Is Government Spending Wasteful? Evidence from Private Equity*

Nickolay Gantchev† Oleg R. Gredil‡ Constantine N. Yannelis§

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Abstract

Does the US government systematically overpay contractors? We test this using private equity (PE) buyouts as a revealed-preference test for profit opportunities. PE firms specifically target government contractors for acquisition but then dramatically reduce government contracting post-buyout—contract counts decline 35% and values decline 89% by year seven. Venture capital and growth equity investors show no such pattern, confirming these effects are in PE deals, in which sponsors put less priority on revenue growth over profitability. The exits concentrates in fixed-price contracts while cost-plus contracts weakly grow, pointing to a profitability-based explanation rather than regulatory burden or government exclusion. Conditionally on remaining an active contractor, PE-backed firms are more likely to be awarded cost-plus contracts than their non-PE backed counterparts. Unconditionally on the pre-deal characteristics, the targets backed by politically- or federal agency-connected PE sponsors, as well as those with prior investing experience in government contractors, reduce contracting at least as aggressively, if not faster. In contrast, PE-sponsored targets with strong renegotiation track records pre-deal appear to grow their government contracts book. While PE firms can identify and retain select profitable relationships, their systematic culling of business with federal agencies suggests that government contracting margins are, on average, too compressed to attract profit-maximizing capital. Our matching of the PE investees to government contract records is based on the common identifier provided directly by the respective data vendors, which minimizes the risk of falsely assigning PE ownership in formerly related contractors.

Keywords: Contracting, Public procurement, Political connections, Private equity, Growth equity, Venture capital

JEL-Classification: G23, G24, G34, G38, H57.

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†University of Virginia McIntire School of Commerce, CERP, ECGI; e-mail: gantchev@virginia.edu

‡Tulane University A.B. Freeman School of Business; e-mail: ogredil@tulane.edu

§University of Cambridge, Pembroke College; e-mail: cny21@cam.ac.uk

"Somewhere close to 10 percent of the federal budget, which is upwards of $500 billion, is lost every year to waste, fraud, and abuse."

—Scott Bessent, U.S. Treasury Secretary, 01/10/2026 interview

1. Introduction

Does the U.S. federal government overpay for goods and services? This question lies at the heart of decades of public policy debate, with critics claiming government procurement represents a massive transfer of taxpayer wealth to contractors. If true, this would constitute one of the largest market inefficiencies in the economy – with annual federal procurement exceeding $700 billion, even modest overpayment would imply hundreds of billions in annual rents. Yet despite extensive anecdotal evidence and political rhetoric, identifying whether government truly overpays remains an empirical challenge. Directly observing contractor profitability is difficult because most government contractors are privately held, and isolating the returns specifically attributable to government business within diversified firms is nearly impossible.

We provide a novel revealed-preference test by examining how sophisticated, profit-maximizing investors behave when acquiring government contractors. Private equity (PE) investors are perhaps the most aggressive screeners of profit opportunities in modern capital markets, with strong incentives and substantial resources dedicated to identifying and capturing profit opportunities.[1] Their actions reveal beliefs about profitability more credibly than stated intentions or accounting measures. If the government systematically overpaid contractors, creating excess returns, PE investors acquiring these contractors should expand government business to capture these rents. Instead, the data show the opposite: PE-backed firms significantly reduce government contracting post-acquisition, providing revealed-preference evidence against systematic overpayment.

The pattern begins with a puzzle. We show that past government contracting experience strongly and significantly predicts which firms PE investors target for buyout. This targeting pattern is unique to buyouts and economically meaningful – venture capital and growth equity investors show no such preference or even avoid government contractors. Firms with ten or more years of government contracting are significantly more likely to be involved in a buyout, and the PE-backed contractor share has been steadily growing since 2008, with most buyout equity sponsors having

[1]See, inter alia, Jensen (1989); Kaplan and Strömberg (2009); Acharya, Gottschalg, Hahn, and Kehoe (2013); Gompers, Kaplan, and Mukharlyamov (2016); Fracassi, Previtero, and Sheen (2022).

had recent experience investing in government contractors. PE buyout-sponsors (General Partners or GPs) clearly view government contracting experience as valuable when evaluating acquisition targets. Yet about a year following acquisition, these same sophisticated investors begin to reduce the government contracting business they specifically paid to acquire.

The post-acquisition contraction is large and grows over time. By seven years post-buyout, contract counts decline by approximately 35% and contract values by roughly 89% relative to non-PE-backed contractors. The effects begin modestly – statistically insignificant in year one – but grow monotonically, reaching economically large magnitudes by year three and continuing through year seven. This gradual pattern suggests deliberate portfolio restructuring rather than immediate liquidation of government business. Importantly, venture capital and growth equity backed contractors show no such pattern, inconsistent with explanations based on general economic conditions or governmental policies that would affect all private capital portfolio companies. This contrast suggests that when investors prioritize revenue growth over operational profitability—as venture capital and growth equity sponsors typically do—PE-backed contractors continue growing their government business.

Why would profit-maximizing investors specifically target government contractors only to exit this business? The regulatory burden hypothesis offers an intuitive alternative explanation: perhaps PE-backed firms avoid government contracts not because they are unprofitable, but to escape disclosure requirements and compliance costs that conflict with PE preferences for operational opacity. However, multiple findings contradict this explanation. If regulatory burden drove the exit, we would expect PE sponsors to reduce all contract types since disclosure requirements apply universally. Instead, the decline concentrates almost entirely in fixed-price contracts while cost-plus contracts remain stable. Under fixed-price contracts, the contractor bears the full risk of cost overruns and margins depend on competitive bidding; under cost-plus contracts, the government reimburses allowable costs and pays an additional fee. The selective exit from fixed-price contracts thus points to a profitability-based explanation: PE-backed contractors exit government business that offers thin returns under competitive conditions, not government business per se.

The firm-level evidence reinforces this conclusion. Classifying contractors by their pre-buyout contract mix, we find that firms with predominantly fixed-price portfolios are 5.5 percentage points more likely to exit government contracting entirely following a buyout, while contractors with more cost-plus portfolios are more likely to secure new contracts. Among contractors that maintain government business post-buyout, the surviving portfolio shifts marginally toward cost-plus contracts. These extensive-margin results complement the intensive-margin patterns: buyout targets not only reduce fixed-price contracts within their portfolio, but those whose government business consists primarily of such contracts exit entirely—consistent with the fixed overhead of maintaining compliance infrastructure being unjustified when the underlying contracts offer thin returns.

We also find that politically connected PE sponsors—those whose employees have political connections or federal agency experience—show equally strong or stronger contracting reductions than non-connected firms. This is a key test of the overpayment hypothesis. If government contracts offered above-market rents, connected GPs would be best positioned to capture them – through favorable contract terms, reduced competition, or privileged access to procurement officers. The fact that these investors reduce government business more aggressively implies that direct knowledge of government contracting, rather than revealing profit opportunities, reinforces the decision to exit.

A related alternative is that the government, rather than PE sponsors, drives the contraction – perhaps by excluding PE-backed contractors due to ownership concerns. Several patterns contradict this explanation. The selectivity across contract types indicates strategic PE choice rather than blanket government policy. PE sponsors exit high-competition contracts more than low-competition ones, yet government would find it easiest to replace contractors precisely in competitive settings where alternatives abound. Moreover, PE sponsors with more prior government contracting experience show significantly larger decreases in their targets' government business, consistent with informed strategic decisions rather than involuntary exclusion.

We also examine whether GPs exploit the well-documented surge in government spending at the end of the federal fiscal year. Agencies facing "use-it-or-lose-it" budget authority rush to obligate funds before September 30, with spending in the last week reaching 4.9 times the weekly average (Liebman and Mahoney, 2017). These year-end contracts represent a setting where government is most willing to spend on unfavorable terms, offering connected PE sponsors the greatest opportunity to extract rents through favorable contract modifications. We find no evidence that they do. For the combination most conducive to rent extraction – low-competition contracts modified at fiscal year-end by politically connected GPs – the effect on modifications is positive but economically modest and barely significant. Meanwhile, connected GPs managing competitively bid contracts during the same period significantly reduce their modification activity. Even when the conditions for rent-seeking are most favorable, sophisticated PE investors do not expand their government contracting relationships.

The cross-sectional evidence reinforces this interpretation. GPs who have prior experience with government contracting deals show significantly larger reductions in government business, suggesting a learning mechanism: experience confirms the limited upside of government contracting, leading experienced GPs to implement larger reductions in subsequent deals. Conversely, targets with demonstrated track records of successful contract renegotiations – measured by pre-acquisition contracts showing both dollar amount and duration increases – experience significantly smaller declines post-buyout. PE sponsors thus distinguish between government relationships that offer defensible margins and those that do not – keeping the former and exiting the latter.

Our findings have implications for the debate on government spending efficiency. The evidence suggests that PE values government contractors for capabilities and market positions that can be redeployed toward more profitable private-sector opportunities, rather than for the government business itself. The post-acquisition contraction, concentrated in fixed-price and competitive contracts, demonstrates that government business offers insufficient risk-adjusted returns to attract profit-maximizing investors. The finding that even politically connected PE sponsors with information advantages show no robust profit extraction from year-end, non-competed contracts reinforces this conclusion. This interpretation does not imply that government procurement is free of inefficiency, but it does challenge the premise that the government systematically overpays for the goods and services it purchases.

This paper joins an extensive literature on the effects of PE, beginning with work by Kaplan (1989). Ewens and Farre-Mensa (2022) and Sorensen and Yasuda (2023) provide recent reviews of this literature. More specifically, this paper contributes to studies on the impacts of PE-sponsors on the acquisition targets' strategy and operating performance (see, e.g., Acharya, Gottschalg, Hahn, and Kehoe, 2013; Boucly, Sraer, and Thesmar, 2011; Bernstein and Sheen, 2016; Fracassi, Previtero, and Sheen, 2022, among others). Within this literature, the closest work to ours is Liu (2022), who shows that PE ownership changes hospital bargaining with insurance plans. Our paper differs along several dimensions: rather than examining bilateral price negotiation in a single industry, we study how PE sponsors reshape the full scope of a firm's contracting relationship with the federal government—choosing which contracts to pursue, retain, or abandon across a broad cross-section of industries. We further show that the scope of this intervention depends on the extent to which revenue growth is a priority over profitability, as revealed by the contrasting behavior of buyout versus growth equity and venture capital investors.

A related strand of the PE literature examines externalities of PE ownership on the targets' customers (Eaton, Howell, and Yannelis, 2019; Gupta, Howell, Yannelis, and Gupta, 2024; Howell and Liu, 2025; Howell, Jang, Kim, and Weisbach, 2025) and other stakeholders (Davis, Haltiwanger, Handley, Jarmin, Lerner, and Miranda, 2014; Gornall, Gredil, Howell, Liu, and Sockin, 2025; Bellon, 2025). This literature shows that the externalities tend to be negative when customers are captive or poorly informed, but can be positive when aligned with improving PE-sponsor investment returns and future deal flow. Our paper complements these studies by examining a setting in which the primary customer is a government agency operating under formal procurement rules and subject to public scrutiny—a setting where captive-customer concerns are less relevant, yet PE sponsors still systematically reduce the business relationship.

Our paper also contributes to the literature on government procurement and contracting. A substantial body of work examines the design and efficiency of government contracts, particularly the choice between fixed-price and cost-plus arrangements (Bajari and Tadelis, 2001; Crocker and Reynolds, 1993; Carril and Duggan, 2020). We add to this literature by showing that private investors implicitly evaluate these contract types differently: PE sponsors retain cost-plus contracts while exiting fixed-price contracts, providing market-based evidence on the relative profitability of different pricing structures from the contractor's perspective. A separate strand investigates the role of political connections and institutional design in procurement outcomes. Brogaard, Denes, and Duchin (2021) show that politically connected public firms renegotiate government contracts on more favorable terms, while Decarolis, Guiffrida, Iossa, Mollisi, and Spagnolo (2020) demonstrate that the competence of procurement officers affects contract performance. Our paper differs from these studies in that it examines contracting outcomes from the investor side rather than the government side—using PE sponsors' portfolio decisions as a revealed-preference measure of contract profitability. Government contracts also influence employment (Barrot and Nanda, 2020), investment and profitability (Cohen and Li, 2020), and R&D activity (Ngo and Stanfield, 2022); whereas these papers take the government contracting relationship as given, we study the conditions under which sophisticated investors choose to maintain or exit it.

Finally, this paper speaks directly to the literature on government spending efficiency. Bandiera, Prat, and Valletti (2009) distinguish between active waste (corruption) and passive waste (inefficiency) in Italian public procurement, finding that most excess spending reflects the latter. Liebman and Mahoney (2017) document that year-end budget pressure leads to lower-quality federal procurement, with spending in the final week of the fiscal year reaching nearly five times the weekly average, and Boland and Godsell (2021) show that bureaucratic discretion at fiscal quarter-ends facilitates political bias in contracting outcomes. These studies measure waste by comparing prices or outcomes across government transactions. Our approach is fundamentally different: rather than benchmarking government purchases against one another, we use the behavior of profit-maximizing outside investors as a market test of whether government contracting generates above-market returns. The fact that PE sponsors—including those with political connections and agency experience—systematically exit certain types of government contracting after acquiring contractors provides revealed-preference evidence that these returns are, on average, insufficient to attract private capital. This finding complements the existing literature by offering a test based on investor behavior rather than on the structure of government transactions themselves.

2. Data and Variables

Our empirical analysis requires linking private capital transactions to federal procurement records at the firm level. We merge two primary data sources: Pitchbook, which provides comprehensive coverage of private equity, venture capital, and related deals, and USAspending.gov, the official repository of federal contract awards. The merge relies on company DUNS identifiers present in both databases through 2022 and allows us to track the complete government contracting activity of PE-backed firms both before and after acquisition. Additional construction details appear in Appendix A1; a glossary of key variables appears in Appendix A3.

2.1. Deals

Our deal sample draws from Pitchbook's "All Deals North America Search" universe. We select deals closed between 2009 and 2024 that involve ownership or control transfer to or from an institutional PE or VC investor and in which the target has a DUNS identifier in Pitchbook. The resulting sample includes 27,712 transactions: 8,616 buyouts, 2,575 growth equity deals, 6,294 venture capital rounds, with the remainder comprising mergers and acquisitions, PIPEs, and IPOs (Table I, Panel A). While our primary analysis focuses on buyouts, the venture capital and growth equity transactions provide an important benchmark for distinguishing buyout-specific effects, and the remaining deal types serve as a broader control group of firms that attract institutional investment.

For each transaction, we extract target company characteristics including age at the time of the deal, disclosed transaction values when available, and operating history. Buyout targets are substantially more mature than venture capital targets, with an average age of 37.2 versus 8.4 years. Disclosed deal size is available for only about 17% of buyouts, with a mean transaction value of $926 million; a similar fraction reports new or total debt information. We also collect industry classifications, headquarters locations, and prior financing history—number and types of previous VC, growth equity, buyout, recapitalization, and other transactions over the preceding five years. This deal history provides crucial controls for company maturity and capital market experience.

We match Pitchbook targets to federal contract awards using DUNS identifiers. The last four columns of Panel A report the number of contracts held by each target during the three years preceding the transaction. About 40% of private capital targets had at least one government contract in the three years preceding the deal across all deal types, with the exception of PIPEs (just over 30%). This high frequency reflects the breadth of federal procurement programs. The scale of government contracting varies considerably across deal types. Venture capital recipients hold far fewer contracts, with a 75th percentile of only six versus 15 or more for other categories, while growth equity targets actually hold larger contract volumes than buyout targets. The distribution is heavily right-skewed throughout, with means exceeding 75th percentiles by factors of 4 to 10.

2.2. Investors

We collect specialization and activity characteristics of the investors in our deal sample, including the total number of deals completed during each calendar year, fundraising activity, and political and government agency connections. Panel B of Table I reports summary statistics for the 1,214 fund managers (General Partners, or GPs) that completed the 8,616 buyout deals in our sample. Each observation averages a GP's activity across the years between its first and last deal during 2009–2024. In a typical GP-year, a fund manager completes 4.25 deals, of which 0.5 involve a government contractor, and raises 1.25 funds averaging just under $300 million. The distributions are notably right-skewed: the fraction of deals involving a contractor averages 15% overall but reaches 43% at the 90th percentile. Since 42% of all buyout deals feature a government contractor (Panel A), more active GPs appear disproportionately likely to invest in these firms.

Panel A of Figure 1 plots the number of government contractors that are recent buyout targets and reveals a steady upward trend—from less than 0.4% of all contractors in 2010 to over 1.8% in

2024, a growth rate that surpasses the corresponding trend for growth equity and venture capital recipients. The reduction in absolute counts during 2023–2024 primarily reflects attrition in our match rate after government contract records dropped the DUNS identifier post-2022.[2] The figure also shows that the count of contractors acquired by GPs with prior government contractor experience has grown in parallel, and increasingly accounts for the bulk of buyout-backed contractors—over 84% by 2021–2022, up from less than 50% in 2008. Even accounting for the low entry rate of new buyout GPs and the mechanical effect of repeated sampling, this pattern suggests that buyout fund managers have become substantially more experienced in targeting government contractors.

