Breaking Down the U.S. Employment Multiplier Using Micro-Level Data

Edoardo Briganti*
Bank of Canada

Holt Dwyer†
UC San Diego

Ricardo Duque Gabriel‡
Federal Reserve Board

Victor Sellemi§
UC San Diego

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Abstract

Using newly matched U.S. defense contract and restricted administrative employment data, we show that the employment effects of defense procurement are costly, concentrated, and slow to diffuse. Employment gains are initially driven by large existing contractors and come at a high cost of approximately $290,000 per job-year. While employment in non-contracting firms is crowded out on impact, positive spillovers emerge gradually and account for half of regional employment gains by the third year after a spending shock, suggesting delayed but persistent medium-term gains across industries. Within contractors, only 15% of job creation occurs at recipient establishments, highlighting the role of supply chain linkages.

*Email: ebriganti@bankofcanada.ca Webpage: edoardobriganti.com
†Email: bdwyer@ucsd.edu Webpage: holtdwyer.com
‡Email: ricardo.f.duquegabriel@frb.gov Webpage: ricardogabriel.com
§Email: vsellemi@ucsd.edu Webpage: victorsellemi.com

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I. Introduction

Government procurement is one of the largest components of public expenditure and a central instrument of industrial policy in advanced economies. By directing demand to private firms, procurement shapes production networks, supports employment, and influences the geographic distribution of economic activity. Its role is especially prominent in defense, where spending sustains industrial capacity and advances national security objectives. In recent years, rising geopolitical tensions and rearmament efforts have renewed interest in procurement not just for defense purposes, but also as a tool for long-term economic development. Yet, despite its scale and policy importance, there remains considerable debate over the effectiveness of defense procurement in generating local employment and the channels through which job gains are realized.

In the U.S., defense procurement channels hundreds of billions of dollars annually to private firms, shaping supply chains and regional labor markets, and supporting national production capacity. While previous studies have estimated regional and aggregate employment effects of defense procurement, less is known about where these job gains originate. In particular, little evidence exists on how effects unfold across contractors and non-contractors, whether employment gains originate primarily from direct recipients or from other defense contractors, to what extent small and medium-sized firms benefit, and whether job creation arises through new firm entry or the expansion of already existing contractors. Disentangling these margins is essential for understanding both the efficacy and distributional impact of procurement as a labor market intervention.

We show that the employment effects of defense procurement are costly, concentrated, and slow to diffuse, yet they can support regional economic development over time. Employment gains are initially driven by large existing contractors and come at a high cost of approximately $290,000 per job-year, reflecting the high wages present in skill-intensive industries in the defense sector. On impact, employment in non-contracting firms is crowded out, but positive spillovers emerge gradually and account for over half of the regional effect by year three, indicating meaningful but delayed spillovers to non-contractor firms. Finally, within contractors, only about 15% of job creation occurs at establishments directly receiving contract awards, highlighting the importance of

production linkages in diffusing procurement-driven employment growth.

To draw these conclusions, we use publicly available data on the universe of federal contracts from USASpending.gov, which draws from the Federal Procurement Data System (FPDS).[1] We aggregate contracts by year and region to construct a measure of regional defense procurement spending, which serves as our main explanatory variable. To examine its effects on the labor market, we focus on employment as the primary outcome, drawing on several data sources that provide employment measures at the regional level. First, we measure total employment using data from the Bureau of Economic Analysis (BEA), which in turn relies on the Quarterly Census of Employment and Wages (QCEW). To analyze heterogeneity by firm size, we use the Business Dynamics Statistics (BDS) from the U.S. Census Bureau, which report employment and firm counts by region and firm size. Unemployment and labor force statistics are obtained from the Local Area Unemployment Statistics (LAUS) provided by the Bureau of Labor Statistics (BLS). We harmonize these three datasets into a single baseline sample covering the period 2001–2019 and spanning 358 Metropolitan Statistical Areas (MSAs). The MSA is a suitable unit of analysis, as it balances the need for a large sample size—there are roughly 380 MSAs in total—while limiting cross-regional spillovers, since MSAs aggregate adjacent counties into metropolitan areas that behave as small open economies.

In addition, this project was granted access to restricted-use microdata from the QCEW for 42 signatory states and the District of Columbia.[2] These data include monthly employment and quarterly wage information for the universe of employer establishments, and is referred to as the Longitudinal Database of Establishments (LDBE). We use state-of-the-art string-matching algorithm to link the universe of defense contractors from FPDS to the universe of establishments from QCEW and aggregate the resulting data to the year–MSA level. This procedure allows us to decompose regional employment into contractor and non-contractor components and to study the employment effects of defense procurement over the 2006–2019 period across 254 MSAs. The restricted-use QCEW data also enable us to analyze the direct effects of procurement on contract recipients at

[1] For a detailed discussion of this dataset see (Cox et al., 2024).

[2] We received access through the Restricted Data Access (RDA) service of the BLS. As of June 2025, RDA is suspended for all projects due to resource limitations of the BLS.

the establishment level.

We begin by estimating the average regional employment response to defense procurement shocks using the same instrumental variable framework of Auerbach, Gorodnichenko, and Murphy (2020). The approach isolates exogenous variation in regional procurement by interacting national changes in defense spending with historical regional exposures to federal contract awards (Demyanyk, Loutskina, and Murphy, 2019; Auerbach, Gorodnichenko, and Murphy, 2024). This exercise provides a baseline against which we evaluate heterogeneity in the employment response. We find that procurement-induced demand generates modest but sustained employment gains: over a three-year horizon, the employment multiplier is approximately 0.1, meaning that a procurement shock equal to 1% of regional wages and salaries increases regional employment by 0.1%. While economically meaningful, these gains are costly, averaging approximately $290,000 per job-year, in 2008 dollars. This high cost potentially reflects the high-wage structure of defense-intensive sectors (Bartal and Becard, 2024) and underscores the limits of defense procurement as a tool for short-run job creation. We this baseline in mind, we break down the U.S. regional employment multiplier along several key margins — including firm size, contractor status, and establishment-level exposure — to uncover the mechanisms through which procurement-driven demand translates into job creation.

First, we break down the regional employment response to procurement shocks by firm size using BDS data. This exercise quantifies the share of job gains attributable to small, medium, and large firms. We find that employment gains are disproportionately driven by large contractors, with most of the growth occurring along the extensive margin of existing firms rather than through new entry or establishment formation. To complement this outcome-based decomposition, we turn to contract-level data linked to establishment employment records, which we aggregate to the firm level. This linkage enables us to trace the effects of regional shocks on government contracts through the size distribution of recipient firms. We find that small and medium-sized enterprises receive a minority of contract dollars and account for a proportionately small share of the resulting employment gains. Although firm-level studies often find that small businesses respond more strongly to a given procurement shock, our results highlight that at the macroeconomic level these

firms play only a limited role in aggregate employment multipliers. Taken together, these exercises reveal that procurement-driven employment growth is concentrated among large, incumbent firms, reinforcing rather than reshaping the industrial structure of local economies.

Second, we break down the regional employment response by contractor status using newly matched U.S. defense contract and restricted administrative employment data from the Bureau of Labor Statistics. These matched data allow us to distinguish between firms with a history of receiving defense contracts ("contractors") and those without ("non-contractors"). This disaggregation reveals important dynamics after a fiscal expansion: employment in non-contracting firms is crowded out on impact, suggesting resource reallocation or displacement effects in the short run. However, positive spillovers emerge gradually, and by the second year after a shock, non-contractors account for nearly half of the regional employment gains. By year three, their contribution exceeds that of direct contractors. These patterns indicate that procurement-induced demand propagates beyond the set of direct recipients, but only with a lag. Taken together, the results suggest that while procurement is not well-suited to deliver immediate stimulus, it can support medium-term employment growth across a broad set of industries, including those not directly linked to defense.

Finally, we break down the contractor employment response by isolating the effects on establishments that directly receive contract awards. Using the same matched micro-level data, we track the employment trajectories of contract-winning establishments and find that only about 15% of job creation occurs at these direct recipients. The remaining 85% of gains are concentrated among other defense contractors within the region. These findings imply that the observed employment effects are not confined to direct recipients but instead diffuse through broader supply chains and organizational networks. Moreover, the employment gains at recipient establishments are persistent, extending well beyond the median contract duration. This underscores the importance of accounting for indirect channels, such as subcontracting relationships and industry input-output linkages, when evaluating the labor market effects of procurement policy.

Related Literature and Contribution We contribute to the literature on the sub-national effects of government purchases on employment in the U.S. at the industry level (Perotti, 2007; Nekarda and Ramey, 2011; Komarek, Butts, and Wagner, 2022; Barattieri, Cacciatore, and Traum, 2023), at the state level (Chodorow-Reich et al., 2012; Nakamura and Steinsson, 2014; Dupor and Guerrero, 2017), and at the regional or sub-state level using the Federal Procurement Data System (Demyanyk, Loutskina, and Murphy, 2019; Auerbach, Gorodnichenko, and Murphy, 2020; Muratori, Juarros, and Valderrama, 2023; Auerbach, Gorodnichenko, and Murphy, 2024). Overall, these studies find positive effects of government spending on employment and/or hours worked, and negative effects on unemployment.[3] The debate on employment multipliers has centered largely on the magnitude of positive effects. For example, Choi, Penciakova, and Saffie (2023) argue that political connections reduce the number of job-years created by increased government spending, while Demyanyk, Loutskina, and Murphy (2019) bring forward evidence that higher regional consumer indebtedness increases the size of the multiplier.

We make two key contributions to this literature. First, we show that defense procurement has become a relatively costly source of job creation compared with other types of spending or similar programs in earlier periods. In particular, we estimate a cost-per-job in the range of $284,000 to $305,000 in 2008 dollars, depending on the horizon of the estimation. By contrast, the review by Chodorow-Reich (2019) reports estimates for ARRA spending in the range of $25,000–$125,000 per job.[4] This difference does not seem to be driven by the nature of the fiscal shock as other work focusing on earlier waves of spending also find lower estimates of the cost-per-job (Nakamura and Steinsson, 2014; Muratori, Juarros, and Valderrama, 2023). In addition, we argue that this difference does not appear to be entirely driven by subcontracting, cross-MSA spillovers, or the lengthy duration of contracts. While a definitive explanation lies beyond the scope of this paper, one plausible explanation lies on the recent trend of defense contractors becoming increasingly concentrated in high-tech sectors which employ high-skilled workers with high-paying jobs (Bartal and Becard, 2024).

[3] To the best of our knowledge, only Hager and Huber (2025) find evidence of negative long-run employment multipliers, using procurement shocks from Germany's Census population recount and arguing that increased procurement generates a "dynamism drain."

[4] Estimates reviewed are in contemporaneous 2008-2010 dollars.

