change in log revenue	0.552	0.593	−0.052	0.141	0.462	0.858	1.325	934
EBITDA growth (midpoint)	0.512	0.709	−0.256	0.173	0.565	0.922	1.316	936