Despite this growth in the number of PE-backed contractors, Panel B of the figure shows that their share of aggregate contract volume and value has remained roughly stable since 2018, hovering around 1–2% of federal totals. The contrast is striking: the count of PE-backed contractors has more than tripled, yet their footprint in federal procurement has not expanded. This divergence could reflect PE investors targeting smaller contractors later in the sample, PE-backed firms growing their government business more slowly than other contractors, or—as our subsequent analysis suggests—PE firms actively reducing the government contracting business of their portfolio companies.

We construct three measures of GP political connections from biographical information in Pitchbook investor profiles. Following Faccio and Hsu (2017), we classify a GP as politically connected if team member biographies mention legislative bodies, executive branch positions, political parties, or federal departments. We further distinguish two specific types of connection: congressional connections, based on references to legislative branch experience, and federal agency connections, based on prior employment at agencies and sub-agencies that actually award procurement contracts in our sample. The last three rows of Panel B show that 19% of GPs are politically connected, 16% have federal agency connections, and 9% have congressional connections. These distinctions prove important in our subsequent analysis, which tests whether connected GPs behave differently when managing their portfolio companies' government contracting relationships.

2.3. Government contracts

We construct our contractor-year panel from federal contract transaction records spanning 2008 through 2024. The construction begins with all contract actions—both initiations and modifications—

[2] We used the 2021–2022 Unique Entity Identifiers (UEI) to DUNS correspondence to forward-fill DUNS conditional on no change in the company name. This procedure generates attrition in the sample, but ensures against false positive matches.

for contractors that can be matched to Pitchbook companies via DUNS identifiers. We impose several restrictions to ensure data quality. First, we exclude contracts with initial obligated amounts below $3,000, as such micro-purchases follow simplified acquisition procedures and face different reporting requirements (see Gantchev, Goldman, and Zhang, 2025, for details). Second, we retain only contracts for which we observe the initial award during our sample period, ensuring we capture the complete contract lifecycle from inception. Third, we drop contracts missing essential identifiers and restrict to contracts awarded to U.S. for-profit entities that are not flagged as small businesses.[3] These filters yield a base sample of 15.2 million unique contracts.

We aggregate contract-level data to create a quasi-balanced contractor-year panel. For each DUNS-year observation, we calculate the total value and count of newly initiated contracts, average contract durations, and modification patterns. To account for contractors' intermittent government business, we fill the time series to create a complete panel spanning each contractor's first observed contract year through their last, plus two-year buffers on each end (within the 2008–2024 range). Years with zero contracting activity receive zero values for contract counts and amounts rather than missing values.

Matching contractors to Pitchbook transactions proceeds through DUNS-based mapping, with careful handling of parent-subsidiary relationships. When a contractor's DUNS number does not directly match a Pitchbook entity, we check whether the parent DUNS (as recorded in the contract data) links to Pitchbook. We then assign the parent's identifier to the subsidiary, enabling us to aggregate contracting activity at the consolidated entity level. This matching yields government contracting histories for 6,208 contractors that appear in at least one Pitchbook transaction.

Our final panel comprises approximately 1.4 million contractor-year observations spanning 227,268 unique contractors over the 2008–2024 period (Table I, Panel C). Among the 6,208 contractors that eventually receive private equity investment, the average firm-year includes 16.3 new contract initiations with total initial obligated amounts of $0.79 million. The distribution exhibits substantial right skewness: the median contractor-year has zero new contracts, while the 95th percentile reaches 13 contracts and $2.9 million in annual awards. Contract modifications are common, with average net modification amounts of $0.09 million per contractor-year. When contracts are modified, amounts grow by an average of 8%, while durations increase by 4.21 months. Contractors in this treated group average 8.36 years since their first observed government contract. The 221,060 never-

[3]For more than 20% of the years in the sample.

treated contractors exhibit substantially lower contracting activity, averaging 5.0 new contracts per year with $0.36 million in initial amounts, underscoring the need for contractor fixed effects in our regression analyses.

Our analysis exploits rich heterogeneity in contract characteristics to distinguish among competing explanations for post-buyout contracting patterns. We classify contracts by pricing structure: under cost-plus contracts, the government reimburses allowable costs plus a fee, so that cost risk remains largely with the government; under fixed-price contracts, the contractor bears the risk of cost overruns in exchange for a predetermined price. We measure competition intensity through the number of bids received, enabling us to separate highly competitive contracts from those with limited competition or sole-source awards. We identify year-end contracts modified during the last week of the federal fiscal year (September 24–30), when agencies face "use-it-or-lose-it" budget pressures documented in Liebman and Mahoney (2017). We also cross-classify contracts by whether the acquiring GP has political connections, federal agency experience, or congressional relationships. We aggregate all splits to the DUNS-year level for estimation.

These contract-level splits are central to our identification strategy. If PE-backed firms exit government contracting because it is insufficiently profitable, the pattern should vary systematically with contract economics and GP information advantages. If instead regulatory burden or government exclusion drives the exit, it should not.

3. Who targets government contractors?

We begin by testing what observable target characteristics, including government contracting track record, predict the type of transaction a company is involved in. Using the sample of deals described in Panel A of Table I, we estimate the following linear probability model with an outcome variable indicating whether deal i was a buyout:

$$\mathbb{I}(\text{dealType}_{ijt} = \text{BO}) \;=\; \beta X_{ijt} + a_j + y_t + \epsilon_{ijt} \quad , \tag{1}$$

where a_j and y_t are the target's industry and deal year fixed effects. X_{ijt} comprises two sets of company characteristics. The first includes company age and the history of deals it has been involved in over the previous five years, including private and public capital investments and M&As. The second measures the target's government contracting activity: recent contract amounts and counts, and the spell of government contracting work.[4] These variables are coded as zero for deal targets that do not match to government contract data.

The results appear in Table II. Columns (1) through (4) report estimates for the buyout indicator, columns (5) and (6) for growth equity, and column (7) for venture capital. Standard errors are double-clustered by industry and year.

The estimates reveal a strong and robust association between government contracting and buyout targeting. In column (1), log contract amounts and past three-year contract counts both positively predict buyouts, with coefficients of 0.177 and 1.084 (both significant at the 5% level). Adding the contracting spell dummies in column (2) sharpens the picture: the coefficient on log contract amounts remains stable at 0.155, while the count variable loses significance. Instead, the effect is absorbed by the longest spell category—firms with ten or more years of government contracting are 4.1 percentage points more likely to be involved in a buyout (significant at the 5% level). Columns (3) and (4) add past deal type dummies. Prior buyout involvement is by far the strongest predictor of a current buyout deal (coefficient of 0.166 in column 3, significant at the 1% level), while prior VC rounds negatively predict buyouts (-0.049, also significant at the 1% level). Crucially, the government contracting variables survive these controls. In the full specification (column 4), log contract amounts retain a coefficient of 0.157, and the ten-year spell indicator remains significant at 0.031 (both at the 5% level).

The contrast with other deal types is informative. In columns (5) and (6), none of the government contracting variables significantly predict growth equity deals. For venture capital in column (7), the relationship reverses: log past three-year contract counts enter with a large negative coefficient (-3.025, significant at the 1% level), and a contracting spell of ten years or more reduces the probability of a VC deal by 1.5 percentage points (also at the 1% level). Company age works in the same direction—strongly positive for buyouts and strongly negative for both growth equity and VC—consistent with buyout investors targeting more mature firms. These patterns confirm that buyout investors specifically seek out established government contractors, while VC investors avoid them.

Government contracting experience thus sorts firms into buyout deals rather than other private capital transactions. The joint test that all government contracting variables equal zero is rejected for buyouts and strongly rejected for venture capital (p-values of 0.033 and 0.000 in the full speci-

[4] To determine the spell, we check the first year with contract awards for the corresponding DUNS–UEI in Gantchev, Goldman, and Zhang (2025) dataset that goes back to 2001. We thank the authors for providing this information.

fications, columns 4 and 7), but not for growth equity ($p = 0.070$ in column 6). This pattern aligns with the economics of each deal type. Buyout funds seek mature firms with established operations, recurring revenue relationships, and demonstrable cash flows—attributes that a long government contracting track record directly proxies for. Venture capital investors, conversely, target early-stage firms where government contracting experience, with its compliance infrastructure and rigid pricing, signals the opposite of the high-growth profile they seek. Growth equity occupies an intermediate position, and accordingly shows no systematic relationship with contracting history. These results establish that government contracting experience is a first-order predictor of buyout targeting, not merely a byproduct of other firm characteristics that attract private equity investment.

Table II establishes that government contracting experience predicts buyout involvement. We next examine whether observable characteristics of buyout investors predict whether their target is a government contractor. We restrict the sample to deals by GPs that completed at least one buyout, as reported in Panel B of Table I, and measure investor characteristics as of the deal year rather than averaged over the sample period. The sample also includes non-buyout deals (predominantly growth equity) by these GPs. We estimate:

$$\mathbb{I}(\text{target is GC}_{ijt} = 1) \;=\; \beta_I X^I_{ijt} + \beta_T X^T_{ijt} + a_j + y_t + \epsilon_{ijt} \quad , \tag{2}$$

where X^T_{ijt} are company characteristics as in Table II but excluding past contracting activity, and X^I_{ijt} are the GP characteristics described in Panel B of Table I.

Table III reports the results. In column (1), none of the GP scale variables—log deal count, log funds raised, or log average fund size—significantly predict contractor targeting. Column (2) introduces the political connection measures. The federal agency connection indicator enters with a coefficient of 0.028, significant at the 5% level, while political connections broadly defined and congressional connections show no predictive power. Column (3) combines all GP variables; the federal agency indicator remains marginally significant at 0.025, but the scale variables and other connection measures are insignificant, and the joint F-test of the GP controls yields a p-value of 0.606. Columns (4) through (6) add the target's past deal history as controls. Controlling for deal history substantially increases explanatory power, raising the overall R^2 from 0.031 to 0.103 in the full specification. The GP variables, however, remain insignificant across all specifications. Even the federal agency connection indicator loses its significance once target deal history is included. The GP's listing status is also insignificant across all specifications.

Overall, Table III indicates that selection into government contracting targets operates through firm characteristics rather than investor characteristics. This result motivates our empirical strategy in the next sections, where we control for the target's pre-buyout contracting history to isolate the treatment effect of PE ownership on government contracting activity.

4. Empirical strategy

We estimate the effects of PE deals on targets' government contracting activity using a staggered difference-in-differences design. Two features of our setting complicate standard approaches. First, PE deals occur at different times for different contractors across our 2008–2024 sample period, creating variation in treatment timing that can bias traditional two-way fixed effects (TWFE) estimators when treatment effects are heterogeneous across groups or over time (Goodman-Bacon, 2021; De Chaisemartin and D'Haultfoeuille, 2020). Second, PE backing is non-absorbing: the same contractor may experience buyouts, exits, and subsequent acquisitions, violating the absorbing-treatment assumption maintained by most recent DiD estimators (Sun and Abraham, 2021; Callaway and Sant'Anna, 2021; Borusyak, Jaravel, and Spiess, 2024). We therefore adopt the De Chaisemartin and d'Haultfoeuille (2024) (dCdH) estimator, which explicitly accommodates non-absorbing treatments.[5]

The estimator compares the outcome evolution of "switchers"—contractors whose PE-backing status changes at time F_g—to that of control contractors with the same baseline treatment whose status has not yet changed. For each switcher g and post-treatment lag $\ell \geq 1$, the building block is an ℓ-period difference-in-differences comparison:

$$\mathrm{DID}_{g,\ell} = \left(Y_{g,F_g-1+\ell} - Y_{g,F_g-1}\right) - \mathbb{E}\left[Y_{g',F_g-1+\ell} - Y_{g',F_g-1} \,\middle|\, D_{g',1} = D_{g,1}, F_{g'} > F_g - 1 + \ell, X_{g'} = X_g\right], \tag{3}$$

where $Y_{g,t}$ denotes the outcome for contractor g in year t, $D_{g,t}$ indicates PE-backing status, $D_{g,1}$ is the baseline (period-one) treatment, F_g is the first period when contractor g's status changes, and X_g represents time-invariant contractor characteristics. The first term captures the ℓ-period outcome change for switcher g from just before the treatment change ($F_g - 1$) to ℓ periods after. The second term is the average ℓ-period outcome change for control contractors that share the same baseline treatment, have not yet switched, and belong to the same trend group defined by X_g. The

[5] Implemented via Stata `did_multiplegt_dyn` command.

aggregate event-study effect DID_ℓ averages these comparisons across all switchers for whom the effect can be estimated:

$$\text{DID}_\ell = \frac{1}{N_\ell} \sum_{g:F_g-1+\ell \leq T_g} \text{DID}_{g,\ell}, \tag{4}$$

where N_ℓ is the number of contributing switchers and T_g is the last period contractor g is observed. Since PE backing is non-absorbing, we estimate effects separately for switch-ins (firms entering PE backing) and switch-outs (firms exiting). Our main analysis focuses on switch-ins.

We also report the average total effect per unit of treatment, which aggregates effects across all post-treatment periods weighted by the total treatment increment mass:

$$\delta = \sum_{\ell=1}^{L} w_\ell \, \text{DID}_\ell, \quad \text{where} \quad w_\ell = \frac{N_\ell}{\sum_{g:F_g \leq T_g} \sum_{\ell'=1}^{T_g-F_g+1} (D_{g,F_g-1+\ell'} - D_{g,1})}, \tag{5}$$

where L is the maximum number of post-treatment periods and the denominator sums the total incremental treatment doses received by all switchers across all post-switch periods. This parameter measures the average cumulative effect of one additional unit of treatment, accounting for the fact that treatment effects may persist and accumulate over time.

Critical to our identification strategy is the non-parametric specification of the trend group X_g, which allows for differential trends across contractors based on their government contracting history and recent activity levels.[6] This choice directly responds to the selection patterns documented in Tables II and III. Those results indicate that PE investors—particularly buyout funds—target firms with government contracting experience, but that this targeting reflects operational characteristics (maturity, scale, established agency relationships) rather than government contracting per se. By allowing trends to vary flexibly across bins of contracting tenure and recent activity, we ensure that treatment effects are identified from comparisons of contractors with similar operational profiles. This addresses the concern that estimated PE effects might spuriously capture differential trends across contractor types that both attract PE investment and experience different natural trajectories in government contracting.

To ensure that the $\text{DID}_{g,\ell}$ estimands are well-defined and to minimize the effects of potentially non-random attrition, we require each contractor to have at least seven years of contracting data in the sample, including at least two years before and two years after the deal.

[6] GC spell—*Yrs since 1st contr.*: contractors with 0–2, 2–6, 6–11, 11–16, and 16–25 years since first contract; and recent GC activity—*Prior year contract counts*: contractors with 0, 1–2, 3–9, or 10+ contracts in the prior year.

5. PE backing and government contracting

We apply the dCdH estimator to examine how government contracting activity evolves around PE deals. We consider four outcome dimensions: the number and value of new contracts, contract durations, and contract modifications. For each, we report dynamic event-study effects for up to seven years post-deal, as well as the aggregate total effect defined in equation (5). We estimate effects separately for buyouts and for growth equity and venture capital deals (combined), the latter serving as a benchmark for changes unrelated to buyout ownership.

Table IV reports the estimates. Panels A and B examine the number and value of new contracts, respectively. Because these variables are highly right-skewed and contractor-years without new contracts are coded as zero, we use $\log(1 + x)$ transformations of the raw variables.[7] Panels C and D examine contract durations and contract modifications. All specifications include contractor fixed effects and year-by-contractor-cohort fixed effects, where cohorts are defined along two dimensions: five bins by the number of contracts in the prior year and five bins by the number of years since the first government contract. Standard errors are clustered by contractor.

Panel A reports effects on contract counts. The placebo estimates for years -4 through -1 are small and statistically insignificant, consistent with parallel pre-trends. The first post-buyout year shows a marginally significant positive coefficient (0.028, significant at the 10% level), consistent with a short-lived completion of contracts in the pipeline. From year two onward, the effects turn negative and grow monotonically: the point estimates increase in magnitude from -0.038 in year two to -0.247 by year seven (all significant at the 1% level from year three onward). The total effect is -0.436 (significant at the 1% level), implying a 35% reduction in contract counts ($= 1-\exp(-0.436)$) relative to non-PE-backed contractors. The percentile rank specification in columns (5)–(8) confirms the pattern, with a total effect of -0.115 (significant at the 1% level), corresponding to a decline of roughly 12 percentile points in the cross-sectional distribution of contract counts.