Second, leveraging our unique access to the QCEW microdata (LDBE), we find evidence of mild short-run crowding-out effects on employment among firms not directly affected by defense procurement, which dampens the overall size of the employment multiplier. Third, we contribute by providing a novel breakdown of the employment multiplier by firm size and contractor status, improving our understanding of the transmission mechanism of government purchases in the labor market.

We also relate to the literature studying the effects of procurement contracts on individual firms in Austria (Gugler, Weichselbaumer, and Zulehner, 2020), Brazil (Ferraz, Finan, and Szerman, 2021), Portugal (Gabriel, 2024), South Korea (Lee, 2024), and Spain (di Giovanni et al., 2023), all of which find positive effects of procurement contracts on employment growth and more mixed findings on private sales. For the U.S., the empirical evidence is more limited: Hebous and Zimmermann (2020) and Budrys (2022) analyze the investment response of publicly traded firms to contracts, while Juarros (2022) studies the effects of state-level shocks on financial variables of non-contractors using Orbis data. None of this work focuses on employment growth, primarily due to the lack of high-frequency and high-quality employment data.

We contribute to this strand of the literature in two ways. First, we exploit institutional features of the U.S. federal procurement system to identify a novel set of unanticipated contracts that can serve as demand shocks at the establishment level, allowing us to make causal statements about contract effects. We find that only about 5% of Department of Defense contracts are likely to be fully unanticipated at the firm level. Second, we provide direct evidence of the positive effects of contracts on employment growth among U.S. contract recipients, using establishment-level data from LDBE. We find significant and persistent employment effects on recipients, although their magnitude is modest: only about 15% of the regional employment multiplier on contractors reflects the direct effect on contract recipients. This underscores the importance of input-output linkages and subcontracting networks among firms connected to government procurement.

The remainder of the paper is structured as follows. Section II discusses the institutional background of federal procurement spending in the U.S. Section III describes our regional-level data and identification strategy and presents our baseline estimates of the employment multiplier. Sec-

tion IV examines heterogeneity by firm size, showing that large firms not only account for the bulk of the employment response but are also the main recipients of contracts originating from a defense spending shock. Section V leverages our restricted-access LDBE data to separate the multiplier into contractor and non-contractor responses. Section VI turns to the establishment-level analysis and provides an estimate of the share of the employment multiplier attributable to the direct effects on contract recipients. Finally, Section VII concludes.

II. Institutional Background

Procurement spending refers to the purchase of goods and services by the government from private entities. Figure 1 plots federal procurement spending by fiscal year, as measured in the National Income and Product Accounts (NIPA). The left panel reports procurement as a share of total government spending, while the right panel shows its share of GDP. On average, procurement spending constitutes about 16% of total government spending and roughly 3% of GDP. Given its magnitude and its direct effect on U.S. private firms, federal procurement is an important channel through which the government can stimulate economic activity as more than 90% of U.S. procurement spending corresponds to contracts with a primary place of performance in the U.S.

Beginning in fiscal year 2001, the full universe of federal procurement contracts has been publicly available through USASpending.gov. These data are drawn from the Federal Procurement Data System (FPDS), the platform used by federal contracting officers to record every federal contracting action.[5] Figure 1 shows FPDS data, aggregated by fiscal year, in red. The FPDS series aligns closely with national accounts, offering an exceptionally detailed micro-origin of federal procurement spending.[6]

The richness of FPDS data enables research at highly disaggregated levels, including sub-industries (six-digit NAICS codes), regions (MSAs and counties), firms, and even establishments. In this pa-

[5]This dataset is also employed in Demyanyk, Loutskina, and Murphy (2019), Auerbach, Gorodnichenko, and Murphy (2020), Hebous and Zimmermann (2020), Juarros (2022), Cox et al. (2024), Muratori, Juarros, and Valderrama (2023), Barattieri, Cacciatore, and Traum (2023), Auerbach, Gorodnichenko, and Murphy (2024), and Auerbach, Gorodnichenko, and Murphy (2025).

[6]Differences in timing between the NIPA and FPDS series reflect how spending is recorded (Briganti, Brunet, and Sellemi, 2025). FPDS records obligations at the contract award date—when firms are most likely to begin responding to unexpected contracts—whereas NIPA appears to incorporate several military contracts with delay.

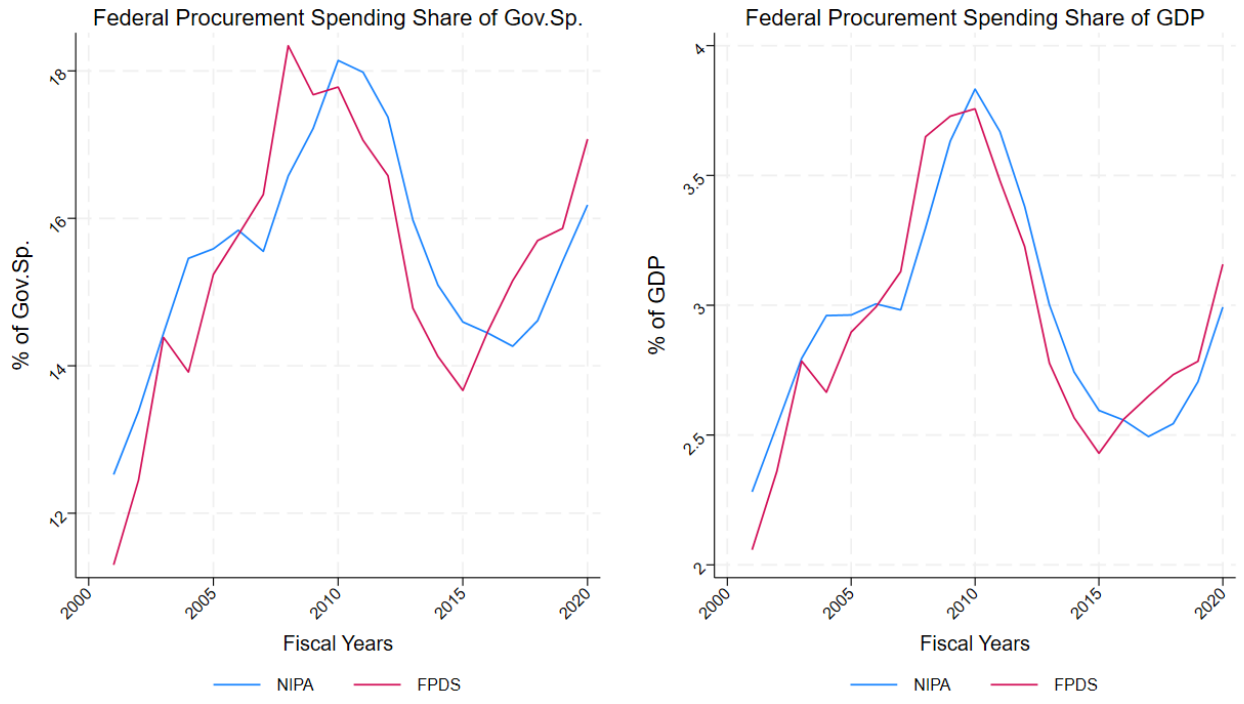


FIGURE 1 — FEDERAL PROCUREMENT SPENDING SHARES

Notes: Federal procurement spending is calculated by adding up (i) NIPA federal government intermediates goods and services purchased and (ii) NIPA federal government gross investment in structure, equipment, and software (see Cox et al. (2024) and Briganti and Sellemi (2023)). Federal spending is the sum of defense and non-defense spending.

per, we exploit this granularity to study the effects of government purchases on employment using a top-down approach, moving from the MSA level to the most granular establishment level (i.e., direct contract recipients).

Breakdown of Federal Contracting As noted in Auerbach, Gorodnichenko, and Murphy (2020), behind each government contract lies a long history of transactions, with significant heterogeneity in the types of contracts awarded. We present here a novel breakdown of federal contracting to highlight its complex and highly heterogeneous composition. Figure 2 reports the distribution of contracts across the most common categories.[7]

Two-thirds of all contracts are awarded by the Department of Defense. Regional-level analyses

[7]We are grateful to a federal government contracting officer, who preferred to remain anonymous, for clarifying the details of each contract type.

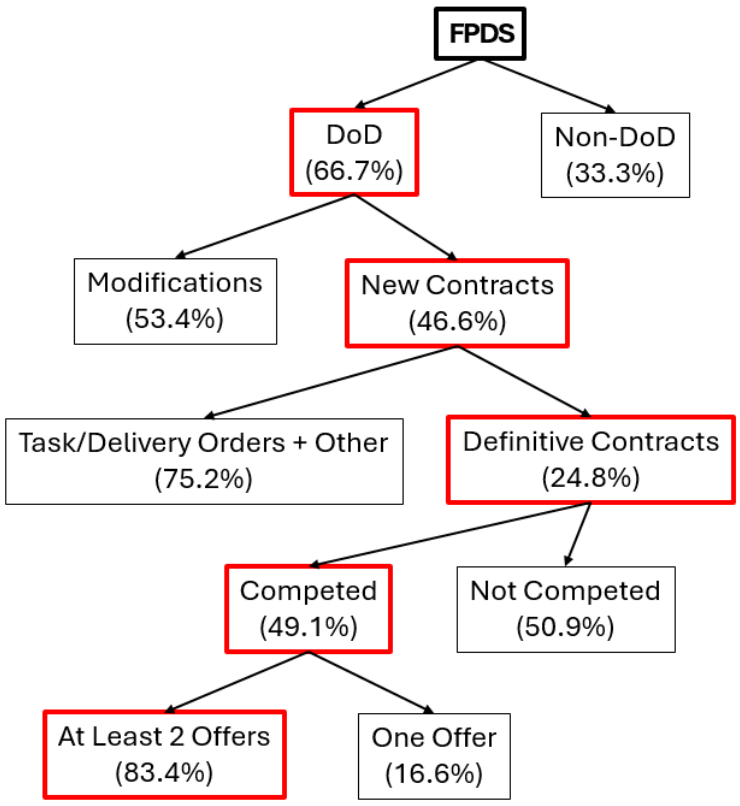


FIGURE 2 — BREAKDOWN OF FPDS CONTRACTS

Notes: Data refer to averages of fiscal-year shares, calculated using contract values rather than the number of contracts. Sample: FY2001–FY2019.

have traditionally focused on defense spending, as its time variation is more plausibly exogenous. Consistent with this approach, we also concentrate on this component of spending in what follows.

Second, only 46.6% of all defense transactions in FPDS are newly awarded contracts. The remainder are contract modifications, such as options, extra work, or administrative actions, all related to an existing contract. Third, not all newly awarded contracts are necessarily "new." In fact, 75.2% of all "new" contracts are task orders (for services) and delivery orders (for goods) issued under a pre-existing parent contract, called an indefinite delivery vehicle (IDV).[8] Conversely, only 24.8% of new defense contracts represent standalone contracts, which are not part of any ongoing relationship with the government. These are technically referred to as "definitive contracts."

[8]IDVs are regulated by the Federal Acquisition Regulation (FAR) 16.5. Specifically, an IDV serves as a mechanism awarded to one or more vendors, streamlining the provision of supplies and services. This method is particularly advantageous for handling both expected and unforeseen needs, simplifying the procurement process by eliminating the need for a new solicitation for each task or delivery order and reducing the paperwork for these orders, among other benefits.

Fourth, contracts can be awarded either competitively or non-competitively. Non-competitive contracts are mainly for complex products, for which agencies often prefer to award "sole-sourced" contracts. For instance, products or services might be deemed available from a sole source if that source offers unique and innovative concepts or proposes a concept or service unavailable from other providers.[9] Figure 2 shows that 50.9% of newly awarded definitive defense contracts fall into this non-competed category. It is noteworthy that FPDS reports the competition details and bid counts from a parent IDV to its subsequent task/delivery orders, even if these orders were not competed. As a result, many contracts seemingly reported as "new" and "competed" in FPDS are neither newly awarded (since their parent IDV may have been awarded months or even years earlier) nor individually competed, as they represent additional, potentially anticipated government orders placed under the terms of the originally competed IDV. Lastly, only 83.4% of new, definitive, competed defense contracts receive at least two offers, signaling an actual competitive scenario.

Overall, only about 5% of total defense procurement is accounted for by new, definitive, and competed contracts with at least two offers. Comparable shares hold for federal non-defense contracts, which make up the remaining one-third of total FPDS contracts.

Moreover, competitively awarded contracts undergo a public solicitation process designed to foster competition.[10] Using SAM data on the universe of federal contract notices from fiscal years 2006 to 2019, we reconstruct the full pre-award to award timeline of competed contracts—for instance, from the earliest pre-solicitation date to the award notice date.[11] We find that the median time from the earliest pre-solicitation to the award notice is 20 days, while 75% of solicited contracts are awarded within 52 days.[12] Thus, analyses conducted at the quarterly or annual level are unlikely to be affected by firms acting in response to favorable solicitations rather than actual awards, as these typically take place within the same time period.

[9] See FAR 6.302-1-a.

[10] See FAR 5, *Publicizing Contract Actions*. Since October 1, 2001, contract actions with an expected value above $25,000 must be publicized on the government platform sam.gov (SAM). Contract actions below this threshold may still be posted to increase visibility.

[11] We follow the guidance in Gonzalez-Lira, Carril, and Walker (2021) to work with solicitation data. We thank Andres Gonzalez-Lira for directing us to the General Services Administration Technical Documentation for the FedBizOpps (FBO) website, whose content has since migrated to Contract Opportunities (SAM).

[12] Further details on contract solicitations are provided in Appendix C.1.

Considerations for Micro and Macro Analysis Taken together, this exposition suggests that many federal contracts, including those related to defense, are at least partly anticipated by firms. This creates challenges for identification at the firm level, which we discuss below. At the regional level, however, the use of Bartik-type instrumental variable provides a way to isolate the unanticipated component of procurement spending by exploiting variation in regional contract exposure induced by unanticipated national changes in defense contracts. As highlighted in our breakdown (Figure 2), this institutional feature of procurement contracting reinforces the case for adopting an instrument when analyzing regional effects of procurement spending. We therefore follow the existing literature and employ the same instrument as Auerbach, Gorodnichenko, and Murphy (2020) in our MSA-level analysis in Sections III through V.

By contrast, establishment-level analyses cannot abstract from contract-level details, since anticipation matters directly for identifying firms' responses. Accordingly, in our establishment-level results in Section VI, we draw on institutional knowledge to restrict attention to the set of contracts that are plausibly unanticipated by direct recipients, arguing that those contracts are still representative contracts plausibly awarded during a regional shock.

In sum, this section motivates a two-tiered empirical strategy. At the regional level, we exploit variation in contract exposure using an instrument to isolate unanticipated shifts in procurement spending and trace their macroeconomic effects. At the establishment level, we focus on plausibly unanticipated contract awards to identify firms' direct responses, enabling us to decompose the contractor-side employment gains into effects at recipient establishments versus the broader contractor network. Together, these approaches allow us to trace how procurement affects employment both at the macro level and through firm-level channels, providing a clearer picture of where and how job creation occurs.

III. The Regional Employment Multiplier

We begin by estimating the overall effect of defense procurement on regional employment to establish a benchmark for our analysis. This aggregate perspective captures both the direct employment

TABLE 1 — OUTCOME VARIABLES DATA SOURCES

Clean (Maximum Sample Size) Datasets:					
Source	*Institution*	*Availability*	*MSAs*	*Sample*	*Tables*
Quarterly Census of Employment and Wages (QCEW)	BEA	Public (Discontinued)	380	2001-2019	Tables A3
Longitudinal Database of Establishments (LDBE)	BLS	Restricted (Research Access Discontinued)	262	2006-2019	Tables A4, A5
Business Dynamics Statistics (BDS)	Census	Public	373	2001-2019	Tables A6, A7
Local Area Unemployment Statistics (LAUS)	BLS	Public	366	2001-2019	Table A8
Harmonized Merged Datasets:					
QCEW+BDS+LAUS	-	-	358	2001-2019	Tables 2, 4, 5, 6, A1, A2
QCEW+BDS+LAUS+LDBE	-	-	254	2006-2019	Tables 7, 8, A9, A10, A11

Notes: The QCEW public data table from the BEA is called `CAINC4_ALL_AREAS_1969_2022.csv`. The table has now been discontinued by the BEA due to budget cuts but it is still available for download from the BEA archive. The number of MSAs is obtained after merging the datasets with a common zipcode, to county, to CBSA crosswalk available from `www.huduser.gov`, which is used to merge with FPDS contracts data.

response among contractors and broader spillover effects. Anchoring our study in this baseline allows us to situate our findings within the broader literature on fiscal multipliers and procurement-driven demand, while also laying the groundwork for a more granular examination of employment dynamics that follows.

Data We collect MSA-level data from different sources, summarized in the top panel of Table 1. The data are then merged into two harmonized datasets illustrated in the bottom panel of the Table.

First, we use the public version of the QCEW provided by the BEA to measure total employment, wages, personal income, and population at annual frequency from 2001 to 2019 for 380 MSAs. Second, we obtained access to firm-level microdata covering 42 states and DC from the Longitudinal Database of Establishments (LDBE), the BLS microdata used to produce the public QCEW.[13] After matching the universe of establishments with the universe of defense contractors from FPDS, we aggregate private employment data at annual frequency from 2006 to 2019 for

[13] Access to a state's microdata is automatic upon BLS approval for certain Cooperative Agreement Signatory States, but requires separate state approval from non-signatory states. The restricted access to researchers program has recently been discontinued by the BLS amid budget cuts.

262 MSAs. This allows us to break down the regional time series of employment into contractors (i.e., matched firms) and non-contractors (i.e., not matched firms). Third, we use the Business Dynamics Statistics (BDS) from the Census to break down private employment and the total number of firms by firm size. These data are available at annual frequency from 2001 to 2019 for 373 MSAs. Lastly, we use labor force and unemployment data from Local Area Unemployment Statistics (LAUS), provided by the BLS. We collect data at annual frequency from 2001 to 2019 for 366 MSAs.

We then harmonize samples by merging the LAUS, BDS, and public QCEW data. This merged dataset includes 358 MSAs observed from 2001 to 2019. Our baseline results reported in the paper rely on this dataset. Appendix A.3 replicates results using also the largest clean available dataset for each analysis, as listed in the top panel of Table 1.

The restricted QCEW data from LDBE impose a more severe sample reduction: the harmonized merged dataset from all four sources goes from 2006 to 2019 and includes 254 MSAs. Baseline results which break down the employment multiplier into contractor and non-contractor responses use this database. In addition, Appendix A.4 presents robustness checks for all other regional analysis in the paper based on this smaller common dataset. In this case, some estimates lose precision, but the results remain qualitatively identical to those reported in the main text. Furthermore, employment multipliers estimated with this sample are consistent with those ones obtained from the larger harmonized sample.

Estimation of the Regional Employment Multiplier Following the regional multiplier literature, we estimate the following equation:

$$\frac{E_{\ell,t+h} - E_{\ell,t-1}}{E_{\ell,t-1}} = \beta_h \cdot \frac{G_{\ell,t+h} - G_{\ell,t-1}}{Y_{\ell,t-1}} + \lambda_{t,h} + \alpha_{\ell,h} + u_{\ell,t+h}, \tag{1}$$

where $E_{\ell,t}$ represents employment in region ℓ and year t, and $Y_{\ell,t-1}$ is annual regional wages and salaries.[14] The terms $\alpha_{t,h}$ and $\lambda_{\ell,h}$ are time and location fixed effects, respectively (specific to

[14]Results are robust if we use personal income as a normalizing weight, as in Muratori, Juarros, and Valderrama (2023). Using wages and salaries is more similar to the use of earnings as the normalization factor in Auerbach, Gorodnichenko, and Murphy (2020).

the horizon of the estimate h). The government spending measure $G_{\ell,t}$ represents defense contracts from FPDS, aggregated by region-year.[15]

The estimand of interest, β_h, measures the percentage increase in regional employment in response to a 1% increase in defense spending relative to wages and salaries.

Identification Two main concerns arise in studying the impact of federal procurement spending on employment. First, the geographical distribution of contracts may be partly endogenous due to political factors (Mintz, 1992). Second, large federal contractors may forecast future regional government demand and increase production in anticipation of future contracts, meaning that it is necessary to isolate a shock that is not only exogenous to the state of the economy but also unanticipated (Ramey, 2011; Auerbach, Gorodnichenko, and Murphy, 2020). This is particularly true for federal procurement spending, as most of it is awarded through long-term agreements (see Figure 2).

In other words, the main regressor in Equation (1) contains an endogenous and potentially anticipated numerator. To address these concerns, Auerbach, Gorodnichenko, and Murphy (2020) construct an instrument that replaces the endogenous and potentially anticipated numerator, $G_{\ell,t+h} - G_{\ell,t-1}$, with an exogenous and unanticipated counterpart. Specifically, the regional change in defense contracts is replaced by the national change in defense contracts, $G_t := \sum_\ell G_{\ell,t}$, reallocated across regions using the long-run exposure of contracts flowing to each region, $\exp_\ell$:

$$\exp_\ell := \frac{1}{19} \sum_{t=2001}^{2019} \frac{G_{\ell,t}}{G_t}.$$

The resulting instrument is:

$$Z_{\ell,t+h} := \frac{\exp_\ell \cdot (G_{t+h} - G_{t-1})}{Y_{\ell,t-1}},$$

which corresponds to the original main regressor of Equation (1), but with the numerator purged

[15] Defense contracts are proxied by contracts awarded by the Department of Defense (agency code 97). Location is identified by means of the primary place of performance zip-code and, when missing, the recipient zip-code. Zip-code to MSA cross-walks are used to identify the final location. Unlike Demyanyk, Loutskina, and Murphy (2019) and Auerbach, Gorodnichenko, and Murphy (2020), who spread Department of Defense contracts over their duration, we assign to contracts their award date from FPDS.

of potentially endogenous and anticipated components.