Panel B reports effects on contract values. The results are qualitatively similar but larger in magnitude. The placebo coefficients are generally insignificant, consistent with parallel pre-trends. As with counts, the first post-buyout year shows a positive and significant coefficient (0.280, significant at the 5% level), followed by large and growing declines. The total effect on log contract values is -2.253 (significant at the 1% level), implying an 89% reduction ($= 1 - \exp(-2.253)$).

[7] Cohn, Liu, and Wardlaw (2022) show that $\log(1 + x)$ transformations may generate biases. Figure A1 reports placebo tests with normal random and $\log(1 + \text{random binomial})$ variables to verify that no such bias arises in our sample and setting.

The percentile rank total effect is -0.112 (significant at the 1% level). Figures 2 and 3 plot these event-study estimates. The gradual pattern—insignificant in year one, growing through year seven—suggests deliberate portfolio restructuring rather than immediate liquidation of government business.

To further assess robustness to outliers, we compare the percentile rank results for buyouts against those for growth equity and venture capital (GEVC) deals (combined). As Figures 2 and 3 show, GEVC-backed contractors exhibit no decline in either contract counts or values; if anything, the point estimates suggest slightly higher (though insignificant) growth. This contrast rules out explanations based on general economic conditions or broad governmental policies that would affect all PE-backed contractors equally.

Panel C examines contract durations. The log duration specification shows a total effect of -0.867 (significant at the 1% level), indicating that PE-backed contractors receive shorter contracts post-buyout. The dynamic pattern mirrors Panels A and B: a marginally significant positive effect in year one, followed by increasingly negative effects from year two onward. Columns 5–8 test whether PE backing affects the probability that a contract's duration increases upon modification. Here the total effect is small (0.017) and statistically insignificant, suggesting that the duration reduction reflects changes in the types of contracts awarded rather than renegotiation of existing contracts.

Panel D turns to contract modifications. The modified amount percentile rank shows a total effect of -0.058 (significant at the 1% level), indicating that the contracts of PE-backed contractors receive smaller modifications relative to the cross-sectional distribution. The probability of a dollar amount increase upon modification also declines, with a total effect of -0.073 (significant at the 5% level). These results indicate that PE-backed contractors not only receive fewer and smaller new contracts, but that their existing contracts are also modified less generously. Figure A4 plots the modification results and confirms the absence of pre-trends for buyouts and the lack of any effect for GEVC deals.

Overall, the evidence in Table IV points to a broad-based decline in government contracting activity following buyouts. PE-backed contractors receive fewer contracts, of lower value, for shorter durations, and with smaller subsequent modifications. The effects emerge gradually over the first two to three years and persist through year seven, consistent with a deliberate strategic reorientation rather than an abrupt exit. The absence of any comparable pattern for GEVC-backed contractors suggests that the decline is specific to buyout ownership rather than a general consequence of private capital investment.

6. Why do buyout targets reduce government contracting?

The baseline results establish that buyout targets significantly reduce their government contracting activity post-acquisition. We now examine potential channels behind this reduction. For most cross-sectional tests, we either split the contractor-year sample by observable characteristics or recompute outcomes for contract subsets and re-estimate the dCdH specification.

6.1. Political connections and the rent-extraction hypothesis

If politically connected GPs exploit government relationships for above-market rents, such firms should expand—or at least maintain—their targets' government contracting post-buyout. Figure 4 tests this prediction by splitting the sample based on whether the acquiring GP has political connections (Panels A1 and B1) or federal agency connections (Panels A2 and B2). The results show that connected GPs produce contracting reductions that are at least as large as those of non-connected GPs. This holds for both contract count and value percentile ranks, regardless of whether the connection is political or agency-based.

The point estimates for connected GPs, if anything, indicate steeper declines through year seven. This is a key test of the overpayment hypothesis. If government contracts offered above-market rents, GPs with political or agency connections would be best positioned to capture them—through favorable contract terms, reduced competition, or privileged access to procurement officers. The fact that these investors reduce government business more aggressively than non-connected GPs implies that direct knowledge of government contracting, rather than revealing profit opportunities, reinforces the decision to exit.

Figure A2 confirms that these patterns are not present for growth equity or venture capital deals separately, ruling out broad private-capital effects.

Figure A3 reports additional splits by deal timing (pre- and post-2017), deal size disclosure, debt information availability, and post-deal financial reporting. The contracting decline is broadly similar across these subgroups, suggesting that neither leverage intensity nor deal vintage drives the baseline results.

6.2. Fixed-price versus cost-plus contracts

We next examine whether the decline varies by contract pricing structure and competition intensity. Figure 5 splits the sample along two dimensions: the number of bids received (top versus bottom tercile) and the pricing formula (fixed-price versus cost-plus). Panels A1 and B1 show that the decline in both contract counts and values is concentrated among high-competition contracts (top bid tercile), while low-competition contracts exhibit smaller and less precisely estimated reductions. Panels A2 and B2 reveal a sharp contrast by pricing formula: fixed-price contracts decline substantially, whereas cost-plus contracts show no significant change.

Table V formalizes these patterns. In Panel A (contract counts), the total effect for fixed-price contracts is −0.453 (significant at the 1% level), while the total effect for cost-plus contracts is −0.039 and statistically insignificant. Panel B (contract value percentile ranks) confirms the same pattern: a total decline of 0.134 in percentile rank for fixed-price contracts (significant at the 1% level) versus 0.043 for cost-plus contracts (insignificant).

The fixed-price versus cost-plus distinction is central to our interpretation. Under fixed-price contracts, the contractor commits to deliver at a predetermined price and bears the full risk of cost overruns; margins therefore depend on the contractor's ability to control costs and on the intensity of competition at the bidding stage. Under cost-plus contracts, the government reimburses allowable costs and pays an additional fee, largely insulating the contractor from cost risk. The selective exit from fixed-price contracts suggests that PE-backed firms view these margins as insufficient to justify the operational resources required, while the stability of cost-plus contracts is consistent with their guaranteed-fee structure making them worth retaining.

This pattern also helps distinguish between competing explanations. If regulatory burden drove the post-buyout exit, we would expect buyout targets to reduce all contract types, since disclosure and compliance requirements apply regardless of pricing formula. Instead, the decline concentrates in contracts where the contractor bears cost risk and margins are compressed by competitive bidding, while contracts with guaranteed reimbursement remain stable. The evidence thus points to a profitability-based explanation: buyout targets exit government business that offers thin returns under competitive conditions, not government business per se.

The preceding analysis splits contracts by pricing formula within each contractor. A complementary test asks whether the exit pattern varies across firms based on the composition of their pre-buyout contract portfolios. If profitability drives the post-buyout exit, contractors whose government business consists predominantly of fixed-price contracts – where margins are thinnest – should be most likely to exit entirely, since the fixed overhead of maintaining government contracting capability is least justified when the underlying contracts offer compressed returns.

We classify each contractor as a *fixed-price contractor* if more than half of its government contracts were fixed-price during the 3-years preceding the buyout, and estimate a standard two-way fixed effects specification with contractor and year-by-cohort fixed effects on the full contractor-year panel. We proxy for exits with an indicator of whether the contractor received any new contracts during the year. Table VI reports the results.

Column (1) establishes the baseline: buyout-backed contractors are 1.4 percentage points less likely to sign any government contract in a given year (significant at the 1% level). Column (2) introduces the interaction between the buyout and the fixed-price contractor indicators. The interaction coefficient is significant at the 1% level, indicating that contractors with predominantly fixed-price portfolios are 5.5 percentage points more likely to exit government contracting entirely following a buyout, relative to contractors with non-fixed-price portfolios. The model suggests that non-fixed-price contractors are 3.6 percentage points more likely to have new contracts in a given year post-buyout, although the coefficient is significant only at the 10% level. Column (3) replaces Buyout with GE/VC deal indicator as a falsification test. The interaction is positive and insignificant, confirming that the fixed-price exit gradient is specific to buyouts. Column (4) shows that the dollar-value of newly established or modified contracts also decline only for fixed-price contractors: the coefficient on the interaction is -0.634 (significant at the 5% level), while the coefficient on the post-buyout indicator is 0.445 (significant at the 10% level). Finally, column (5) examines cream-skimming among contractors that maintain government business: conditional on remaining an active contractor, buyout-backed firms shift marginally toward cost-plus contracts (coefficient 0.007, significant at the 10% level), consistent with selective retention of higher-margin relationships with the government.

These results reinforce the profitability interpretation from two directions. First, the extensive-margin gradient—fixed-price contractors exiting at nearly 6 percentage points higher rates— cannot be explained by regulatory burden, since FAR disclosure and compliance requirements apply identically regardless of contract pricing formula. The only dimension that varies across these firms is the economics of their government business: whether the contractor bears cost risk under competitive bidding (fixed-price) or ensures that at least the costs are reimbursed. Second, the cream-skimming result shows that buyout sponsors do not simply abandon government contracting indiscriminately. Among firms that stay, the surviving portfolio tilts toward the contract types that offer likely better margins. Together with the intensive-margin results in Table V, these patterns indicate that buyout investors optimize along the profitability gradient – exiting thin-margin relationships entirely while retaining those that justify the compliance overhead.

6.3. Year-end modifications and rent-seeking

Contract modifications represent an additional margin through which PE-backed firms may interact with the government. Figure A4 plots event-study estimates for modifications around PE deals and confirms that buyout targets experience declining modification amounts and no change in the probability of favorable duration modifications, consistent with the baseline results in Table IV, Panels C and D. GEVC-backed contractors again show no comparable pattern.

Figure 6 examines contract modifications around the end of the federal fiscal year (September 30), split by GP connectedness and the number of bidders. Prior work has documented a surge in year-end spending as agencies exhaust expiring budgets (Liebman and Mahoney, 2017), making this the setting where rent-seeking by connected GPs, if present, should be most visible. If such firms extract rents through favorable contract renegotiations, this behavior should concentrate in low-competition, year-end modifications by connected GPs—the "lucrative and informed" quadrant (Panel A, top left). The data show a modestly positive but imprecisely estimated pattern in this quadrant. In contrast, connected GPs managing high-competition contracts modified in the last two weeks of September show significant declines (Panel A, bottom left). Non-connected GPs managing competitive year-end contracts show relatively flat patterns near zero (Panel A, bottom right).

Table VII formalizes these patterns by reporting total effects over six years post-buyout for contract subsamples defined by fiscal year timing, competition intensity, and GP connectedness. For the combination most conducive to rent extraction—low-competition contracts modified in the last two weeks of September by politically connected GPs—the total effect is positive (0.204) but only marginally significant and economically modest. Even under the most favorable conditions for exploiting government relationships, connected GPs do not meaningfully expand their targets' contract renegotiation activity.

The more striking finding is on the other side of the competition spectrum. Connected GPs managing high-competition contracts modified in the last two weeks of September show a total effect of -0.544 (significant at the 5% level). These firms actively reduce modifications on competitively bid contracts precisely during the period when year-end budget pressure would make such modifications easiest to obtain. Non-connected GPs show no significant effects in either direction, and the October placebo period shows no significant patterns for any group. Panel B reports analogous results for federal agency connections, with qualitatively similar findings.

The combination of these results reinforces the interpretation from Section 6.1. Connected GPs do not use their political access to secure favorable year-end renegotiations; instead, they disproportionately exit the competitive contracts where margins are thinnest. This is consistent with informed investors recognizing that competitive government contracting offers limited returns, and reallocating resources accordingly.

6.4. GP experience and target renegotiation skill

To understand which buyout targets drive the average treatment effects, we estimate cross-sectional regressions relating post-buyout changes in government contracting to pre-acquisition characteristics of both the acquiring GP and the target firm. For each buyout target, we compute the forward change in contract count and value percentile ranks over two to three years post-deal and regress these changes on GP and target characteristics, controlling for firm age, listing status, industry, and deal year fixed effects (equations 6 and 7). Standard errors are clustered by industry.

For each target g and time horizon $h \in \{2, 3\}$ years, we compute the forward change in outcomes:

$$\Delta^h Y_g = Y_{g,F_g+h} - Y_{g,F_g} \quad , \tag{6}$$

where $Y_{g,t}$ represents the outcome (contract count or value percentile rank) for contractor g in year t, and F_g is the year of the buyout. We then estimate:

$$\Delta^h Y_g = \alpha + \beta^{\text{GP}} X_g^{\text{GP}} + \beta^{\text{Target}} X_g^{\text{Target}} + \gamma Z_g + \varepsilon_g, \tag{7}$$

where X_g^{GP} includes characteristics of the acquiring general partner (e.g., prior experience investing in government contractors, political connections, fund characteristics), X_g^{Target} includes pre-acquisition characteristics of the target firm (e.g., government contracting tenure, recent contracting volume, past deal history), and Z_g includes additional controls (firm age, listing status, industry, deal year). Standard errors are clustered by industry.

Panel A of Table VIII focuses on GP characteristics. The key regressor, $\mathbb{I}(\text{GC-deals 1-to-5y before})_g$, indicates whether the GP completed at least one deal involving a government contractor in the five years preceding the current buyout. Across all specifications, this indicator enters with negative and significant coefficients (-0.040 to -0.047 for contract count changes, significant at the 5% level). GPs that previously invested in government contractors thus produce larger reductions in their current targets' government business. This finding suggests a learning mechanism: experience with government contracting does not reveal hidden profit opportunities but instead confirms the limited upside of this business, leading experienced GPs to implement larger reductions in subsequent deals.

Two additional GP-level variables provide nuance. An indicator for whether the GP's past government contractor-involved deals experienced contract value increases enters positively (0.043 in column 2, significant at the 10% level), suggesting that GPs whose prior targets succeeded in growing government business are somewhat less likely to reduce it in subsequent deals. The GP's industry preference for top government contracting industries also enters positively for contract counts (0.028, significant at the 5% level). These results indicate that while the average experienced GP reduces government contracting, GPs with a demonstrated track record of growing this business are more selective in doing so.

Panel B turns to the target's pre-acquisition characteristics, focusing on the target's track record in contract renegotiation. Brogaard, Denes, and Duchin (2021) show that politically-connected public firms improve the profitability of their government contract portfolios by negotiating up the initial amounts and postponing deadlines. Accordingly, our key variable, $\mathbb{I}(> 3 \text{ GCs mod.\$ \& dur } \uparrow \text{ in } t_{d-3:d-1})_g$, indicates whether the target had more than three contracts with both dollar amount and duration increases upon modification in the three years before the buyout—a proxy for the target's ability to extract favorable terms from the government. This indicator enters positively and significantly: 0.081 for contract count changes (significant at the 5% level) and 0.171 for contract value changes (significant at the 1% level). Targets that demonstrated skill in securing favorable modifications pre-acquisition experience substantially smaller declines in government contracting post-buyout.

This result is consistent with GPs evaluating government contracts on their individual economic merits. Targets with a demonstrated ability to renegotiate contracts on favorable terms possess a competitive advantage that makes their government relationships worth retaining. Targets without such advantages see their government business reduced more aggressively, consistent with PE firms redirecting resources toward higher-return activities.

Together, the cross-sectional results paint a picture of strategic portfolio optimization rather than indiscriminate exit. Experienced GPs produce larger reductions, indicating that familiarity with government contracting reinforces the decision to scale back. At the same time, targets with demonstrated renegotiation skill retain more of their government business. GPs thus appear to distinguish between government relationships that offer defensible margins and those that do not—keeping the former and exiting the latter.

7. Conclusions

This paper uses private equity buyouts as a revealed-preference test for whether the U.S. federal government systematically overpays contractors. If government contracts offered above-market rents, profit-maximizing PE investors should expand this business after acquiring government contractors. We find the opposite. GPs specifically target government contractors for acquisition—contracting experience is a first-order predictor of buyout involvement—but then reduce the number, value, and duration of government contracts by economically large magnitudes over the seven years following the deal.

Several findings narrow the set of explanations for this pattern. The decline concentrates in fixed-price contracts, where contractors bear cost risk and margins depend on competitive bidding, while cost-plus contracts with guaranteed reimbursement remain stable. Politically connected PE firms—those best positioned to exploit government relationships—reduce government contracting at least as aggressively as non-connected firms. Even in the setting most conducive to rent extraction—low-competition, year-end contract modifications by connected GPs—we find no evidence of expansion. Cross-sectional analysis reveals that experienced PE investors who previously acquired government contractors produce the largest reductions, while targets with demonstrated skill in contract renegotiation experience smaller declines. These patterns are consistent with PE firms selectively exiting government business that offers thin margins under competitive conditions, while retaining relationships where targets possess defensible advantages in securing favorable terms.

Our results have implications for the debate on government spending efficiency. The finding that sophisticated, profit-maximizing investors voluntarily exit government contracting after acquiring contractors suggests that government procurement does not, on average, offer the above-market rents that critics of government waste often assume. If anything, the evidence indicates that government contracting margins are sufficiently compressed—particularly for fixed-price contracts subject to competitive bidding—that PE firms reallocate their targets' resources toward more profitable private-sector opportunities. This interpretation does not imply that government procurement is free of inefficiency, but it does challenge the premise that the government systematically overpays for the goods and services it purchases.