Papers in this literature have referred to this instrument as a Bartik-type instrument (Bartik, 1991) or a shift-share instrument. However, it has been noted that this instrument, $Z_{\ell,t+h}$, does not constitute an exact shift-share setup.[16] In principle, there is nothing problematic about the instrument proposed by Auerbach, Gorodnichenko, and Murphy (2020) which is widely used in the literature. Nevertheless, recasting the instrumentation problem within an exact shift-share framework is a useful robustness exercise, as the econometric properties of such instruments are well understood (Goldsmith-Pinkham, Sorkin, and Swift, 2020; Borusyak, Hull, and Jaravel, 2022).

In Appendix B.2, we show that the problem of estimating regional multipliers can be recast as a special case shift-share instrumental variable problem (Bartik, 1991), where there is only heterogeneous regional exposure to one industry shock, namely the military spending shock. In that section, we demonstrate that baseline estimates of the employment multiplier using an exact shift-share instrumental variable are consistent with the baseline estimates proposed here. We thus refer to this instrument as a shift-exposure instrument throughout the rest of the paper.

Our identification strategy asks whether differential exposure to national defense spending expansions leads to differential changes in employment. Table B2 in Appendix B indeed shows that our instrument cannot be predicted by past values of our outcome variable. Specifically, we show that lagged employment growth rates, the lagged values of the LHS of Equation (1) with $h = 0$, have no predictive power for our baseline instrument, $Z_{\ell,t+h}$, which suggests that the instrument is orthogonal to a set of potential (pre-determined) confounders. Hence, regions more exposed to the regional shock were not those experiencing abnormal past employment growth rates; in other words, they were not on systematically stronger or weaker economic growth trajectories.

Instrument Time-Variation Figure 3 shows the real value of defense contracts directed to MSAs from 2001 to 2019. Differences in the level of this variable are used to create time variation in the numerator of the instrument.

National changes in defense expenditure are largely driven by exogenous geopolitical and fis-

[16] We thank Gabriel Chodorow-Reich for raising this point when discussing the paper at the 2025 NBER Conference: Fiscal Dynamics of State and Local Governments.

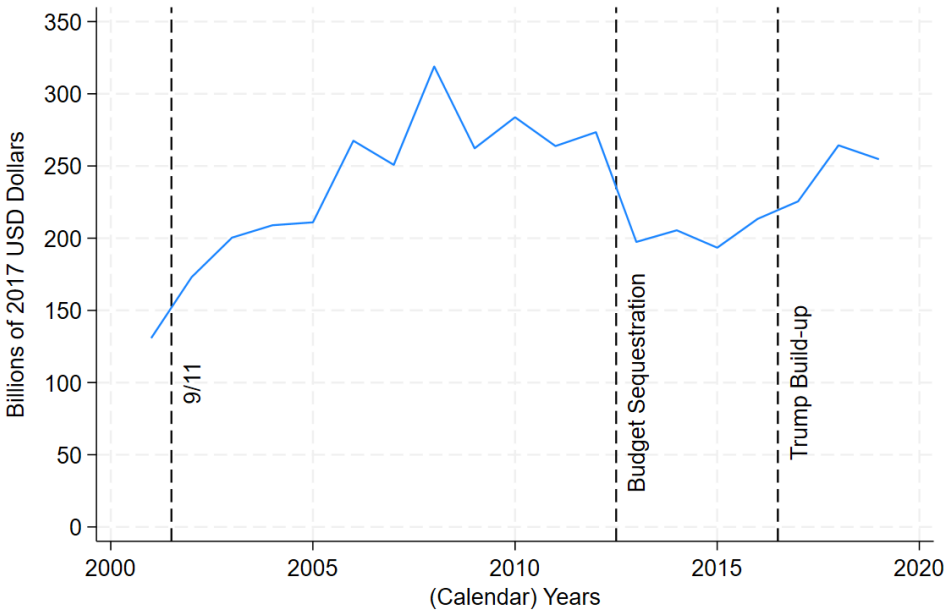


FIGURE 3 — DEFENSE CONTRACTS IN MSAS BY YEAR

cal events. In the early 2000s, defense spending increased substantially following the 9/11 terrorist attacks and the ensuing wars in Afghanistan and Iraq. In the aftermath of the financial crisis, concerns about the worsening government deficit led the Obama administration to announce budget cuts in 2011, which were repeatedly delayed until March 2013, when *budget sequestrations* were finally implemented. Subsequently, the Russian invasion of Crimea in 2014 and the establishment of unified Republican control of Congress and the Presidency after President Trump's election in 2016 triggered a substantial increase in defense procurement spending, reversing the downward trend caused by the sequestrations. The fiscal policy literature has labeled these events as exogenous to output variation.[17]

These examples illustrate that, although the short time series ($T = 19$) may raise concerns of coincidental correlations—such as the overlap of the 9/11 build-up with the Dotcom crash—there is no systematic countercyclical pattern between national defense spending shifts and national economic growth. Indeed, defense spending cuts in 2013 occurred despite modest growth (nominal GDP grew by about 2%), while the 2016 military build-up coincided with strong growth.

[17] Ramey and Zubairy (2018) record large positive shocks after 9/11 and a sharp negative shock in 2013 due to sequestration in their defense news shock series. Alesina, Favero, and Giavazzi (2014) list the Budget Sequestration Act as an exogenous expenditure-based fiscal consolidation. Amodeo and Briganti (2025) provide a full narrative of major events driving the path of U.S. defense spending in the post-2000 sample, including those discussed here.

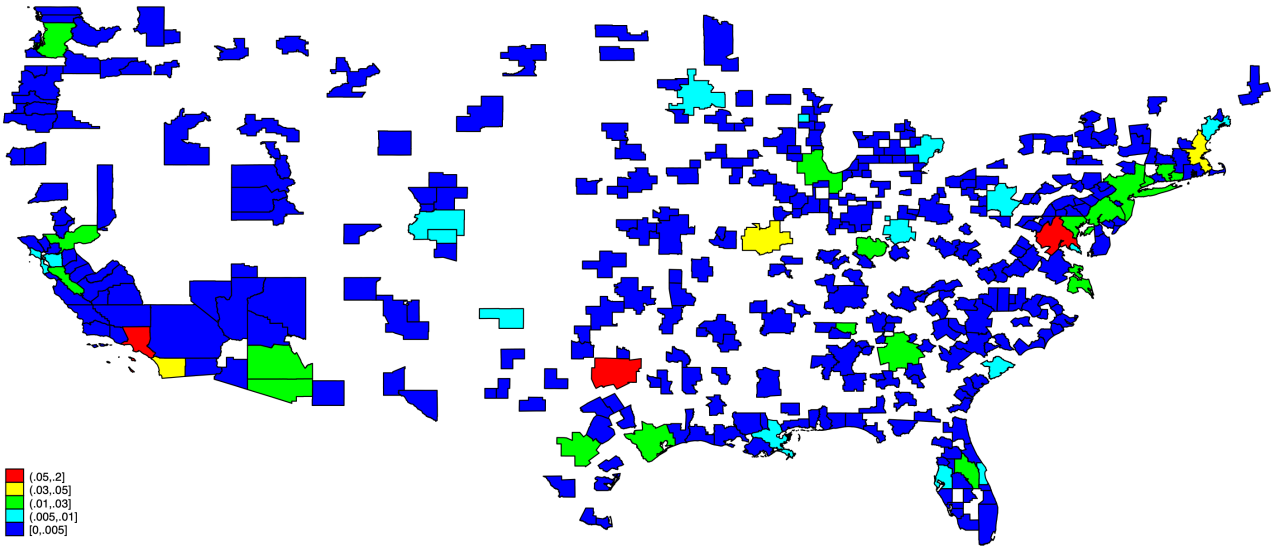


FIGURE 4 — MSA DISTRIBUTION OF LONG-RUN EXPOSURE OF DEFENSE CONTRACTS ($\exp_\ell$)

Notes: The figure shows the value of the long-run exposure to national-level shocks to defense contracts, used in the numerator of the instrument ($\exp_\ell := \frac{1}{T}\sum_{t=2001}^{2019} \frac{G_{\ell,t}}{\sum_\ell G_{\ell,t}}$). Red means long-run exposure larger than 5%. Yellow is from 3% to 5%; green is from 1% to 3%; cyan is from 0.5% to 1%; blue is less than 0.5%. The figure omits Hawaii and Alaska for graphical purposes.

Moreover, the inclusion of time fixed effects ($\lambda_{t,h}$) in our baseline specification ensures that any remaining aggregate variation, potentially correlated with national shifts in defense spending and employment growth rates, is fully absorbed. This benefit comes at the cost of interpreting our employment multiplier not as a national-level multiplier but rather as a cross-sectional multiplier. Cross-sectional multipliers can be viewed as a lower bound of deficit-financed, no-monetary-policy, closed-economy national multipliers (Chodorow-Reich, 2019).

Instrument Cross-Sectional Variation The instrument relies on the long-run regional exposure to defense contracts, $\exp_\ell$, to distribute national-level shifts across regions. Given our relatively short time dimension compared with the cross-sectional size of the sample ($N \gg T$), it is desirable to rely on exogenous exposures. Figure 4 illustrates their geographic distribution.

The MSA with the largest share of contracts is Washington–Arlington–Alexandria, accounting for about 12% of Department of Defense spending. This concentration reflects the presence of the Pentagon and numerous other military installations, whose location was determined by proximity to the capital and the White House. More broadly, MSAs with high defense contract

TABLE 2 — REGIONAL EMPLOYMENT MULTIPLIERS - BASELINE ESTIMATES

Response of Total Employment from (Public) BEA Data					
Horizon	*Coefficient* (β_h)	*p*	*Effective F*	*Job-Years ($1M)*	*Cost-per-Job ($)*
impact	0.034	0.021	29.232	1.111	$899,756
	(0.015)			(0.481)	($389,098)
1 year	0.104	0.006	53.120	3.440	$290,733
	(0.038)			(1.244)	($105,111)
2 years	0.099	0.016	27.603	3.275	$305,332
	(0.041)			(1.351)	($125,994)
3 years	0.107	0.026	21.063	3.524	$283,746
	(0.048)			(1.581)	($127,316)

Notes: Sample: 2001-2019; 358 MSAs (QCEW+BDS+LAUS Harmonized Sample). GDP price deflator from BEA, base year 2008. Robust standard errors in parentheses, clustered at the MSA level. Results using either two-way clustered standard errors by state and year or using the Driscoll and Kraay (1998) standard errors are of similar magnitudes. Montiel Olea and Pflueger (2013) effective F is calculated with `weakivtest`. Number of Job-Years refers to one million $. Standard error of cost-per-jobs are obtained with the Δ-method.

shares are typically characterized by long-standing military activities whose location was determined by geostrategic, rather than economic, considerations well before the start of our sample period.[18] Thus the geographic allocation of national funds across regions is plausibly predetermined relative to current economic conditions. In Appendix B.2, we show that our regional exposures, plotted in Figure 4, are correlated with the shares of a special case shift-share instrument, constructed as long-run averages of the ratio of regional defense contracts to regional wages and salaries: $w_\ell := \frac{1}{19}\sum_{t=2001}^{2019} \frac{G_{\ell,t}}{Y_{\ell,t}}$. We also show that there is no systematic difference in the time-average annual employment growth rates, LHS of Equation (1) with $h = 0$, between regions with low and high shares. Moreover, even if such differences existed, our baseline regression includes region fixed effects ($\alpha_{\ell,h}$), which absorb any time-invariant heterogeneity in employment growth across MSAs.

Baseline Estimates We calculate regional employment multiplier by estimating Equation (1) with 2SLS, using $Z_{\ell,t+h}$ as an instrument for the regional change in defense contracts. The left panel of Table 2 reports the 2SLS estimates of Equation (1).

[18] Other large MSAs include Fort Worth-Dallas (about 7%, Naval Air Station Joint Reserve Base Fort Worth, 1942), Los Angeles (5%, Los Angeles Air Force Base, 1962), San Diego-Carlsbad (3%, Naval Base San Diego, 1918), and St. Louis (3.1%, Scott Air Force Base, 1917). The locations of these installations, established long before the sample period, reflect strategic rather than economic considerations.

First, values of the F-statistics are generally above the suggested critical value of 23, indicating no weak-instrument problem (Montiel Olea and Pflueger, 2013). Second, multipliers on impact are estimated precisely but their magnitude is economically small; in the next section we will show that this result stems from crowding-out of workers from non-contractor firms, on impact.

The three-year employment multiplier is approximately 0.1, meaning that an increase in regional defense contracts by 1% of regional wages and salaries, result in 0.1% increase in regional employment. To gain interpretability, we construct the implied number of job-years as follows:

$$\text{job-years}_h := \frac{\beta_h}{N\,T} \sum_{\ell=1}^{N} \sum_{t=2001+1}^{2019} \frac{\$1,000,000}{Y_{\ell,t-1}} \cdot E_{\ell,t-1},$$

where β_h represents the estimated employment multipliers in the second column of Table 2. The reciprocal of the number of job-years give us the estimate of the cost-per-job consistent with Chodorow-Reich (2019) and Muratori, Juarros, and Valderrama (2023).

Our two-year employment multiplier implies a cost-per-job of approximately $305,000 in 2008 dollars. We use 2008 as the base year to make our estimates comparable to papers on ARRA transfers, which typically use nominal contemporaneous dollars. For context, Chodorow-Reich (2019) review ARRA transfers and report a cost-per-job range (also at the two-year horizon) between $25,000 and $125,000. Table 3 presents a range of cost-per-job estimates from recent empirical studies of fiscal interventions, with a particular focus on defense procurement and ARRA transfers.[19]

Contemporary estimates of the employment effects of defense procurement suggest notably higher costs per job than those associated with broader fiscal stimuli like ARRA transfers or local spending. For example, the estimates of Auerbach, Gorodnichenko, and Murphy (2020) correspond to a cost-per-job at the two-year horizon of roughly $237,000 2008 dollars. These figures are broadly consistent with our estimate of $305,000 at the same horizon. Moreover, in contemporaneous work, Park, Zhou, and Zubairy (2025) use quarterly defense contracts data from 2011

[19] Other estimates from non-U.S. data include: Corbi, Papaioannou, and Surico (2019), who estimate $8,000 per year using Brazilian municipal transfers; Buchheim and Watzinger (2023), who find $24,000 per year from German public investment in school energy efficiency; and Gabriel, Klein, and Pessoa (2023), who find €30,000 per year from regional Eurozone data.

TABLE 3 — COST-PER-JOB - REVIEW OF ESTIMATES

Study	*Type of G*	*Sample*	*Geography*	*Job-Years*	*Cost-per-Job*
Nakamura and Steinsson (2014)	Defense Contracts	1966-2006	US States	2.23	$44,836
Dupor and Guerrero (2017)	Defense Contracts	1951-2014	US States	0.05	$1,872,639
Demyanyk, Loutskina, and Murphy (2019)	Defense Contracts	2007-2009	828 US CBSAs	0.67	$148,711
Auerbach, Gorodnichenko, and Murphy (2020)	Defense Contracts	2001-2016	383 US MSAs	0.42	$236,822
Muratori, Juarros, and Valderrama (2023)	Defense Contracts	1979-2019	US MSAs	1.43	$69,817
Park, Zhou, and Zubairy (2025)	Defense Contracts	2011-2020	828 US Counties	0.29	$343,659
Wilson (2012)	ARRA Transfers	2009-2010	US States	0.80	$123,839
Conley and Dupor (2013)	ARRA Highway Funding	2009-2011	US States	0.76	$131,578
Serrato and Wingender (2016)	Population Revisions	1980, 1990, 2000	US Counties	3.25	$30,785
Dupor and Mehkari (2016)	ARRA Subcomponents	2008-2010	US Commuting Reg.	0.95	$104,931
Adelino, Cunha, and Ferreira (2017)	Local Spending	2007-2013	US Municipalities	-	$25,000
Dupor and McCrory (2018)	ARRA Subcomponents	2008-2010	US Commuting Reg.	1.85	$54,054
Chodorow-Reich (2019)	ARRA Transfers	2008-2010	US States	2.01	$49,750

Notes: Source is Chodorow-Reich (2019) and authors' calculations using estimates from listed papers. Job-Years per $100,000 calculated at two-year or closest available horizon and using 2008 dollars. For Nakamura and Steinsson (2014) results are from Table 3, Row 1, which uses the Bartik instrument for prime military contracts; for Park, Zhou, and Zubairy, 2025 we use estimates unadjusted for subcontracting for comparability. Geography refers to main estimates. Estimates from Dupor and Guerrero, 2017 depend heavily on inclusion of years 1953-1954 in the sample, as noted in that paper.

to 2024 and estimate a cost-per-job of about $343,000 (in 2008 dollars) at the county level. This evidence suggests that military procurement in the twenty-first century has become significantly less cost-effective at generating employment compared to either earlier waves of defense spending (Nakamura and Steinsson, 2014; Muratori, Juarros, and Valderrama, 2023) or other forms of public stimulus, such as transfers (Chodorow-Reich, 2019).[20]

We highlight that this newly documented fact does not appear to be driven by three potential explanations: subcontracting, cross-MSA spillovers, and the lengthy duration of contracts.

First, when defense contracts are awarded to firms, those firms may subcontract part of the work to companies located in different geographical units than the awardee. This mechanism could generate a downward bias in employment multipliers, which in turn raises the implied cost-per-job estimate. Park, Zhou, and Zubairy (2025) document this phenomenon and show that adjusting for subcontracting reduces their baseline cost-per-job estimate from approximately $406,000 to $333,000 in 2019 dollars (roughly $344,000 to $282,000 in 2008 dollars). This value is only slightly lower than ours and remains substantially higher than estimates from evaluations of non-defense spending. Hence, while subcontracting may contribute to the observed high cost-per-

[20]It should be noted that the impact of transfers may vary significantly with economic conditions. For example, recent research suggests that federal-state transfers during the COVID pandemic produced additional employment at a cost of roughly $225,000 2019 dollars per job-year (Clemens, Hoxie, and Veuger, 2025).

job, it is unlikely to be the primary reason why defense procurement has recently failed to generate a low cost-per-job.

Second, one may argue that the high cost-per-job stems from the use of sub-state geographic aggregation, which might miss employment generated from cross-MSA spillovers. Investigating this hypothesis, Auerbach, Gorodnichenko, and Murphy, 2020 estimate an outflow effect of contracts slightly above half of the direct effect, concentrated entirely within 50 miles of the receiving MSA. Since this analysis is conducted at the MSA level, and assuming that employment gains are proportional to earnings gains, the conversion factor from their estimated coefficients to the number of job-years would be unchanged. In other words, accounting for these spillovers would reduce the implied cost-per-job estimate to roughly two-thirds of the baseline value — around $160,000 in 2008 dollars — which remains an upper bound of the estimates presented in Table 3.[21] Overall, this suggests that cost-per-job estimates would still be high even after adjusting for plausible cross-MSA spillovers.

Third, we measure regional government spending using defense contracts and assign the full contract value to the award year. This approach is intended to capture potential anticipatory behavior in response to the fiscal shock represented by contract awards. Indeed, firms may respond rapidly to newly awarded contracts by raising inventories (Briganti, Brunet, and Sellemi, 2025). However, while the full value of a contract is counted immediately when calculating the cost-per-job, its employment effects are spread over the contract's duration. This timing mismatch may help explain why our impact estimates of cost-per-job are high relative to one-year horizons. Nonetheless, we believe this choice does not materially affect estimates beyond the short run. The delay of defense procurement spending as recorded in the national accounts (NIPA) relative to defense contracts (FPDS) appears to be on the order of one to two years (Figure 1). Consistently, Briganti, Brunet, and Sellemi (2025) report an average delay of three to four quarters using a longer quarterly series of defense procurement contracts and spending. Finally, Demyanyk, Loutskina,

[21] Let the direct effect be θ and the conversion factor be c, so that the cost-per-job is $1/(\theta \cdot c)$. If the outflow effect is approximately one half of the direct effect, the total effect becomes $\theta + \frac{1}{2}\theta = \frac{3}{2}\theta$. Hence, the cost-per-job accounting for spillovers is $\frac{1}{\left(\frac{3}{2}\theta \cdot c\right)} = \frac{2}{3} \cdot \frac{1}{\theta \cdot c}$, that is, two-thirds of the direct-effect estimate.

and Murphy (2019) analyze both contracts and spending and obtain very similar estimates, while Auerbach, Gorodnichenko, and Murphy (2020) spread contracts over their duration and find similar results to ours (see Table 3). Together, this evidence suggests that our choice does not affect results beyond the impact horizon.

We believe that a definitive explanation lies beyond the scope of this paper, whose contribution is to be the first to systematically document this fact and provide an empirical breakdown of the employment multiplier using micro-level matched data.