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Table I
Summary Statistics

This table describes the sample of Pitchbook deals in Panel A, buyout GPs in Panel B, and government contractors (GCs) in Panel C. Contract counts are multiples of 10, while contract amounts are of USDmln. The data constructions steps are described in Section 2 and Appendix A1. Appendix A3 summarizes the variables.

Panel A: Pitchbook deals

	Investee Co age				Disclosed deal size				GCs last 3 years (excl. 0s)			
	non-miss	mean	p25	p75	non-miss	mean	p25	p75	non-miss	mean	p25	p75
BO	8,616	37.15	19.0	47.0	1,462	926.16	50.0	900.0	3,615	53.60	3.0	15.0
GE	2,575	31.61	13.0	38.0	901	240.29	6.4	128.2	1,086	187.47	3.0	16.0
VC	6,294	8.44	4.0	11.0	5,594	29.56	2.4	23.0	2,496	8.90	2.0	6.0
MnA	9,471	36.47	16.0	47.0	3,513	894.90	18.0	323.0	3,896	52.55	3.0	15.0
PIPE	730	39.28	17.0	47.0	519	280.33	6.7	199.0	227	33.21	2.0	18.0
IPO	26	18.08	7.0	19.0	26	363.35	67.2	468.0	10	27.50	4.0	28.0

Panel B: Buyout GP characteristics (N=1,214)

	mean	sd	p10	p25	p50	p75	p90
All deals completed this year	7.37	10.06	1.00	2.00	4.25	8.30	15.80
Deals feat. GCs this year	0.59	0.70	0.00	0.00	0.50	1.00	1.33
Frac. of deals feat. GCs	0.15	0.23	0.00	0.00	0.07	0.17	0.43
Funds raised over last 5y	2.02	2.50	1.00	1.00	1.25	2.00	3.40
Ave. fund size (USD bln.)	0.66	1.07	0.03	0.11	0.28	0.66	1.67
I(Polit-connected GP)	0.19						
I(FedAg-connected GP)	0.16						
I(Congr-connected GP)	0.09						

Panel C: Government contractors (averages over the sample spell)

	Deal in PB (N=6,208)						Never treated (N=221,060)					
	mean	sd	p5	p10	p90	p95	mean	sd	p5	p10	p90	p95
# of new contracts	1.63	44.81	0.0	0.0	0.7	1.3	0.50	23.02	0.0	0.0	0.3	0.6
Yr.Tot initial amt	0.79	5.06	0.0	0.0	1.1	2.9	0.36	10.00	0.0	0.0	0.2	0.7
Yr.Tot modif. amt	0.09	0.89	−0.0	−0.0	0.1	0.3	0.03	0.99	−0.0	−0.0	0.0	0.0
Av. init. duration	7.67	9.02	0.3	0.5	19.3	24.8	4.84	8.44	0.0	0.1	12.3	18.5
Av. growth if mod.	0.08	0.25	−0.1	−0.1	0.3	0.5	0.03	0.18	−0.1	−0.1	0.1	0.3
Av. dur. chge if mod.	4.21	7.31	−2.4	−0.1	15.0	21.4	2.05	5.31	−1.2	0.0	7.8	13.4
Yrs since 1st contr.	8.36	4.78	1.8	2.2	15.0	16.0	6.31	4.67	1.3	1.3	13.5	15.5

Table II
Do government contractors attract buyouts more than other deals?

This table reports linear probability model estimates of the deal being classified as buyout. The sample (see Table I) include deals that Pitchbook classifies as buyouts, growth/expansion, venture capital, merger/acquisition and reverse merger, exit financing, PIPE, IPO, and secondary transaction. The variables are described in Section 2 with summary appearing in Appendix A3. All specifications include the deals' year and industry fixed effects. T-statistic in parentheses are based on standard errors double-clustered by industry and year.

	I(the deal is buyout)				I(is growth)		I(is VC)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
log(1+GCamount)	0.177**	0.155**	0.172**	0.157**	0.034	0.041	−0.009
	(2.43)	(2.20)	(2.77)	(2.42)	(0.76)	(0.91)	(−0.12)
log(1+past3y-GCcount)	1.084**	0.447	0.876**	0.236	−0.090	0.148	−3.025***
	(2.81)	(1.33)	(2.44)	(0.74)	(−0.94)	(1.34)	(−4.90)
2-to-5y of GCs		−0.009		0.024		−0.003	
		(−0.51)		(1.53)		(−0.46)	
5-to-9y of GCs		0.021		0.031		−0.006	
		(1.14)		(1.66)		(−1.11)	
10y+ of GCs		0.041**		0.031**		−0.015***	
		(2.80)		(2.44)		(−4.93)	
log(Company Age)	0.154***	0.148***	0.124**	0.122**	−0.061***	−0.059***	−0.356***
	(3.21)	(3.18)	(2.78)	(2.79)	(−7.09)	(−7.40)	(−5.22)
Past 5y Deal count:							
VC			−0.049***	−0.052***	0.036***	0.036***	0.139***
			(−5.53)	(−6.15)	(7.38)	(7.33)	(9.92)
Buyout			0.166***	0.159***	−0.007	−0.005	−0.044**
			(10.92)	(10.88)	(−1.06)	(−0.66)	(−2.54)
Growth			0.094***	0.090***	0.026*	0.028*	−0.022
			(3.69)	(3.59)	(1.98)	(2.10)	(−1.65)
Recap			0.091***	0.089***	−0.001	−0.000	0.012
			(5.60)	(5.65)	(−0.06)	(−0.02)	(0.58)
M&A			−0.033	−0.040	0.006	0.008	−0.082***
			(−0.79)	(−0.93)	(0.38)	(0.54)	(−3.33)
PIPE			−0.031**	−0.034**	−0.002	−0.001	0.045**
			(−2.36)	(−2.58)	(−0.23)	(−0.12)	(2.96)
IPOs			0.000	−0.005	0.013	0.014	−0.123***
			(0.00)	(−0.12)	(0.60)	(0.59)	(−4.51)
Pr{GC$=0,GC#=0}	0.010	0.022	0.004	0.033	0.567	0.070	0.000
within R-squared	0.009	0.010	0.021	0.022	0.016	0.017	0.143
overall R-squared	0.045	0.046	0.057	0.057	0.037	0.038	0.253
Observations	27,711	27,711	27,711	27,711	27,711	27,711	27,711

Table III
Buyout investors characteristics and contractor targeting

This table reports linear probability model estimates of the buyout target having a government contact in the buyout year. The sample (see Table I) include deals that Pitchbook classifies as buyouts. The variables are described in Section 2 with summary appearing in Appendix A3. All specifications include the deals' year and industry fixed effects. T-statistic in parentheses are based on standard errors double-clustered by industry and year.

	I(buyout target has GCs this year)					
	(1)	(2)	(3)	(4)	(5)	(6)
log(Deals this year)	−0.001		−0.001	0.001		0.000
	(−0.10)		(−0.16)	(0.12)		(0.08)
log(Funds last 5y)	0.006		0.006	−0.004		−0.004
	(1.05)		(0.87)	(−0.70)		(−0.71)
log(Fund avSize 5y)	0.000		−0.001	−0.005		−0.006
	(0.09)		(−0.23)	(−1.19)		(−1.37)
I(Polit-connected GP)		−0.015	−0.014		−0.010	−0.007
		(−1.48)	(−1.20)		(−0.77)	(−0.51)
I(FedAg-connected GP)		0.028**	0.025*		0.014	0.019
		(2.13)	(2.00)		(1.34)	(1.70)
I(Congr-connected GP)		0.003	0.000		−0.005	−0.004
		(0.26)	(0.01)		(−0.52)	(−0.42)
I(GP is listed)	0.000	0.006	−0.002	0.010	0.001	0.009
	(0.02)	(0.33)	(−0.10)	(0.56)	(0.03)	(0.50)
Company's age & listing	Yes	Yes	Yes	Yes	Yes	Yes
Past 5y Deal counts	No	No	No	Yes	Yes	Yes
Pr{F-test xControls}	0.763	0.190	0.606	0.335	0.556	0.452
within R-squared	0.000	0.001	0.001	0.075	0.073	0.075
overall R-squared	0.030	0.030	0.031	0.103	0.100	0.103
Observations	10,371	10,790	10,371	10,371	10,790	10,371

Table IV
Buyout effects on government contracting

This table reports dynamic difference-in-difference estimates of De Chaisemartin and d'Haultfoeuille (2024) (dCdH) of buyout transaction on the targets government contracts. The fixed effects across all panels are contractor- and year-by-(contractor cohort). The contractor cohorts span two dimensions: 5 bins by the number of contracts last year and 5 bins by the number of years post-2000 since the first government contract. The sample include government contractors as described in Table I with at least 7 years of (possibly intermittent) government contracting activity, including at least 2 years before and after the deal. The variables are described in Section 2 with summary appearing in Appendix A3. Standard errors are clustered by contractor.

Panel A: Number of new contracts

	log(1 + contracts count)				Contract perct. rank (0,1)			
	Coef. (1)	T-stat. (2)	N total (3)	N switch. (4)	Coef. (5)	T-stat. (6)	N total (7)	N switch. (8)
Placebo(-4y)	0.0140	0.44	455,272	1,143	0.0154	1.42	455,272	1,143
Placebo(-3y)	−0.0174	−0.81	687,145	1,877	−0.0025	−0.33	687,145	1,877
Placebo(-2y)	−0.0102	−0.52	916,951	2,173	0.0033	0.48	916,951	2,173
Placebo(-1y)	−0.0140	−0.89	1,150,361	2,453	0.0058	1.00	1,150,361	2,453
Effect(+1y)	0.0276*	1.83	1,289,796	2,639	0.0104*	1.93	1,289,796	2,639
Effect(+2y)	−0.0382**	−2.15	1,209,073	2,617	−0.0155**	−2.47	1,209,073	2,617
Effect(+3y)	−0.1059***	−5.59	1,101,197	2,572	−0.0273***	−4.16	1,101,197	2,572
Effect(+4y)	−0.1613***	−6.54	943,418	1,923	−0.0413***	−5.00	943,418	1,923
Effect(+5y)	−0.1533***	−5.35	788,357	1,466	−0.0370***	−3.88	788,357	1,466
Effect(+6y)	−0.1957***	−5.99	669,344	1,215	−0.0538***	−4.94	669,344	1,215
Effect(+7y)	−0.2466***	−6.47	543,412	955	−0.0658***	−5.13	543,412	955
Total Effect	−0.4360***	−5.98	1,416,191	13,387	−0.1152***	−4.66	1,416,191	13,387

Panel B: Value of new contracts

	log(1 + initial $amount)				Initial $amount perct. rank (0,1)			
	Coef. (1)	T-stat. (2)	N total (3)	N switch. (4)	Coef. (5)	T-stat. (6)	N total (7)	N switch. (8)
Placebo(-4y)	−0.0374	−0.19	455,272	1,143	0.0032	0.30	455,272	1,143
Placebo(-3y)	−0.2308	−1.60	687,145	1,877	−0.0115	−1.51	687,145	1,877
Placebo(-2y)	−0.1393	−1.02	916,951	2,173	−0.0075	−1.05	916,951	2,173
Placebo(-1y)	−0.1990*	−1.71	1,150,361	2,453	−0.0052	−0.86	1,150,361	2,453
Effect(+1y)	0.2802**	2.52	1,289,796	2,639	0.0095*	1.65	1,289,796	2,639
Effect(+2y)	−0.2656**	−2.08	1,209,073	2,617	−0.0163**	−2.48	1,209,073	2,617
Effect(+3y)	−0.6687***	−5.18	1,101,197	2,572	−0.0276***	−4.02	1,101,197	2,572
Effect(+4y)	−0.9721***	−6.22	943,418	1,923	−0.0449***	−5.31	943,418	1,923
Effect(+5y)	−0.6526***	−3.60	788,357	1,466	−0.0305***	−3.11	788,357	1,466
Effect(+6y)	−0.9268***	−4.77	669,344	1,215	−0.0437***	−3.98	669,344	1,215
Effect(+7y)	−1.1833***	−5.22	543,412	955	−0.0653***	−5.20	543,412	955
Total Effect	−2.2534***	−4.95	1,416,191	13,387	−0.1117***	−4.51	1,416,191	13,387

Table IV
Buyout effects on government contracting

(Continued from the previous page)

Panel C: Average contract durations

	log(1 + initial contract duration)				Pr{duration increase upon modification}			
	Coef. (1)	T-stat. (2)	N total (3)	N switch. (4)	Coef. (5)	T-stat. (6)	N total (7)	N switch. (8)
Placebo(-4y)	−0.0410	−0.51	455,271	1,143	0.0010	0.16	443,870	1,211
Placebo(-3y)	−0.1337**	−2.09	687,143	1,877	0.0083	1.63	670,099	1,850
Placebo(-2y)	−0.1042*	−1.79	916,948	2,173	0.0053	1.15	881,225	2,211
Placebo(-1y)	−0.0679	−1.30	1,150,358	2,453	0.0032	0.78	1,112,510	2,611
Effect(+1y)	0.0923*	1.85	1,289,794	2,639	0.0077*	1.84	1,249,089	2,981
Effect(+2y)	−0.1492***	−2.65	1,209,071	2,617	0.0038	0.86	1,170,525	2,976
Effect(+3y)	−0.2873***	−5.07	1,101,195	2,572	0.0073	1.59	1,083,713	2,965
Effect(+4y)	−0.3694***	−5.49	943,416	1,923	−0.0009	−0.18	932,491	2,545
Effect(+5y)	−0.2227***	−2.81	788,356	1,466	0.0070	1.23	784,450	2,123
Effect(+6y)	−0.3179***	−3.82	669,343	1,215	−0.0022	−0.35	655,186	1,780
Effect(+7y)	−0.3032***	−3.09	543,411	955	−0.0091	−1.40	537,519	1,477
Total Effect	−0.8663***	−4.33	1,416,190	13,387	0.0168	0.95	1,407,569	16,847

Panel D: Modification amounts

	Modified amount rank (0,1)				Pr{$amount increase upon modification}			
	Coef. (1)	T-stat. (2)	N total (3)	N switch. (4)	Coef. (5)	T-stat. (6)	N total (7)	N switch. (8)
Placebo(-4y)	−0.0174*	−1.89	356,003	857	−0.0016	−0.13	455,272	1,143
Placebo(-3y)	−0.0142**	−2.34	545,421	1,417	−0.0169*	−1.86	687,145	1,877
Placebo(-2y)	−0.0048	−0.82	737,983	1,620	−0.0037	−0.44	916,951	2,173
Placebo(-1y)	0.0008	0.15	940,489	1,842	0.0002	0.03	1,150,361	2,453
Effect(+1y)	0.0032	0.67	1,120,544	2,181	0.0034	0.45	1,289,796	2,639
Effect(+2y)	−0.0087*	−1.68	1,046,061	2,172	−0.0174**	−2.24	1,209,073	2,617
Effect(+3y)	−0.0147***	−2.84	950,316	2,148	−0.0196**	−2.52	1,101,197	2,572
Effect(+4y)	−0.0167**	−2.55	809,659	1,608	−0.0191**	−1.98	943,418	1,923
Effect(+5y)	−0.0190**	−2.40	667,962	1,198	−0.0149	−1.29	788,357	1,466
Effect(+6y)	−0.0285***	−3.41	565,307	986	−0.0393***	−3.36	669,344	1,215
Effect(+7y)	−0.0287***	−2.88	462,505	770	−0.0265*	−1.92	543,412	955
Total Effect	−0.0578***	−3.03	1,304,281	11,063	−0.0729**	−2.43	1,416,191	13,387

Table V
Buyout effects and contract price formula

This table reports dynamic difference-in-difference estimates of dCdH of buyout transaction on the targets government contracts by type of bidding accepted—'Fixed Price' or 'Cost plus'. The fixed effects across all panels are contractor- and year-by-(contractor cohort). The contractor cohorts span two dimensions: 5 bins by the number of contracts last year and 5 bins by the number of years post-2000 since the first government contract. The sample include government contractors as described in Table I with at least 7 years of (possibly intermittent) government contracting activity, including at least 2 years before and after the deal. The variables are described in Section 2 with summary appearing in Appendix A3. Standard errors are clustered by contractor.