That said, one plausible explanation is a structural shift in the occupational and industrial composition of defense-related activity in the twenty-first century. Specifically, if the first-order employment effects of defense contracts are concentrated within recipient firms operating in high-paying, skill-intensive industries, then the resulting cost-per-job will naturally be higher. In Section V, we show that the initial employment gains are concentrated within contractors. Complementary evidence from Bartal and Becard (2024) shows that modern U.S. defense contractors are increasingly concentrated in high-tech sectors such as aerospace, software engineering, cybersecurity, and advanced manufacturing. These industries employ highly educated professionals, including aerospace engineers, software developers, mathematicians, and cryptographers, leading to a higher wage bill per job created. This shift in the industrial composition of defense contracting can plausibly account for the higher cost-per-job estimates observed in the post-2000 period.

Origin of the Positive Multiplier. The positive MSA-level employment multiplier indicates that, when additional government contracts are awarded in a region, employment is not merely reallocated from other firms—a mechanism that would yield a zero regional multiplier. Instead, extra workers must come from one of three sources: (i) the pool of unemployed, (ii) new entrants to the labor force from the existing regional population, or (iii) other regions, through migration or cross-MSA commuting.

Regarding channels (i) and (ii), in Appendix A.1 we use Local Area Unemployment Statistics (LAUS) data to show that increases in the regional labor force and reductions in unemployment explain the bulk of the additional employment, with labor force changes playing a major role. This

is in line with Auerbach, Gorodnichenko, and Murphy (2024) who also find significant and positive effects on the labor force and reductions in unemployment rate using data from the American Community Survey (ACS). Moreover, in Appendix A.2 we also show, using Business Dynamics Statistics (BDS) data, that the creation of new firms (the extensive firm margin) is negligible over the horizon we study. This indicates that it is existing firms hiring more individuals from the labor force and the pool of unemployed that generate the positive employment multiplier, rather than newly created firms. Using data from the U.S. Census, Auerbach, Gorodnichenko, and Murphy (2024) also find insignificant effects on the number of establishments.

Regarding channel (iii), we expect migration to play only a minor role for three reasons: the temporary nature of the regional shocks, the short (three-year) horizon of our analysis, and the fact that MSAs, by construction, are small open economies centered around a metropolitan nucleus (similar to commuting zones), which limit cross-regional commuting and migration. Consistent with this, Auerbach, Gorodnichenko, and Murphy (2024) find no significant effect on population changes within three years of a regional shock, which coincides with our horizon. Recent work by Foschi et al. (2025) argue that over a five-year horizon, the bulk of the employment response to demand shocks is accounted for by migration.[22] However, there is one notable exception in their findings: when replicating the Auerbach, Gorodnichenko, and Murphy (2020) results, they show that population does not respond to regional shocks to defense procurement. Instead, the positive employment response is entirely driven by labor force increases and reductions in unemployment, consistent with our findings.

IV. Breaking Down the Employment Multiplier by Firm Size

In the previous section, we extended existing results on employment multipliers using more recent and alternative data sources. In the remainder of the paper, we provide novel decompositions of these employment effects along several dimensions to identify its sources and shed light on the transmission mechanism linking government purchases and the labor market.

[22] In particular, they challenge the view that U.S. labor markets have become less flexible in recent years, providing evidence that the elasticity of employment to migration induced by demand shocks has remained constant over time.

Recent findings in the fiscal-policy literature emphasize the role of small firms in amplifying fiscal multipliers via a financial-accelerator mechanism: Hebous and Zimmermann (2020), Budrys (2022), and Gabriel (2024) show that small firms respond more strongly to procurement contracts than large firms; Juarros (2022) shows that the fiscal multiplier increases with a region's small-firm share of employment; di Giovanni et al. (2023) show that procurement contracts help small firms relax financial constraints, although policies that tilt procurement toward small—rather than large—firms may have non-trivial macroeconomic effects on output. However, there are no previous estimates of the share of defense contracts going to such firms or of their overall contribution to the resulting growth in regional employment, making it difficult to gauge whether this mechanism constitutes an important driver of the regional employment response.

We show that defense-spending shocks primarily affect regional employment through growth in the employment of large firms, which receive the bulk of the incremental contracts driven by the regional shocks. This result is not inconsistent with the existence of financial-accelerator effects among small firms, which may be quite significant at the firm level, but it suggests that first-order effects on employment are driven by large firms. We draw on the Business Dynamics Statistics (BDS) to answer this question.

We thus start by establishing that BDS allows us to replicate the results from Section III. We thus use data from BDS private employment, instead of data from BEA total employment, to estimate Equation (1). Table 4 reports the results: employment multipliers are qualitatively similar to those estimated with the public BEA data (see Table 2).

Breakdown by Firm Size Given that the BDS sample yields consistent employment multipliers, we proceed to break down the employment multiplier by firm size. BDS provides an MSA–year breakdown of employment by firm size: (i) small firms employ fewer than 20 workers; (ii) medium firms employ 20–499 workers; and (iii) large firms employ at least 500 workers.[23] We decompose the regional BDS employment growth rate into three size categories:

[23] BDS classifies firm size by aggregating employment across establishments within the same firm. If firm X operates a 10-employee establishment in region A and a 20-employee establishment in region B, it is classified as a medium firm because its total employment is 30. The 10 employees in A and the 20 in B are counted as medium-firm employment in A and B, respectively.

TABLE 4 — ESTIMATES FROM BUSINESS DYNAMICS STATISTICS CONSISTENT WITH BASELINE

Response of Private Employment from (Public) BDS Data				
Horizon	*Coefficient*	*p*	*Effective F*	*Job-Years ($1M)*
impact	0.062	0.056	29.232	1.296
	(0.032)			(0.676)
1 year	0.108	0.008	53.120	2.259
	(0.040)			(0.842)
2 years	0.115	0.015	27.603	2.393
	(0.047)			(0.981)
3 years	0.121	0.027	21.063	2.527
	(0.055)			(1.140)

Notes: Sample: 2001-2019 - 358 MSAs (QCEW+BDS+LAUS Harmonized Dataset). Data source: Business Dynamics Statistics (BDS). All else equal to Table 2.

$$\frac{E_{\ell,t+h} - E_{\ell,t-1}}{E_{\ell,t-1}} = \frac{E^{\text{Small}}_{\ell,t+h} - E^{\text{Small}}_{\ell,t-1}}{E_{\ell,t-1}} + \frac{E^{\text{Medium}}_{\ell,t+h} - E^{\text{Medium}}_{\ell,t-1}}{E_{\ell,t-1}} + \frac{E^{\text{Large}}_{\ell,t+h} - E^{\text{Large}}_{\ell,t-1}}{E_{\ell,t-1}}$$

We then estimate:

$$\frac{E^{\text{Small}}_{\ell,t+h} - E^{\text{Small}}_{\ell,t-1}}{E_{\ell,t-1}} = \beta^{\text{s}}_h \frac{G_{\ell,t+h} - G_{\ell,t-1}}{Y_{\ell,t-1}} + \lambda^{\text{s}}_{t,h} + \alpha^{\text{s}}_{\ell,h} + u^{\text{s}}_{\ell,t+h}, \tag{2}$$

$$\frac{E^{\text{Medium}}_{\ell,t+h} - E^{\text{Medium}}_{\ell,t-1}}{E_{\ell,t-1}} = \beta^{\text{m}}_h \frac{G_{\ell,t+h} - G_{\ell,t-1}}{Y_{\ell,t-1}} + \lambda^{\text{m}}_{t,h} + \alpha^{\text{m}}_{\ell,h} + u^{\text{m}}_{\ell,t+h}, \tag{3}$$

$$\frac{E^{\text{Large}}_{\ell,t+h} - E^{\text{Large}}_{\ell,t-1}}{E_{\ell,t-1}} = \beta^{\text{l}}_h \frac{G_{\ell,t+h} - G_{\ell,t-1}}{Y_{\ell,t-1}} + \lambda^{\text{l}}_{t,h} + \alpha^{\text{l}}_{\ell,h} + u^{\text{l}}_{\ell,t+h}. \tag{4}$$

The two-stage least squares (2SLS) estimates of Equations (2)–(4) produce coefficients β^{s}_h, β^{m}_h, and β^{l}_h, which measure the effects on employment in small, medium, and large firms, respectively. These estimates are reported in Table 5. By construction, the size-specific coefficients sum to the baseline effect, $\beta^{\text{s}}_h + \beta^{\text{m}}_h + \beta^{\text{l}}_h = \beta_h$ (see Table 4).

Table 5 shows that only the employment response of large firms is significantly different from zero. Point estimates indicate that changes in large-firm employment account for nearly the entire impact of regional shocks on total employment. For example, when the first-stage F-statistic reaches its maximum one year after the shock, the employment multiplier is 0.108 (in green), distributed across firm sizes as 0.006 for small firms, 0.007 for medium firms, and 0.095 for large firms.

TABLE 5 — EMPLOYMENT MULTIPLIER IS DRIVEN BY LARGE FIRMS

	Small Firms			*Medium Firms*			*Large Firms*		
Horizon	*Coefficient* (β_h^s)	*p*	*Fraction (%)*	*Coefficient* (β_h^m)	*p*	*Fraction (%)*	*Coefficient* (β_h^l)	*p*	*Fraction (%)*
impact	-0.003	0.718	-4.1%	0.000	0.972	0.8%	0.064	0.034	103.4%
	(0.007)			(0.013)			(0.030)		
1 year	0.006	0.336	5.2%	0.007	0.520	6.7%	0.095	0.013	88.1%
	(0.006)			(0.011)			(0.038)		
2 years	0.005	0.392	4.5%	0.014	0.155	12.2%	0.095	0.019	83.3%
	(0.006)			(0.010)			(0.041)		
3 years	0.006	0.341	4.6%	0.012	0.224	10.1%	0.103	0.036	85.3%
	(0.006)			(0.010)			(0.049)		

Notes: Sample: 2001-2019 - 358 MSAs (QCEW+BDS+LAUS Harmonized Dataset). Data source: Business Dynamics Statistics (BDS). Estimates of Equations (2) (β_h^s), (3) (β_h^m) and (4) (β_h^l). Fraction is calculated as β_h^s/β_h for small firms, β_h^m/β_h for medium firms and β_h^l/β_h for large firms, using the values of β_h from Table 4 (values reported in green).

The large-firm response is the only statistically significant component and accounts for nearly 90% of the total employment response at that horizon. This result is robust to using different samples.[24]

Breaking Down Government Spending by Firm Size We next show that the stronger response of large firms in Table 5 owes to regional shocks triggering contracts to large firms, rather than defense spending having dramatically larger effects on firms in some size category than in others.