Panel A: Number of new contracts (log1plus)

	Fixed price				Cost plus			
	Coef. (1)	T-stat. (2)	N total (3)	N switch. (4)	Coef. (5)	T-stat. (6)	N total (7)	N switch. (8)
Placebo(-4y)	0.0041	0.15	443,179	1,115	0.0374	1.07	48,562	91
Placebo(-3y)	−0.0503**	−2.58	628,334	1,727	0.0054	0.19	71,215	109
Placebo(-2y)	−0.0001	−0.00	791,973	1,939	0.0491**	2.06	89,458	137
Placebo(-1y)	0.0021	0.15	926,819	2,151	−0.0080	−0.59	109,344	178
Effect(+1y)	0.0198	1.30	1,025,942	2,178	0.0226	1.00	131,372	202
Effect(+2y)	−0.0604***	−3.30	969,241	2,155	0.0045	0.17	124,283	176
Effect(+3y)	−0.1067***	−5.38	897,701	2,125	−0.0262	−0.83	119,366	172
Effect(+4y)	−0.1624***	−6.74	803,424	1,630	−0.0409	−1.58	100,177	158
Effect(+5y)	−0.1495***	−5.44	709,691	1,279	0.0056	0.16	87,065	109
Effect(+6y)	−0.1896***	−6.00	616,786	1,078	−0.0390	−1.43	75,625	97
Effect(+7y)	−0.1865***	−5.08	524,107	859	0.0014	0.04	61,687	78
Total Effect	−0.4528***	−6.06	1,099,463	11,304	−0.0388	−0.46	162,708	992

Panel B: Value of new contracts rank (0,1)

	Fixed price				Cost plus			
	Coef. (1)	T-stat. (2)	N total (3)	N switch. (4)	Coef. (5)	T-stat. (6)	N total (7)	N switch. (8)
Placebo(-4y)	−0.0073	−0.63	443,179	1,115	0.0185	0.74	48,562	91
Placebo(-3y)	−0.0282***	−3.20	628,334	1,727	0.0199	0.85	71,215	109
Placebo(-2y)	−0.0042	−0.53	791,973	1,939	0.0419*	1.86	89,458	137
Placebo(-1y)	0.0017	0.25	926,819	2,151	−0.0012	−0.12	109,344	178
Effect(+1y)	0.0116*	1.67	1,025,942	2,178	0.0144	1.01	131,372	202
Effect(+2y)	−0.0232***	−2.95	969,241	2,155	0.0069	0.50	124,283	176
Effect(+3y)	−0.0357***	−4.25	897,701	2,125	−0.0266*	−1.94	119,366	172
Effect(+4y)	−0.0541***	−5.45	803,424	1,630	−0.0267	−1.53	100,177	158
Effect(+5y)	−0.0362***	−3.21	709,691	1,279	−0.0222	−1.06	87,065	109
Effect(+6y)	−0.0477***	−3.75	616,786	1,078	−0.0250	−1.20	75,625	97
Effect(+7y)	−0.0575***	−4.10	524,107	859	−0.0001	−0.00	61,687	78
Total Effect	−0.1342***	−4.43	1,099,463	11,304	−0.0425	−0.81	162,708	992

Table VI
Contract pricing and buyout exits from government contracting

This table examines whether buyout exit from government contracting concentrates among firms with thin-margin contract portfolios via a linear probability model. The outcome in columns (1)–(3) is an indicator equal to one if the firm has at least one government contract in year t. Column (4) uses log of 1 plus sum contract values during the year, both initial and modified. Column (5) uses an indicator for Cost-plus pricing conditional on the new contract received during that year. $\mathbb{I}$(Fixed-price contractor) equals one if the contractor's portfolio was majority fixed-price during 3 years pre-buyout (as of the sample-end for never treated). Column (3) replaces indicator $\mathbb{I}$(Buyout) with $\mathbb{I}$(GE/VC) as a falsification test. Fixed effects are contractor and year-by-(contractor cohort). The contractor cohorts are as in Tables IV–V and span two dimensions: 5 bins by the number of contracts last year and 5 bins by the number of years post-2000 since the first government contract. T-statistics reported in parentheses and based on standard errors clustered by contractor. ***/**/* denote significance at 1/5/10% confidence level.

	$\mathbb{I}$(Any contract)			ln(1+$Value)	$\mathbb{I}$(Cost-plus)
	(1)	(2)	(3)	(4)	(5)
$\mathbb{I}$(Buyout)	−0.014***	0.036*		0.445*	0.007*
	(−2.89)	(1.88)		(1.67)	(1.87)
$\mathbb{I}$(Buyout) × $\mathbb{I}$(Fixed-price contractor)		−0.055***		−0.634**	
		(−2.76)		(−2.32)	
$\mathbb{I}$(GE/VC)			0.018		
			(0.70)		
$\mathbb{I}$(GE/VC) × $\mathbb{I}$(Fixed-price contractor)			0.009		
			(0.33)		
Sample	All	All	All	All	Active
Contractor FE	Yes	Yes	Yes	Yes	Yes
Year × cohort FE	Yes	Yes	Yes	Yes	Yes
Observations	1,849,353	1,849,353	1,849,353	1,849,353	635,911

Table VII
Political connections and end-of-year contract renegotiation

This table reports the total effects (average cumulative effect per unit of treatment) estimates over 6 years post-buyout from dynamic difference-in-difference estimates of dCdH by number of bidders for the contract, time of the year modification took place, and political (or federal agency) connectedness of the deal's GP. The dependent variable is natural log of 1 plus modified amount of the contract. If no modification took place during the specified period, the outcome is coded as zero. The estimation is performed on the firm-year panel that includes contractors with at least 6 years in the sample and at least one year before and after the buyout. Each raw corresponds to a separate regression over the contract sample as indicated in the first column. Lw = last week, L2w = last 2 weeks. Standard errors are clustered by contractor. ***/**/* denote significance at 1/5/10% confidence level.

Contracts subset	Total effect	T-statistic	p-value	N total	N switch
Panel A: GP political connectedness					
Sep Lw < 2 bids: PolConYes	0.1480	1.36	0.173	1,339,548	4,364
Sep L2w < 2 bids: PolConYes	0.2039*	1.77	0.076	1,338,122	4,345
Sep < 2 bids: PolConYes	0.2341	1.28	0.201	1,331,249	4,291
Sep L2w < 2 bids: PolConNo	−0.0006	−0.01	0.995	1,352,268	9,640
Sep Lw > 4 bids: PolConYes	−0.4023*	−1.79	0.074	1,337,937	4,372
Sep L2w > 4 bids: PolConYes	−0.5443**	−2.13	0.033	1,336,348	4,359
Sep > 4 bids: PolConYes	−0.4811*	−1.77	0.077	1,328,974	4,326
Sep L2w > 4 bids: PolConNo	−0.1125	−0.98	0.327	1,350,645	9,667
Oct L2w: PolConYes	−0.2746*	−1.67	0.096	1,336,894	4,372
Oct L2w: PolConNo	0.0635	0.85	0.396	1,351,094	9,685
Panel A: GP Federal agency connectedness					
Sep Lw < 2 bids: FAconYes	0.0611	0.69	0.488	1,346,621	5,592
Sep L2w < 2 bids: FAconYes	0.1274	1.37	0.171	1,345,156	5,572
Sep < 2 bids: FAconYes	0.1286	0.87	0.386	1,338,209	5,484
Sep L2w < 2 bids: FAconNo	0.0118	0.12	0.904	1,346,652	8,380
Sep Lw > 4 bids: FAconYes	−0.2874	−1.59	0.111	1,345,319	5,611
Sep L2w > 4 bids: FAconYes	−0.4510**	−2.21	0.027	1,343,745	5,596
Sep > 4 bids: FAconYes	−0.3960*	−1.74	0.082	1,336,362	5,568
Sep L2w > 4 bids: FAconNo	−0.1362	−1.09	0.274	1,344,983	8,393
Oct L2w: FAconYes	−0.0450	−0.39	0.697	1,343,966	5,590
Oct L2w: FAconNo	−0.0309	−0.34	0.735	1,345,501	8,434

Table VIII
What characteristics explain changes in GC buyout targets?

This table reports cross-sectional regression analysis of changes in the GC count and $ value for the sample of treated firms in table IV. The changes are over 3 (if non-missing) or 2 years and in percentile of the respective quantity relative to the full sample of treated and untreated firm-years. The variables are described in Section 2 with summary appearing in Appendix A3. Standard errors are clustered by industry.

Panel A: GP past experience

	Pctle(GC count) 2-3y Chge			Pctle(GC value) 2-3y Chge		
	(1)	(2)	(3)	(4)	(5)	(6)
I(GC-deals 1-to-5y before)	−0.040**	−0.047**	−0.046**	−0.023	−0.037*	−0.036*
	(−2.19)	(−2.32)	(−2.19)	(−1.54)	(−1.96)	(−1.93)
I(past deals av. GC-value↑)		0.043*	0.041*		0.060**	0.060**
		(1.83)	(1.70)		(2.20)	(2.17)
I(GP pref top-GC ind)	0.028**		0.026*	0.012		0.010
	(2.40)		(1.93)	(0.65)		(0.49)
I(GP is listed)	−0.006	−0.013	−0.007	−0.015	−0.017	−0.015
	(−0.15)	(−0.34)	(−0.20)	(−0.39)	(−0.39)	(−0.36)
Targets's past GC experience:						
2-to-5y of GCs		0.028	0.031		0.056	0.057
		(0.75)	(0.84)		(1.11)	(1.16)
5-to-9y of GCs		0.011	0.014		0.032	0.033
		(0.22)	(0.27)		(0.49)	(0.51)
10y+ of GCs		0.000	0.002		0.021	0.022
		(0.01)	(0.05)		(0.40)	(0.42)
1-to-2 GCs prev. year		−0.063**	−0.063**		−0.062***	−0.062***
		(−2.61)	(−2.65)		(−2.82)	(−2.81)
3-to-10 GCs prev. year		−0.161***	−0.161***		−0.134***	−0.134***
		(−6.94)	(−6.98)		(−5.84)	(−5.77)
10+ GCs prev. year		−0.144***	−0.145***		−0.113***	−0.113***
		(−4.50)	(−4.49)		(−2.99)	(−3.00)
Target's age & listing	Yes	Yes	Yes	Yes	Yes	Yes
Past 5y Deal counts	No	Yes	Yes	No	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
within R-squared	0.006	0.067	0.068	0.001	0.046	0.046
adjusted R-squared	0.043	0.096	0.096	0.026	0.063	0.062
Observations	1,567	1,567	1,567	1,567	1,567	1,567

Panel B: Targets' past renegotiation track record

	Pctle(GC count) 2-3y Chge			Pctle(GC value) 2-3y Chge		
	(1)	(2)	(3)	(4)	(5)	(6)
GC mod.amount growth t_d-1	0.084*			0.068		
	(1.87)			(1.60)		
GC mod.duration growth t_d-1		0.043***			0.036**	
		(3.13)			(2.35)	
I(>3 GCs mod.$&dur ↑ $t_d-3:-1$)			0.081**			0.171***
			(2.21)			(3.43)
Targets's past GC experience	Yes	Yes	Yes	Yes	Yes	Yes
Target's age& listing status	Yes	Yes	Yes	Yes	Yes	Yes
Past 5y Deal counts	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
within R-squared	0.068	0.067	0.064	0.045	0.045	0.048
adjusted R-squared	0.097	0.097	0.094	0.063	0.063	0.066
Observations	1,567	1,567	1,567	1,567	1,567	1,567

Panel A: Contractor counts

Panel B: Contracting dynamics

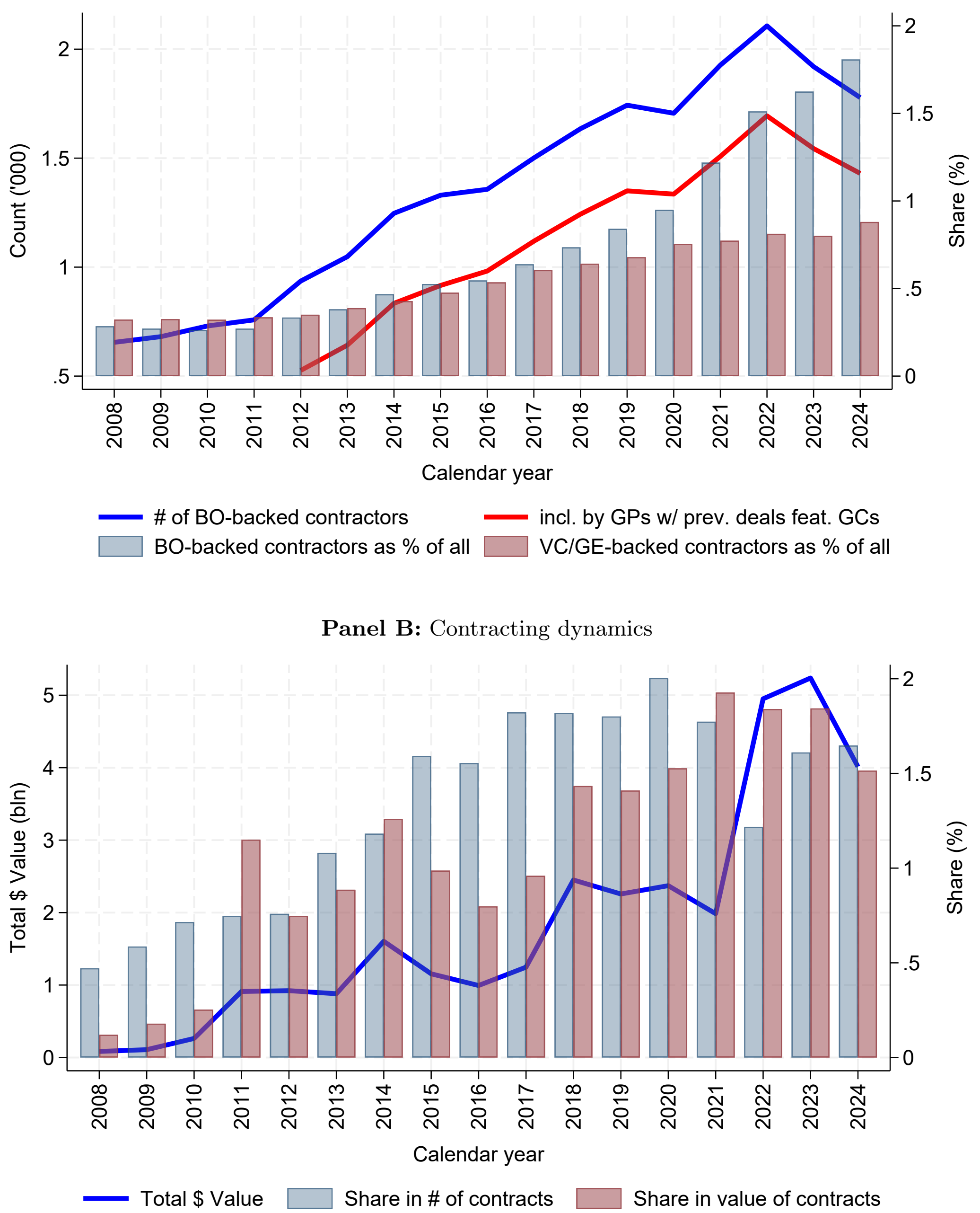
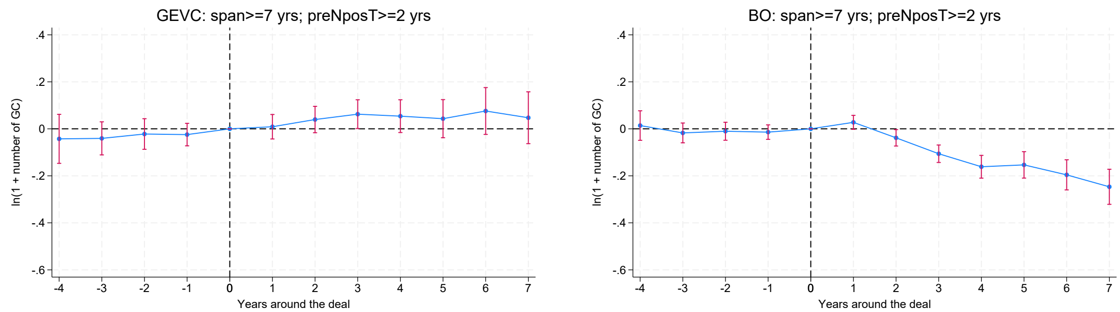


Figure 1. Government contract awards to PE-backed firms. The figure plots the government contracting activity by firms (or parents thereof) that received PE investments in the current year or 3 years before. We consider a firm-year to be a GC-year if the DUNS had at least one contract in excess of $3,000 during our sample period, 2008–2024, and the year is between (or within 2 years from) the first and the last contract awarded to that firm. Panel A reports buyout-backed GC-year counts, as well as their relative share to all GC-year counts in comparison to venture and growth equity-backed, whereas Panel B reports on the actual contracts' aggregate counts and $ values awarded to those buyout-backed targets relative to the US Gov year totals.

Panel A: Number of new contracts (log1plus)



Panel B: Percentile rank (0,1)

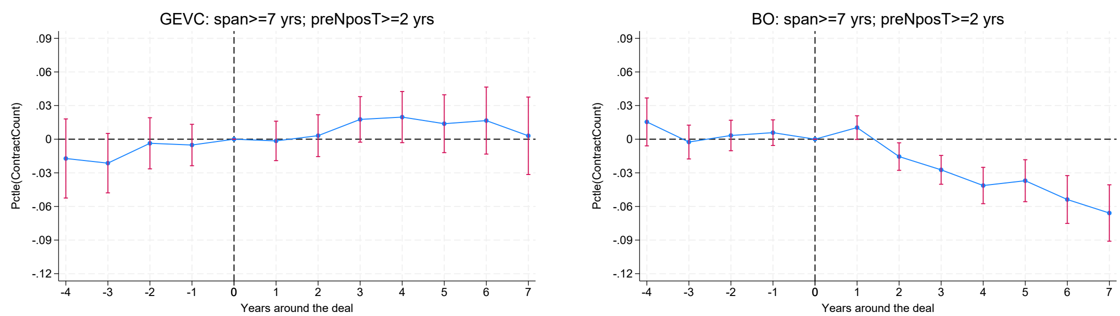
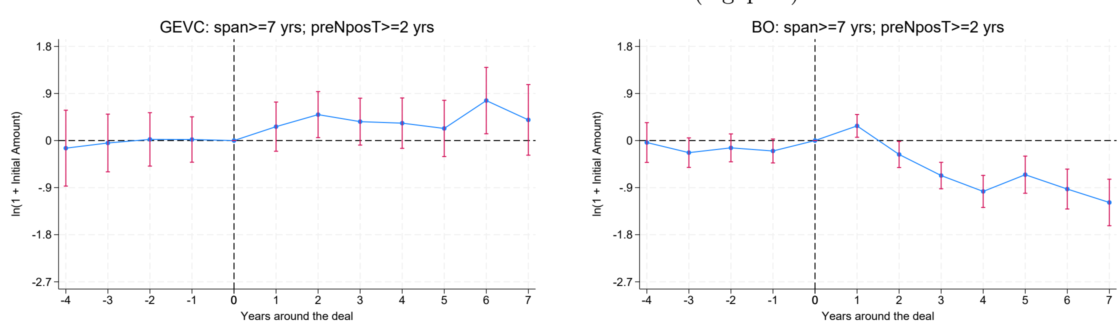


Figure 2. Contract counts around PE deals. This figure plots event studies based on the dCdH estimator. BO denote buyout deals. GEVC denote growth equity deals and venture capital round.

Panel A: Value of new contracts (log1plus)



Panel B: Percentile rank (0,1)

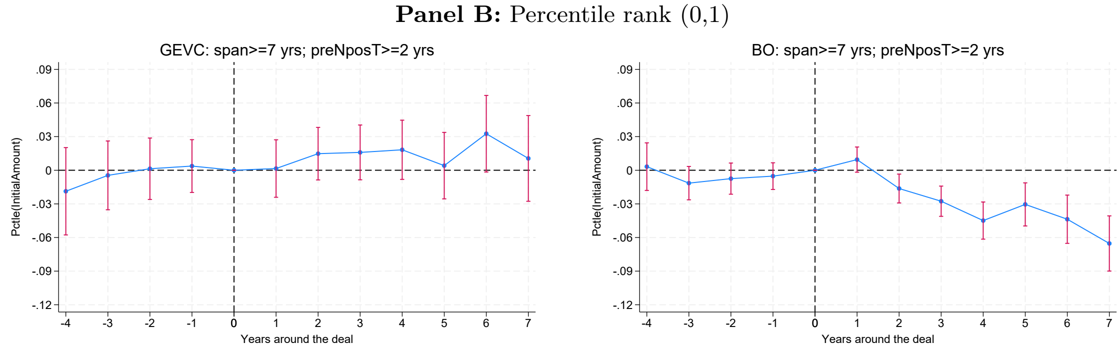
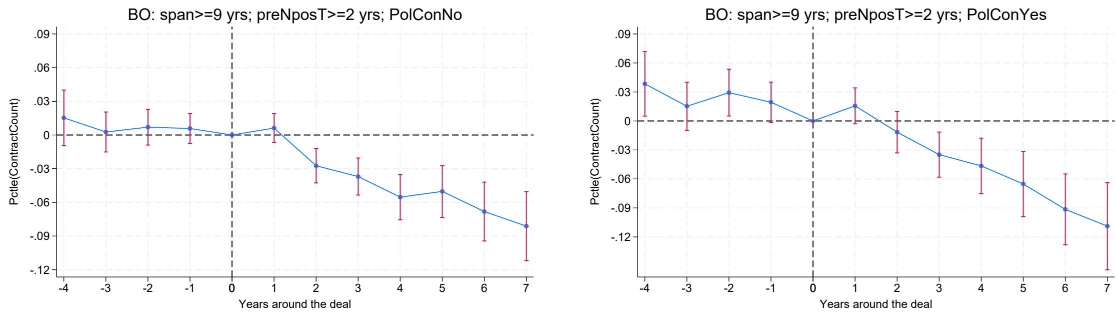
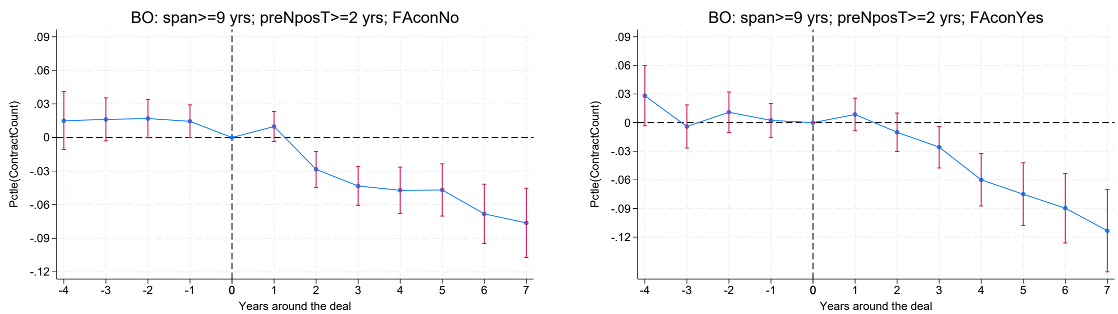


Figure 3. Contract amounts around PE deals. This figure plots event studies based on the dCdH estimator. BO denote buyout deals. GEVC denote growth equity deals and venture capital round.

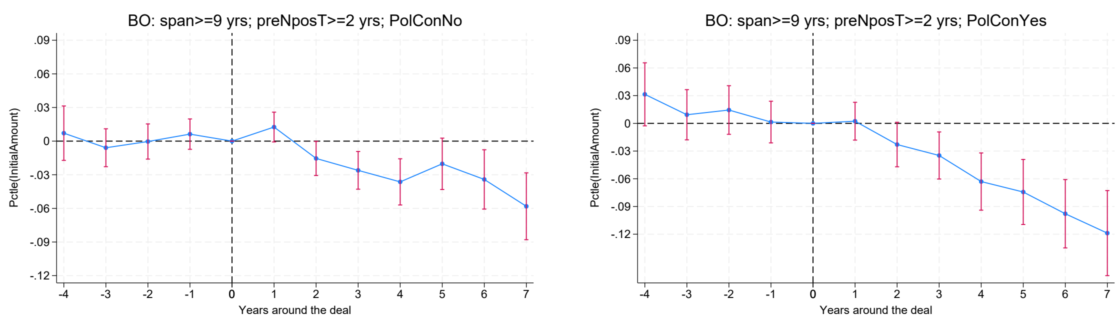
Panel A1: Count ranks by GPs' political connection



Panel A2: Count ranks by GPs' GovAgency connection



Panel B1: Amount ranks by GPs' political connection



Panel B2: Amount ranks by GPs' GovAgency connection

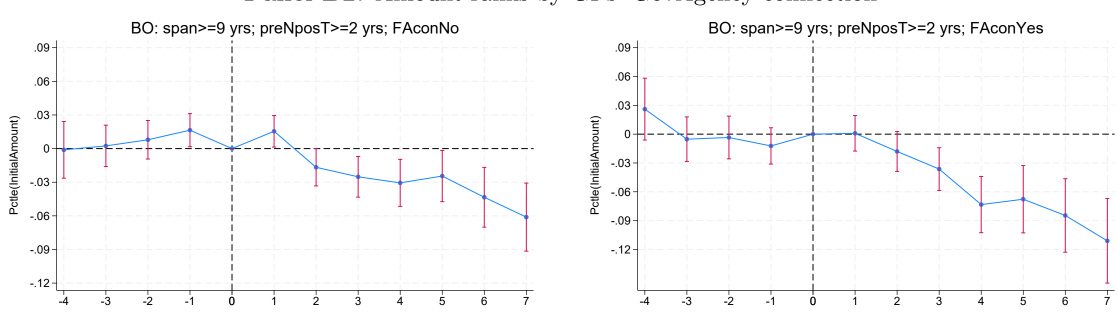
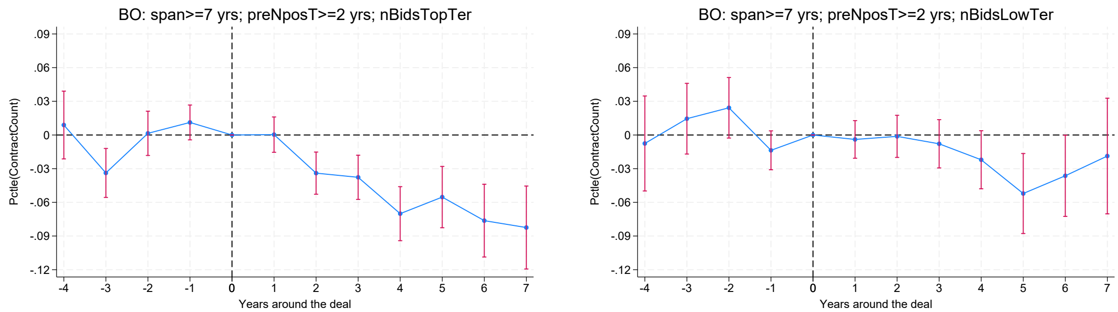
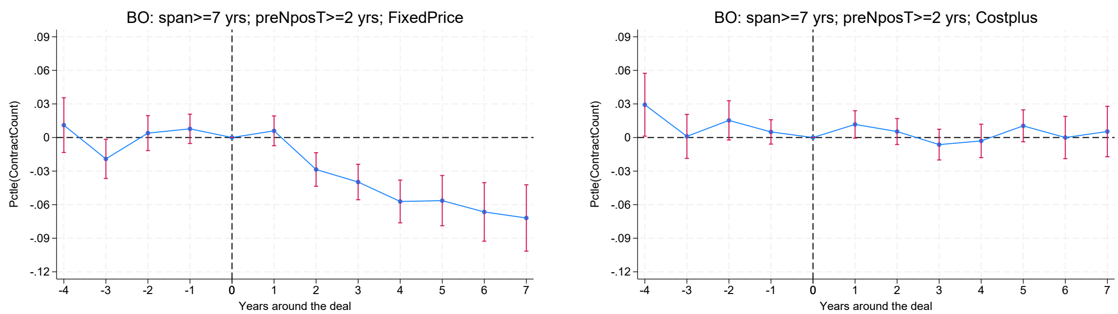


Figure 4. Government connections and contracting dynamics around buyouts. This figure plots event studies based on the dCdH estimator. The splits are based on whether the GP's team based on biographical information reported in Pitchbook has (i) a political connection as defined in Faccio and Hsu (2017) (ii) a connection to Federal Agency as defined in Section 2.

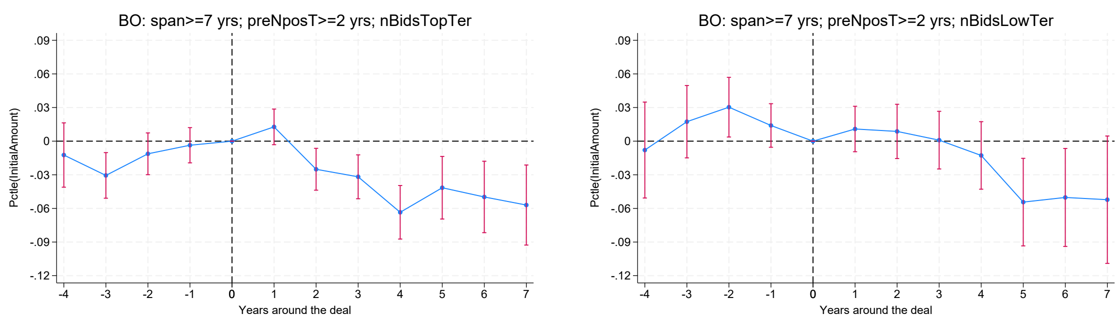
Panel A1: Count ranks by number of bids tercile



Panel A2: Count ranks by pricing formula (0,1)



Panel B1: Amount ranks by number of bids tercile



Panel B2: Amount ranks by pricing formula

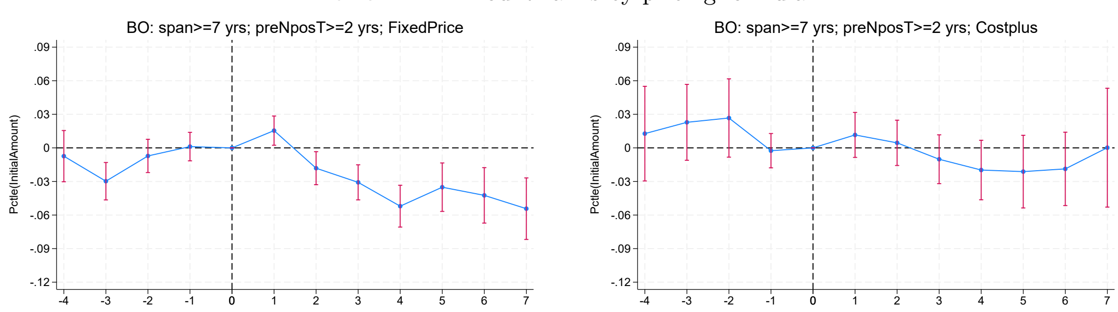
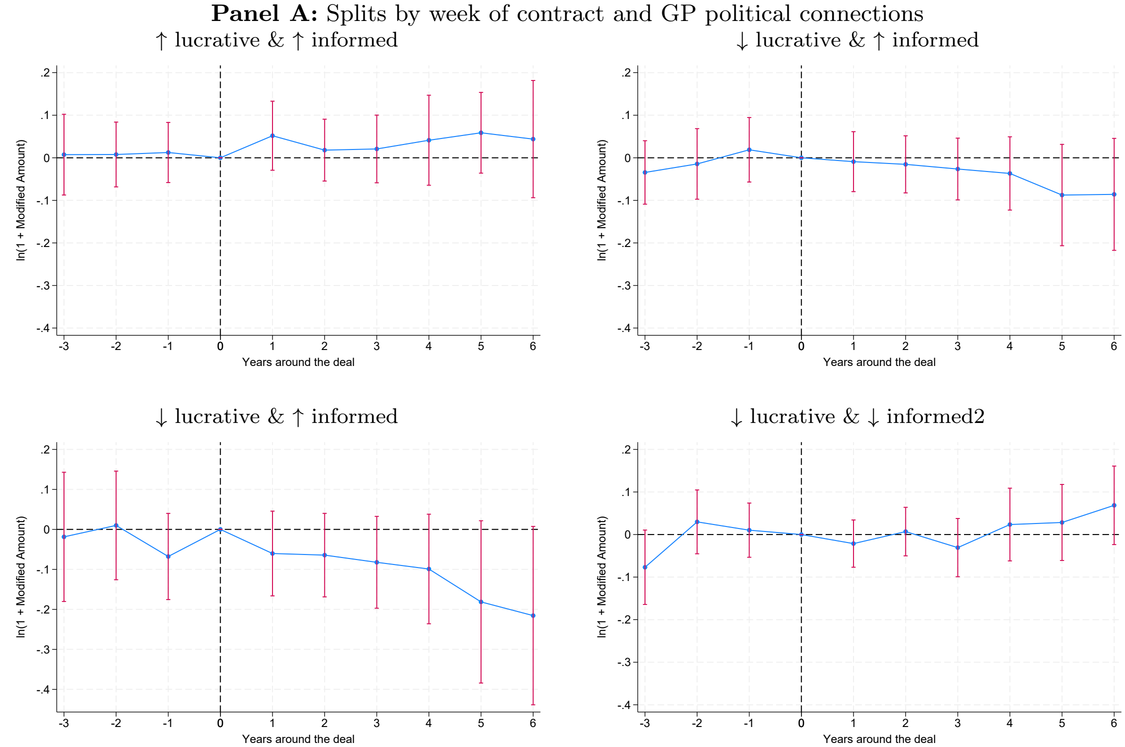


Figure 5. Pricing competitiveness and contracting dynamics around buyouts. This figure plots event studies based on the dCdH estimator. The splits are based on (i) the number of bids (relative to the product code category) submitted for the granted contract (ii) the pricing formula for the contract—Fixed price or Cost plus profit margin.