In order to verify this, we disaggregate yearly MSA-level defense contracts, $G_{\ell,t}$, by recipient-firm size and show that a regional shock mainly increases contracts awarded to large firms. For this purpose, we use the National Establishment Time Series (NETS), a privately maintained census of U.S. establishments. After extensively cleaning the NETS data to increase comparability to the BDS, following the procedures laid out in Barnatchez, Crane, and Decker (2017), and described in more detail in Appendix C.7, we match FPDS data on contracts to firms in NETS using the DUNS number of the recipient (or, if there is no match, the DUNS of the recipient's parent company).[25] We are able to match 97.6% of total FPDS defense contracts (by value) to firms in NETS using this procedure.

[24] Results using the full sample are reported in Appendix A.3 and results for the smaller harmonized sample (QCEW+BDS+LAUS+LDBE) are presented in Appendix A.4. Employment multipliers from BDS private employment data are positive, significant, and its breakdown is consistent with our other estimates, although there is some evidence of modest responses from medium firms in the most restrictive sample.

[25] NETS is fundamentally an establishment-level database. As in Barnatchez, Crane, and Decker (2017) we aggregate establishments up to their ultimate headquarters establishment, which is treated as a firm. We also acknowledge very helpful support from Joonkyu Choi.

TABLE 6 — BARTIK SHOCKS MOSTLY AFFECT CONTRACTS AWARDS TO LARGE FIRMS

	Small Firms ($G_{i,t}^{Small}$)			*Medium Firms* ($G_{i,t}^{Medium}$)			*Large Firms* ($G_{i,t}^{Large}$)		
Horizon	*Coefficient*	*p-value*	*Fraction*	*Coefficient*	*p-value*	*Fraction*	*Coefficient*	*p-value*	*Fraction*
impact	0.024	0.045	2.4%	0.136	0.151	13.7%	0.832	0.000	83.8%
	(0.012)			(0.095)			(0.102)		
1 year	0.043	0.005	4.3%	0.184	0.021	18.6%	0.763	0.000	77.1%
	(0.015)			(0.079)			(0.084)		
2 years	0.038	0.006	3.9%	0.178	0.037	18.0%	0.773	0.000	78.2%
	(0.014)			(0.085)			(0.092)		
3 years	0.047	0.012	4.7%	0.196	0.043	19.9%	0.745	0.000	75.4%
	(0.018)			(0.097)			(0.107)		

Notes: Sample: 2001-2019 - 358 MSAs (QCEW+BDS+LAUS Harmonized Dataset).

Once each contract is assigned to a NETS firm, we can aggregate defense contracts in each MSA to a firm size bin, following the BDS definitions of small, medium, and large firms:

$$\frac{G_{\ell,t+h} - G_{\ell,t-1}}{G_{\ell,t-1}} = \frac{G_{\ell,t+h}^{\text{Small}} - G_{\ell,t-1}^{\text{Small}}}{G_{\ell,t-1}} + \frac{G_{\ell,t+h}^{\text{Medium}} - G_{\ell,t-1}^{\text{Medium}}}{G_{\ell,t-1}} + \frac{G_{\ell,t+h}^{\text{Large}} - G_{\ell,t-1}^{\text{Large}}}{G_{\ell,t-1}}$$

We then run parallel estimates to Equations (2)-(4) but using spending rather than employment by category as the outcome variable. Results are presented in Table 6.

The proportion of spending going to large firms is around 80%, on average, and it is the only firm size category estimated with precision at all horizons. Thus, these estimates suggest that the response of employment to changes in defense procurement spending does not have dramatically larger employment effects on large firms, but rather, the regional shocks mostly affect contract awards to large firms.

Interestingly, Table 6 shows that small and medium firms together receive about one fifth of the contracts awarded in response to a regional shock. However, Table 5 indicates that virtually the entire employment response is driven by large firms. One potential explanation for this result is provided by Fact 3 in Park, Zhou, and Zubairy (2025), which shows that recipients of small contracts tend to subcontract their work to large firms.

V. Breaking Down the Employment Multiplier Between Contractor and Non-Contractor Effects

How much of the employment multiplier originates from the effects of contracts on contractors and how much from effects on non-contractors? For policymakers aiming to maximize the employment impact of contracts, understanding this breakdown is crucial. Specifically, if the effects are mostly confined within the sphere of contractors, policymakers should focus on identifying the types of contracts and firms that elicit the strongest responses. Conversely, if spillover effects on non-contractors dominate, targeting more responsive regions or sectors may be more effective.

To answer this question we leverage our access to the restricted microdata from the LDBE on the universe of establishments in the U.S. to break down the regional employment time-series into a contractor and non-contractor component.[26] The BLS's public data on regional employment, the Quarterly Census of Employment and Wages (QCEW) is constructed by aggregating this microdata from the LDBE as follows:

$$\underbrace{E_{\ell,t}}_{\text{Public}} = \sum_{i} \underbrace{E_{i,\ell,t}}_{\text{Restricted}} ,$$

where i denotes an establishment operating in period t in region ℓ, identified by its physical location address and owner firm.

We thus start by establishing that the LDBE data allows us to replicate the results from Section III and IV. We thus use the aggregated LDBE employment, instead of BEA total employment, to estimate Equation (1). Table 7 presents the estimates of the employment multiplier–i.e., Equation (1)–using the harmonized QCEW+BDS+LAUS+LDBE sample which goes from 2006 to 2019, covering 254 MSAs located in the 42 signatory states.

The employment multiplier grows over the horizons and becomes significant after the impact period, mimicking the same dynamics observed for the baseline estimates with public BEA data (Table 2) and the estimates from the BDS data (see Table 4), which also only accounts for privately owned firms.[27] The precision of the estimates as well as the effective F decrease, which is not

[26] The access to this data has now been discontinued by the BEA due to budget cuts.

[27] BDS employment and total QCEW employment counts provided by the BEA do not coincide because of differences

TABLE 7 — LDBE ESTIMATES ARE CONSISTENT WITH ESTIMATES FROM PUBLIC DATA

	Private Emp. from LDBE (Retricted QCEW Data)				Total Emp. from Public BEA Data
Horizon	*Coefficient* (β_h)	*p*	*Effective F*	*Job-Years/$1M*	*Job-Years/$1M*
impact	0.026	0.143	10.019	0.601	0.762
	(0.017)			(0.409)	(0.632)
1 year	0.096	0.008	29.845	2.235	2.755
	(0.036)			(0.841)	(1.318)
2 years	0.101	0.042	7.532	2.356	2.670
	(0.049)			(1.150)	(1.573)
3 years	0.113	0.063	6.561	2.644	2.989
	(0.061)			(1.419)	(1.867)

Notes: Sample: 2006–2019; 254 MSAs (QCEW+BDS+LAUS+LDBE Harmonized Dataset). Estimates of Equation (1) using (i) restricted data from LDBE on private employment (left panel) and (ii) the public BEA data on total employment (right panel). All other details match Table 2.

surprising given the smaller sample size. Nonetheless, the magnitude of the effective F is reassuringly high one year after the shock. Overall, the LDBE data appears to deliver significant estimates despite the smaller sample size.

These results indicate that our data aggregation with the LDBE–i.e., from establishments' employees to regional employment–produces estimates of the employment multiplier which are in line with those ones obtained from the public total employment data of the BEA. Therefore, we present in blue and in the right panel of the table the estimates of job-years obtained using the public BEA data and the smaller harmonized QCEW+BDS+LAUS+LDBE sample. Notice that the values of job-years obtained using the public data also grow over time and are quantitatively very close to the estimates obtained using the restricted LDBE data. Furthermore, it is not surprising that the number of job-years is higher when using total BEA employment than when using private employment from LDBE or BDS, as total employment also accounts for the response of public employees.[28] We see this result as a reassuring robustness check: a much smaller sample is still able to produce comparable estimates.

in the universe of firms covered. The QCEW covers all firms that pay into the unemployment insurance system and bases employment counts on UI payments, while BDS covers nearly all non-government businesses, with employment counts coming from federal tax data. See Barnatchez, Crane, and Decker (2017), p. 10 for a discussion.

[28] Ramey (2013) and Conley and Dupor (2013) suggest that employment responses may also stem from additional public-sector jobs rather than new private-sector jobs.

Breakdown by Contractor Status Given that the LDBE sample yields consistent estimates, we proceed to break down the employment multiplier by contractor status. For each region, we break down employment into two components:

$$\begin{aligned} E_{\ell,t} &= \sum_{i \in \mathcal{C}} E_{i,\ell,t}^{\text{Contractors}} + \sum_{i \notin \mathcal{C}} E_{i,\ell,t}^{\text{Non-Contractors}} \\ &= E_{\ell,t}^{\text{Contractors}} + E_{\ell,t}^{\text{Non-Contractors}}. \end{aligned}$$

Essentially, we identify the set of establishments that receive at least one government contract over the sample period, $\mathcal{C}$, and aggregate employment for these establishments into the component associated with defense contractors. The residual employment component represents establishments that were never directly involved with defense contracting.

This breakdown of employment is implemented by matching the universe of defense contractors from FPDS with the universe of establishments from the restricted QCEW. The matching is restricted to establishments in MSAs located entirely within the 42 signatory states (plus Washington, D.C.) to which we were granted data access[29], observed between 2006 and 2019. It is carried out using a string-matching algorithm, described briefly below.

We start by extensively cleaning and standardizing establishment names in both datasets, as well as the doing-business-as names present in both datasets and the parent name available in FPDS. We start matching firms within county and sharing the same first letter, using the *reclink* similarity index based on a pair of names and a pair of initial words to generate scores, and flagging only those with a similarity score above 0.92 as initial candidates. We first look for matches between the FPDS establishment name and LDBE establishment name, then between the FPDS establishment name and the LDBE doing-business-as name, then between the FPDS and LDBE doing-business-as names, then between the LDBE establishment name and FPDS doing-business-as name, and finally between the FPDS parent name and the LDBE establishment name. We then extensively filter flagged matches, accepting them only if they have extremely high match scores

[29]The states for which we do not have access are Florida, Kentucky, Massachusetts, Mississippi, New York, North Carolina, Rhode Island and Vermont.

(above 0.995) or very small name distances relative to name length (i.e. only minor misspellings), and eliminating name matches based wholly on short initial words or frequently occurring words or names (unless these are unique within the region in both datasets). Establishments still unmatched within the county are then searched for matches within the CBSA; those few unmatched within CBSA are matched within the state. Blocking on initial letter and geography, removing matched establishments at each iteration of the geography loop, and parallelizing over states in multiple Stata instances makes it computationally feasible to match the large set of contract winners against the even larger set of establishments on the BLS's high-performance cluster.