Panel A: Splits by week of contract and GP political connections



Panel B: Splits by week of contract and GP federal agency connections

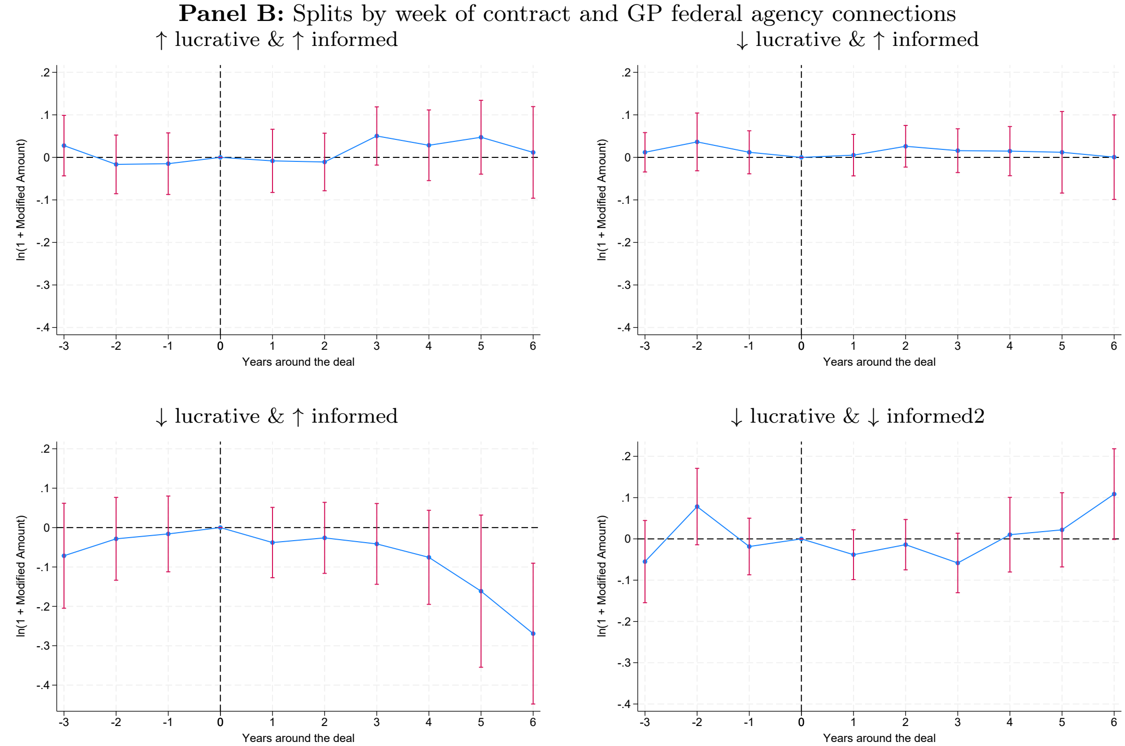


Figure 6. Government connections and contract modifications around buyouts. This figure plots event studies based on the dCdH estimator. The splits are based on whether the GP's team based on biographical information reported in Pitchbook has (i) a political connection as defined in Faccio and Hsu (2017) (ii) a connection to Federal Agency as defined in Section 2. **'↑lucrative & ↑informed'** denotes contracts modified during the week of 9/24–30 by connected GP <2 bids, '**↓lucrative & ↑informed'**—connected GP, 10/25–31 week; **'↓lucrative & ↑informed'**—connected GP, 9/24–30 week, >4 bids; **'↓lucrative & ↓informed2'**—-not connected GP, 9/24–30 week, >4 bids.

Appendix A1. Variable and datasets construction details

Deal sample. We include transactions with the following deal types: "Buyout/LBO", "Investor Buyout by Management", "PE Growth/Expansion", "Seed Round", "Early Stage VC", "Later Stage VC", "IPO", "Exit Financing", "PIPE", "Secondary Transaction - Open Market", "Merger/Acquisition", "Reverse Merger", "Mezzanine", and "Secondary Transaction - Private". We further restrict the sample to deals classified under the "Private Equity" or "Venture Capital" asset classes (DealClass).

GP classification. We set *I(Polit-connected GP)* to 1 if team member biographies mention legislative bodies (Congress, Congressional, House of Representatives, Senate), executive branch positions (White House, specific presidential administrations), political parties (Democratic Party, Republican Party, GOP), federal departments (Agriculture, Defense, Education, Energy, Health and Human Services, Homeland Security, Housing and Urban Development, Interior, Labor, State, Treasury, Veterans Affairs, Commerce), or other key government entities (Council of Economic Advisers, Environmental Protection Agency, National Economic Council, Office of Management and Budget, Small Business Administration); *I(Congr-connected GP)* to 1 if biographies reference only legislative branch experience, enabling us to distinguish lawmakers from executive branch officials. Additionally, we construct a federal agency connection indicator *I(FedAg-connected GP)* that captures prior employment at the agencies and sub-agencies that award procurement contracts in our contracts sample.

Federal contract instrument types. Federal procurement distinguishes between five primary contract instrument types. Purchase Orders (PO) represent standalone contracts for off-the-shelf goods or simple services, typically with straightforward terms and fixed prices. Delivery Orders (DO) are specific orders placed under Indefinite Delivery Vehicles (IDVs), which serve as umbrella agreements allowing agencies to order supplies or services as needed without negotiating separate contracts for each purchase. Definitive Contracts (DC) are standalone agreements with complete terms established at contract award, including specific scope, price, and performance period. Blanket Purchase Agreements (BPA) establish charge accounts with qualified contractors for repeated purchases of supplies or services, with individual BPA Calls representing specific purchases under these agreements. Finally, Task Orders represent specific work assignments issued under IDVs, particularly under Indefinite Delivery/Indefinite Quantity (IDIQ) contracts. Our analysis includes all five instrument types, as economic activity occurs at the individual order or contract level regardless of whether it operates under an umbrella agreement. We retain instrument type indicators to enable robustness checks distinguishing between standalone contracts and orders issued under broader framework agreements.

Contract type classifications. We classify contracts into pricing categories based on the contract pricing type field in the GC data. Cost-plus contracts include pricing codes R ("Cost Plus Award Fee"), U ("Cost Plus Fixed Fee"), V ("Cost Plus Incentive Fee"), and Y ("Time and Materials"). All other pricing codes are classified as fixed-price contracts. We identify competitive contracts using the extent of competition field, including codes A ("Full and Open Competition"), CDO ("Competitive Delivery Order"), and F ("Competed Under SAT"). Contracts with green or sustainability clauses are identified through the recoverable materials sustainability code field, including codes A (bio-based), B (energy efficient), D (environmentally preferable products), E (recoverable material FAR 52.223-4), and F (recoverable material FAR 52.223-9).

Contract initiation and modification tracking. We identify contract initiations as the first action for each unique contract identifier (PIID) where the modification number equals zero. For each contract, we track the sequence of modifications by sorting on action date and creating a sequential counter within each contract. We extract initiation characteristics (initial amount, start date, end date) from the first observation in the sequence and first modification characteristics (modification amount, modified start date, modified end date) from the second observation. The action type field for modifications indicates the nature of the change, enabling us to distinguish between funding adjustments, scope changes, and administrative corrections.

Deduplication strategy. Raw GC data contain duplicate records due to data entry errors and system updates. We remove exact duplicates across all fields. For near-duplicates (same DUNS, PIID, action date, year, and modification number), we implement a stable sorting rule based on contract characteristics: we sort by federal action obligation amount in descending order, then by an initial reference number, keeping the observation with the largest absolute obligation amount. After sorting, we retain one observation per unique combination of DUNS, PIID, action date, year, modification number, and amount. We verify that the resulting dataset contains no remaining duplicates on these key dimensions.

Sample restrictions and filters. Beyond the $3,000 minimum threshold, we apply several additional filters. We drop contracts missing DUNS numbers or contract identifiers, as these cannot be linked to firms or tracked over time. We restrict the sample to contracts awarded to U.S. entities using the recipient country code field, keeping observations coded as "USA" or with missing country codes (since older data lack this field). We exclude contracts where we cannot observe the initiation within our sample period by requiring each contract to have exactly one observed initiation record. This ensures each contract in our analysis has a well-defined start date and initial characteristics.

Government ownership and small business exclusions. We exclude contractors based on ownership structure and set-aside program participation. For each DUNS number, we calculate the time-series average of government ownership indicators and small business designation flags across all observed years. We then drop contractors where the average government ownership indicator exceeds 0.1 (indicating government-owned entities or government-to-government contracts in more than 10% of contractor-years) or where the average small business set-aside indicator exceeds 0.2 (indicating participation in small business programs in more than 20% of contractor-years). These exclusions focus our analysis on commercial contractors where private equity acquisitions represent meaningful governance changes rather than contracts reserved for specific government programs or minority-owned business set-asides.

Parent-subsidiary matching. The GC data record both the immediate contractor DUNS and the parent organization DUNS. We create two linkages to Pitchbook: direct matches where the contractor DUNS appears in the DUNS-to-companyid map, and indirect matches where the parent DUNS links to Pitchbook. When both direct and indirect matches exist, we prioritize the direct match. For contractors with only parent matches, we assign the parent's Pitchbook identifier and aggregate all subsidiary contracting activity to the parent level. This ensures we capture PE acquisitions of parent companies that continue operating through subsidiary contractors.

Time-series structure and panel balancing. For each DUNS number, we identify the first year with any contract activity and create observations for all years from two years before the first contract through two years after the last observed contract. This padding ensures we capture pre-trends and post-exit dynamics in the difference-in-differences analysis. We construct a balanced panel structure, then fill forward time-invariant characteristics (NAICS codes, credit ratings) within each contractor using the most recent non-missing value. Time-varying contract measures (counts, amounts, modifications) receive zero values for years without activity rather than missing values.

Appendix A2. Additional results

Panel A: Outcome = N(0,1) random variable

Panel B: Outcome = Uniform(0,3) random variable

Panel C: Outcome = log(1 + Binomial random variable)

Panel D: Outcome = log(1 + Actual contracts count)

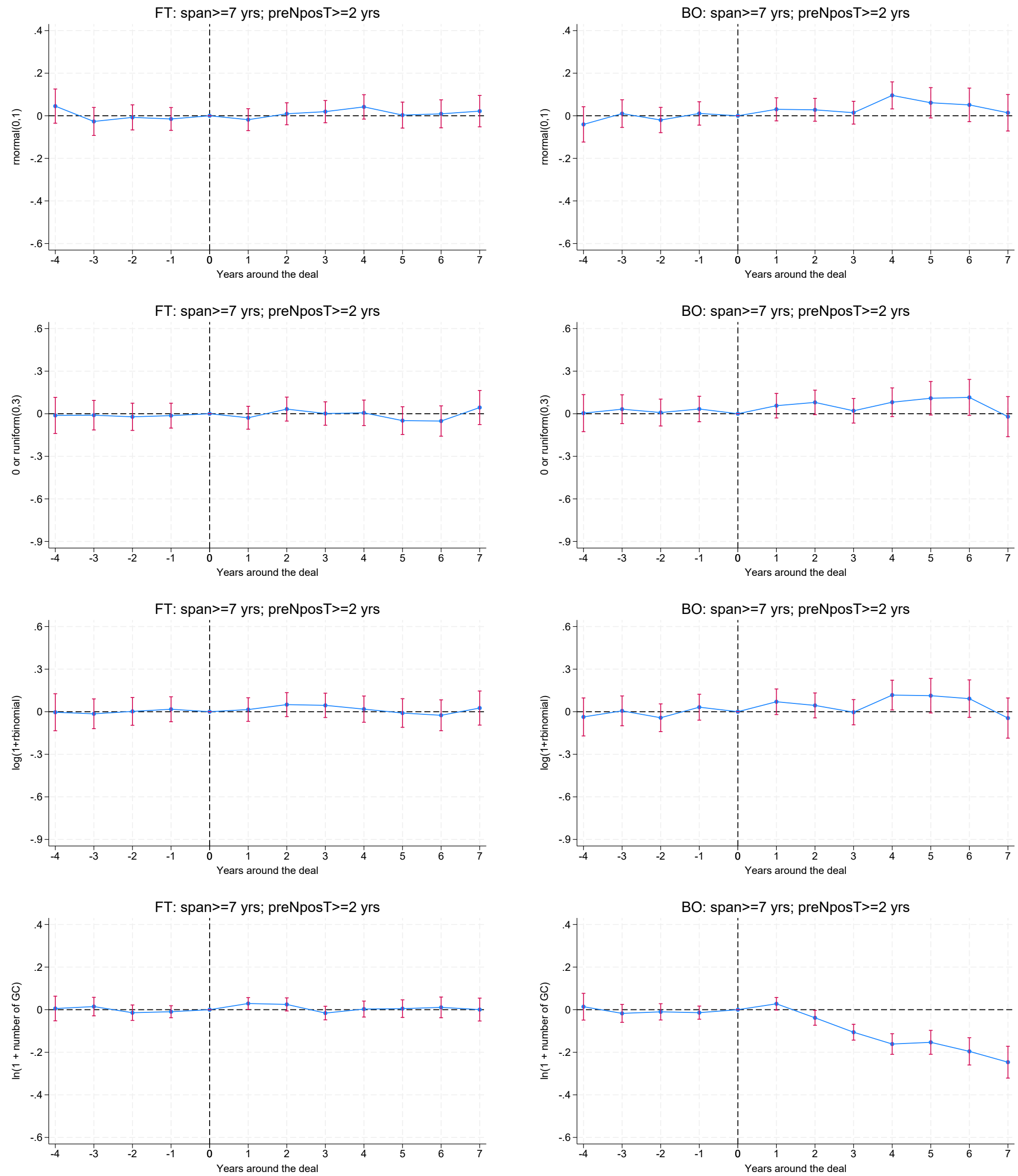


Figure A1. Placebo versus actual log-counts. This figure plots event studies based on the dCdH estimator as in figure 2 but actual contract counts replaced with randomly drawn values. FT = (False Treatment) randomly assigned treatment dummy to match the frequency of buyouts (BO). Besides informing of spurious correlations emanating from the timing of buyout deal and the contractor sample span, this analysis address the concern raised in Cohn, Liu, and Wardlaw (2022) that a use of log of 1 plus count as dependent variable may generate estimation biases.

Panel A: Number of new contracts

Panel B: New contracts count percentile rank

Panel C: Value of new contracts

Panel D: New contracts value percentile rank

Panel E: I(Contract amount ↑ upon modification)

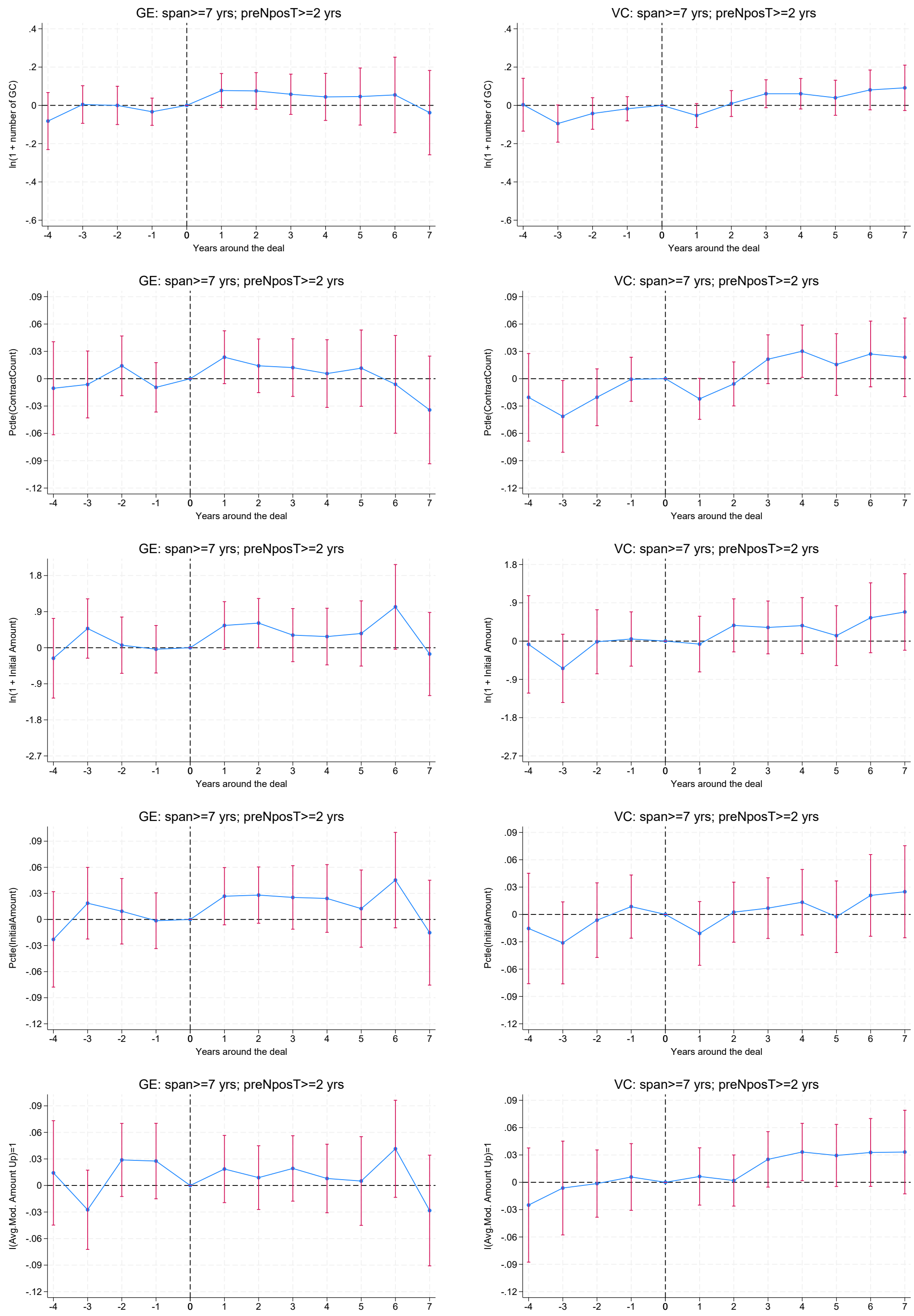
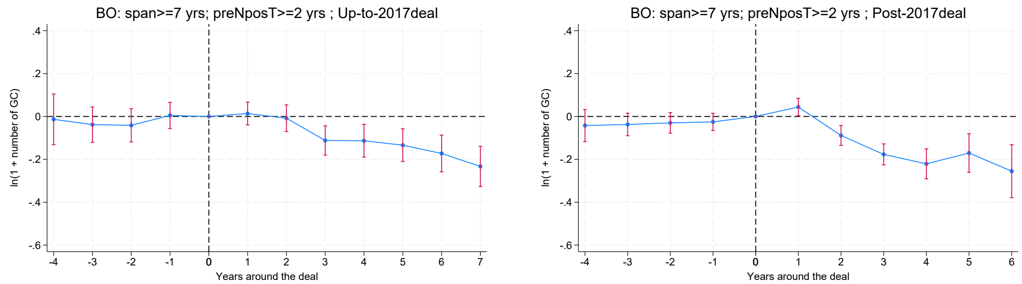
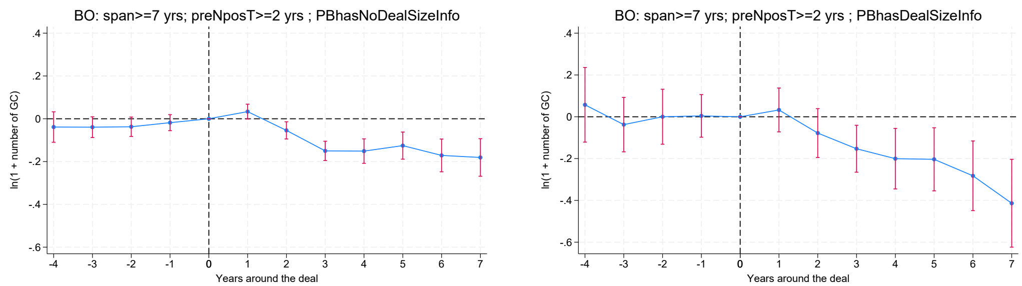


Figure A2. Do government contracting dynamics differ between growth and venture capital deals? This figure plots event studies based on the dCdH estimator as in figures 2–A4 but for grow equity deals (GE) and venture capital rounds (VC) separately.

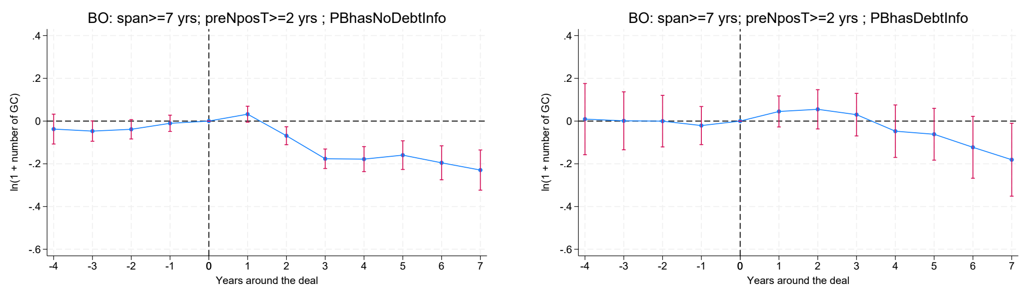
Panel A: Before and after 2017



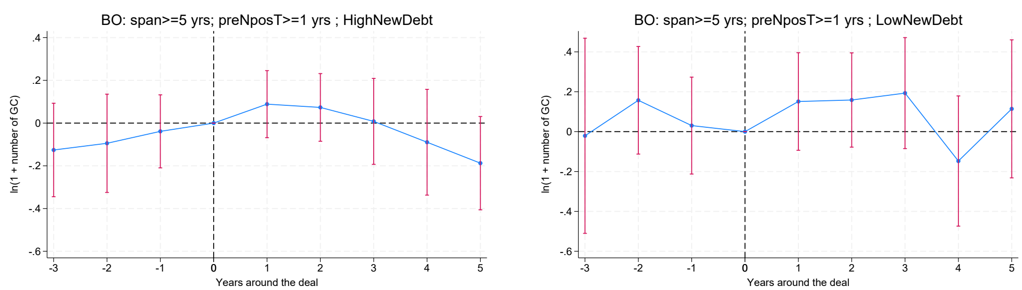
Panel B: PB has the deal's size?



Panel C: PB has the target's new debt from the deal?



Panel D: Split by the New Debt Size if reported to PB



Panel E: PB has the target's EBITDA or Revenue post-deal?

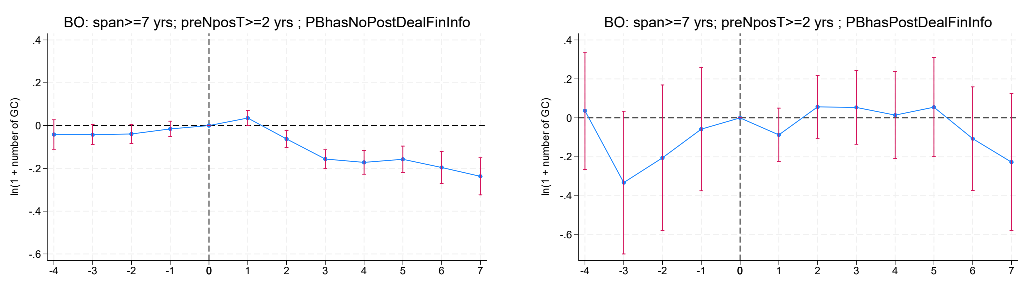
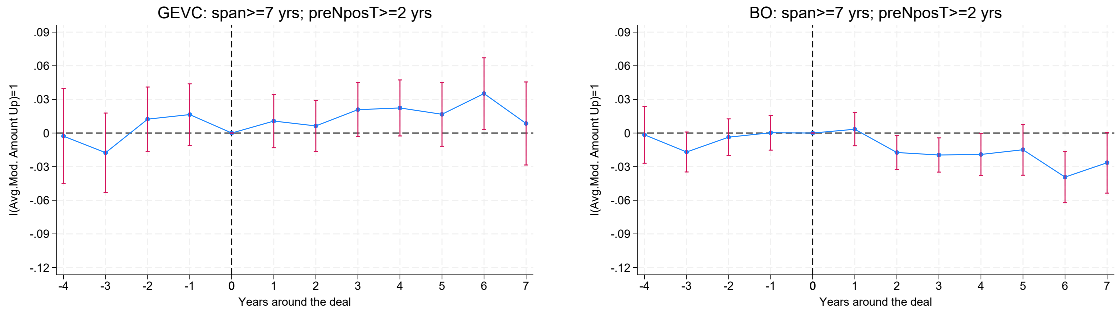
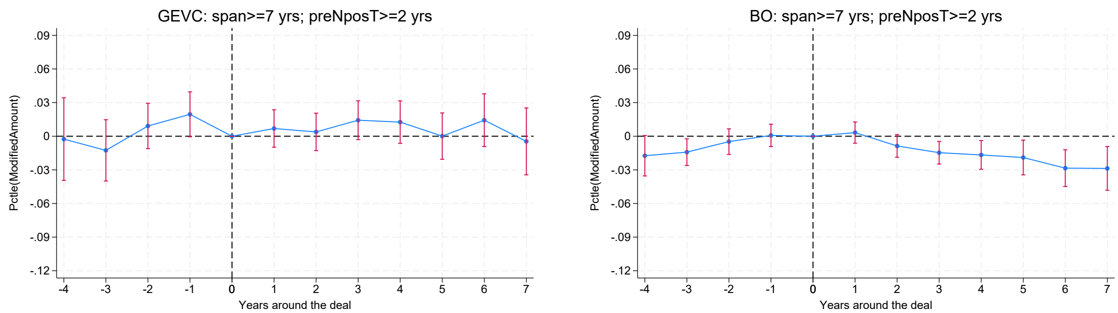


Figure A3. Buyout effects by deal subgroup This figure plots buyout effects on initial contract counts (log of 1+ number) by selected subsamples of deals.

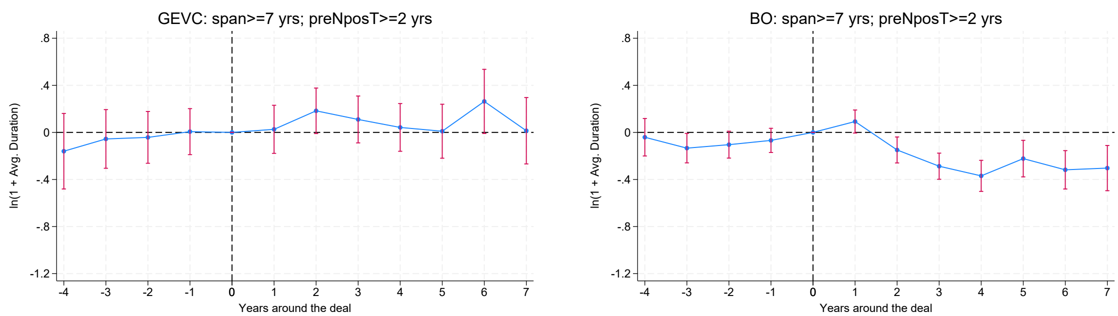
Panel A: I(Contract amount increase upon modification)



Panel B: Modified contract amount percentile rank (0,1)



Panel C: Original contract durations log(1+days)



Panel D: I(duration increase upon modifications)

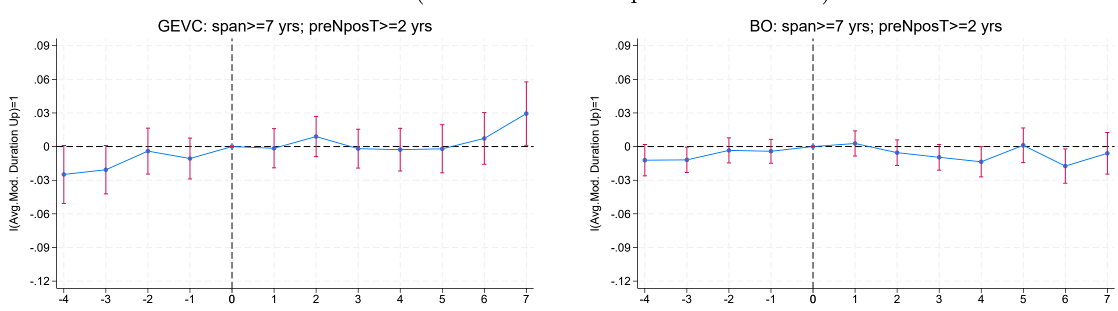


Figure A4. Contract modification around PE deals. This figure plots event studies based on the dCdH estimator. BO denote buyout deals. GEVC denote growth equity deals and venture capital round.

Appendix A3. Variables glossary

Variable	Definition
	Company Characteristics
Investee Co age	Age of the target company at the time of the deal, measured in years.
Disclosed deal size	Transaction value in USD millions as reported in Pitchbook, when available.
GCs last 3 years	Government contracts awarded to the firm in the three years preceding the deal.
# of new contracts	Number of new government contracts awarded to the contractor in a given year.
Yr.Tot init. amt (= 'Initial amount')	Total initial obligated amount (in USD millions) of new government contracts awarded to the contractor in a given year.
Yr.Tot modif. amt (= 'Modified amount')	Total net modification amount (in USD millions) across all contract modifications for the contractor in a given year.
Av. init. duration	Average initial duration (in months) of new contracts awarded to the contractor in a given year.
Av. growth if mod. (= 'GC mod.amount growth')	Average proportional change (in decimal form) in contract dollar amount upon modification, conditional on the contract being modified.
Av. dur. chge if mod. (= 'GC mod.duration growth')	Average change in contract duration (in months) upon modification, conditional on the contract being modified.
Yrs since 1st contr.	Number of years since the contractor received its first government contract in the sample (post-2000).
2-to-5y of GCs	Indicator for contractors with 2–5 years of government contracting history.
5-to-9y of GCs	Indicator for contractors with 5–9 years of government contracting history.
10y+ of GCs	Indicator for contractors with 10 or more years of government contracting history.
1-to-2 GCs prev. year	Indicator for contractors with 1–2 government contracts in the year preceding the deal.
3-to-10 GCs prev. year	Indicator for contractors with 3–10 government contracts in the year preceding the deal.
10+ GCs prev. year	Indicator for contractors with more than 10 government contracts in the year preceding the deal.
I(>3 GCs mod.$&dur $\uparrow t_{d-3:d-1}$)	Indicator equal to one if the target had more than three contracts with both dollar amount and duration increases upon modification in the three years preceding the deal.
PB has Deal Size Info	Indicator equal to one if Pitchbook reports the deal's transaction size.
PB has Debt Info	Indicator equal to one if Pitchbook reports the target's new debt from the deal.
High New Debt / Low New Debt	Indicator for deals above/below median new debt amount, conditional on debt information being reported.
PB has post-Deal Fin Info	Indicator equal to one if Pitchbook reports the target's EBITDA or revenue post-deal.
	GP Characteristics
All deals completed this year	Number of deals completed by the GP in a given calendar year.
Deals feat. GCs this year	Number of deals completed by the GP in a given year where the target had government contracts.
Funds raised over last 5y	Number of funds raised by the GP over the preceding five years.
Ave. fund size	Average fund size in USD billions for funds raised by the GP over the preceding five years.

Continued on next page

Table continued from previous page

Variable	**Definition**
I(GP is listed)	Indicator equal to one if the buyout GP (or its parent) is publicly traded.
I(Polit-connected GP)	Indicator equal to one if the GP's team includes individuals with political connections, defined following Faccio and Hsu (2017).
I(FedAg-connected GP)	Indicator equal to one if the GP's team includes individuals with prior employment at a federal government agency.
I(Congr-connected GP)	Indicator equal to one if the GP's team includes individuals with prior Congressional employment or connections.
I(GC-deals 1-to-5y before)	Indicator equal to one if the GP completed at least one deal involving a government contractor in the one to five years preceding the current deal.
I(past deals av. GC-value↑)	Indicator equal to one if the GP's past deals involving government contractors experienced average increases in government contract values post-acquisition.
I(GP pref top-GC ind)	Indicator equal to one if the GP has declared investment preferences in industries or verticals that rank in the top quintile by government contracting volumes.
	Contract Terms and Settings
Fixed Price	Contracts where the contractor agrees to deliver goods or services at a predetermined fixed price, bearing the risk of cost overruns.
Cost Plus	Contracts where the contractor is reimbursed for allowable costs plus an additional fee or profit margin; typically used for complex projects with uncertain scope.
Number of bids	Number of bids submitted for a given contract, measured relative to the product code category. Used to proxy for contract competition intensity.
nBidsTopTer	Contracts in the top tercile of bid counts (highest competition) within their product code category.
nBidsLowTer	Contracts in the bottom tercile of bid counts (lowest competition) within their product code category.
Sep Lw (last week)	Contracts modified during September 24–30, the last week of the federal fiscal year when "use-it-or-lose-it" spending peaks.
Sep L2w (last 2 weeks)	Contracts modified during September 17–30, the last two weeks of the federal fiscal year.
Sep	Contracts modified during the entire month of September.
Oct L2w	Contracts modified during October 25–31; serves as a placebo period outside the fiscal year-end spending surge.
<2 bids	Contracts receiving fewer than two bids, indicating low competition and potential sole-source or limited competition awards.
>4 bids	Contracts receiving more than four bids, indicating high competition.
Fixed-price contractor	Indicator equal to one if the majority (>50%) of the contractor's government contracts in the 3-year period pre-buyout were classified as "Firm Fixed Price" (`type_of_contract_pricing_code` "J")