Our matching algorithm is able to match almost all defense contractors to LDBE firms; matched contractors account for more than 90% of total defense spending for each state included in the analysis, and typically more than 95%. Confidentiality of the LDBE data prevents us from publishing a sample of name correspondences, but the filtering algorithm was extensively and iteratively refined to eliminate false matches. We assign a LDBE firm contractor status if they have been matched to an FPDS contractor establishment in this process, allowing us to aggregate up to MSA-year time series of defense contractors' and non-contractors' employment. We can then break down the left-hand side of Equation (1) into two components:

$$\frac{E_{\ell,t+h} - E_{\ell,t-1}}{E_{\ell,t-1}} = \frac{E^{\text{Contractors}}_{\ell,t+h} - E^{\text{Contractors}}_{\ell,t-1}}{E_{\ell,t-1}} + \frac{E^{\text{Non-Contractors}}_{\ell,t+h} - E^{\text{Non-Contractors}}_{\ell,t-1}}{E_{\ell,t-1}}.$$

Therefore, we re-estimate Equation (1) using the restricted QCEW sample and then we break down the employment multiplier in two components by estimating the following two equations:

$$\frac{E^{\text{Contractors}}_{\ell,t+h} - E^{\text{Contractors}}_{\ell,t-1}}{E_{\ell,t-1}} = \beta^{\text{c}}_h \cdot \frac{G_{\ell,t+h} - G_{\ell,t-1}}{Y_{\ell,t-1}} + \lambda^{\text{c}}_{t,h} + \alpha^{\text{c}}_{\ell,h} + u^{\text{c}}_{\ell,t,h} \tag{5}$$

$$\frac{E^{\text{Non-Contractors}}_{\ell,t+h} - E^{\text{Non-Contractors}}_{\ell,t-1}}{E_{\ell,t-1}} = \beta^{\text{nc}}_h \cdot \frac{G_{\ell,t+h} - G_{\ell,t-1}}{Y_{\ell,t-1}} + \lambda^{\text{nc}}_{t,h} + \alpha^{\text{nc}}_{\ell,h} + u^{\text{nc}}_{\ell,t,h}. \tag{6}$$

Thanks to the linearity of the 2SLS estimator and the fact that the right hand side of Equations (1), (5) and (6) are the same, the 2SLS estimates of β^c_h and β^{nc}_h from (5) and (6) respectively add up to

TABLE 8 — EMPLOYMENT MULTIPLIER: CONTRACTOR VS. NON-CONTRACTOR BREAKDOWN

	Contractors			*Non Contractors*		
Horizon	*Coefficient* (β_h^c)	*p*	*Fraction*	*Coefficient* (β_h^{nc})	*p*	*Fraction*
impact	0.040	0.053	157.6%	-0.015	0.387	-57.6%
	(0.021)			(0.017)		
1 year	0.055	0.055	57.8%	0.040	0.010	42.2%
	(0.029)			(0.016)		
2 years	0.048	0.087	48.1%	0.052	0.054	51.9%
	(0.028)			(0.027)		
3 years	0.049	0.119	43.6%	0.064	0.060	56.4%
	(0.031)			(0.034)		

Notes: Sample: 2006–2019; 254 MSAs (QCEW+BDS+LAUS+LDBE Harmonized Dataset). Estimates of Equations (5) and (6). Fraction is calculated as β_h^c/β_h for contractors and β_h^{nc}/β_h from non-contractors, using the values of β_h from the left panel of Table 7 (values reported in green).

the 2SLS estimate of β_h:

$$\underbrace{\hat{\beta}_h^{2SLS}}_{\text{Regional Multiplier}} = \underbrace{\hat{\beta}_h^{c,2SLS}}_{\text{Effect on Contractors}} + \underbrace{\hat{\beta}_h^{nc,2SLS}}_{\text{Effect on Non-Contractors}} .$$

In particular, the 2SLS estimate of β_h^c represents the direct effect of defense contracts on the sphere of defense contractors–i.e., firms that won at least one defense contract over the sample–while the estimate of β_h^{nc} represents the broader indirect effect of defense contracts on firms that never won a contract, e.g., effect on local businesses such as restaurants and shops. Therefore, the estimate of β_h^{nc} provides a direct measure of the strength of the indirect multiplier effect of demand shocks on employment and constitutes a primary contribution of this paper to the literature.

We then estimate equations (5) and (6) using 2SLS. Table 8 reports the estimated coefficients, shown in green, β_h^c for contractors and β_h^{nc} for non-contractors. As noted above, these coefficients sum to the estimates shown in green in Table 7, yielding an exact decomposition of the employment multiplier.

From Table 7 we know that the estimated *impact* employment multiplier is 0.026 and is statistically indistinguishable from zero. This near-zero net effect reflects a mild crowding-out of non-contractor employment (-0.015) that offsets the positive and significant employment response of contractors (0.040). Within a year of the shock, contractors ramp up production but hire mainly

from other firms, reallocating workers and yielding a near-zero net effect on total employment.[30]

One year after the shock, both contractor and non-contractor employment expand significantly, with nearly 60% of the response coming from contractors. However, the cumulative non-contractor response (impact plus one year) does not overturn the initial crowding-out: it remains statistically indistinguishable from zero. Two years after the shock, both components are positive and significant, with the effect split roughly 50–50 between the two groups. By three years after the shock, the non-contractor response exceeds the contractor response.

Overall, the indirect employment effects of defense procurement within MSAs are initially modest and may even be negative in the short run (crowding-out). Over time, however, within-region spillover effects emerge and grow in relative importance.

VI. Breaking Down the Employment Multiplier into the Direct and Indirect Effects within Contractors

In this last section of the paper, we examine the *direct effect* of contracts on establishment outcomes. We do so for two reasons. First, zooming in on the establishment level allows us to observe directly whether winning establishments hire additional workers and to assess the persistence of these effects relative to the average contract duration. Second, the contractors' employment response to a regional shock (Table 8) does not isolate the direct effect on award recipients, because the contractor outcome aggregates all establishments that ever received a contract in our sample; it therefore also reflects the response of establishments without a contemporaneous award at the time of a regional shock. Accordingly, we use administrative data on establishment employment to provide an approximate measure of the direct effect of contracts on establishments.

Data

Establishment-level Outcomes As anticipated in Section V, we leverage restricted data access to the Longitudinal Database of Establishments (LDBE), compiled by the BLS. The data source is

[30] In Appendix A.3, we replicate the results using the larger, non-harmonized LDBE sample with 262 MSAs, where the evidence of crowding-out is stronger in both economic magnitude and statistical significance.

the QCEW, which collects data quarterly from Unemployment Insurance Tax agencies in all states. This dataset provides comprehensive monthly employment and quarterly wages at the establishment level. It also includes information on the establishment's name, location (state, county, and town), and primary industry (six-digit NAICS).

We identify firms in LDBE using the Employer Identification Number (EIN). In the 42 states analyzed, 96% of firms (EIN) have only a single establishment within a state, while about 20% of firms have more than one establishment in another state. We focus on firms with a single establishment within a state; hence, we will refer to the unit of analysis as *establishment*. This simplification does not exclude large multi-establishment contractors from the sample, such as Lockheed Martin, which reports different EINs for its different establishments/subsidiaries in different states.

Procurement Contracts We use FPDS to obtain the universe of all federal procurement contracts awarded from 2006 to 2019, at a daily frequency. We focus on establishments that have won at least one unanticipated contract during this period.

We identify approximately 80,000 unique establishments, identified using Dun & Bradstreet's data universal numbering system (DUNS).[31] Large contractors, such as Lockheed, report different DUNS numbers for each specific subsidiary, resulting in DUNS being location-specific. For example, 97.5% of all defense contractors in the FPDS dataset are located in only one MSA, and 99.7% are located in no more than two MSAs.[32] We aggregate all contracts at the level of recipient establishments (identified by DUNS number) by quarter, resulting in an unbalanced panel dataset. We break down contracts into two components.

Matching Establishments with Contractors We merge contracts from FPDS with establishments' outcomes from LDBE using a fuzzy string-matching algorithm, described above in Section V. We successfully matched 13,000 establishments that have received at least one unanticipated contract between 2006 and 2019. Additionally, we eliminate establishments that (i) have gaps in

[31] DUNS identifiers actually correspond to an even more granular unit than establishments as they represent a "*line of business*". In principle, one establishment may have multiple DUNS; in practice, most establishments correspond to a single DUNS.

[32] This statistic is constructed using all recipient DUNS numbers that received at least one contract from the Department of Defense from 2006 to 2019.

their time series, (ii) receive their only contract shock in their first four observed quarters, so we cannot control for four lags of employment, (iii) receive their only shock in their last two observed years, so we cannot estimate the full impulse response function for that establishment, and (iv) have fewer than one employee on average. We are left with 5,317 establishments with clean and complete histories. To avoid our estimates to be biased by the extremely long right-tail of the establishment employment size distribution, we cap the employment of establishments at 150 employees. This leaves us with 5,142 establishments in the analysis sample.[33] Our establishment-level results are robust to splitting the sample into quartiles of the employment-size distribution; thus, they are not driven by the 150-employee cutoff (see Appendix C.4). Although each establishment in our sample has fewer than 150 employees, some belong to medium or large firms because firm size categories used in the BDS categorize the sum of employment across all establishments within a firm.[34]

Establishment-Level Methodology and Identification

Most government contracts cannot be treated as quasi-random shocks at the establishment level. As discussed in Section II, the majority of procurement spending takes place in the context of long-term agreements (e.g., IDVs) whose timing may be anticipated well in advance by the awardee. Similarly, many awardees are selectively chosen by contracting officers using non-competitive acquisition procedures. Furthermore, many contracts in the FPDS are merely modifications of existing agreements rather than new orders (see Figure 2 in Section II). Thus, estimates obtained from regressing establishment outcomes on any government contract would suffer from two forms of bias: *selection bias*, i.e., the establishments winning awards may be positively selected and thus display higher growth; and *anticipation*, i.e., when the awards are anticipated, they affect the establishment prior to the award date, so the impulse response function does not fully capture the impact of the award.

Therefore, total government contracts $G_{i,t}$, awarded in quarter t to establishment i, can be di-

[33] Details on our sample of establishments are discussed in Appendix C.2.

[34] As discussed in Section IV, BDS classifies firms with 20–499 employees across all establishments as medium-sized and those with 500 or more employees as large.

vided into two components:

$$G_{i,t} = \tilde{G}_{i,t} + \varepsilon^g_{i,t}$$

where $\tilde{G}_{i,t}$ refers to *potentially anticipated contracts*, that is, contracts which are potentially subject to the problem of foresight, and the residual component, $\varepsilon^g_{i,t}$, refers to a demand shock, an *unanticipated contract*, which is not subject to the problems mentioned above.

We use panel local projections to estimate the effect of \$1 of unanticipated contracts on employment (Jordà, 2005). In particular, we estimate via OLS the following baseline equation:

$$E_{i,t+h} - E_{i,t-1} = \beta^h \cdot \varepsilon^g_{i,t} + \gamma^h_0 \cdot \tilde{G}_{i,t} + \text{Lags} + \underbrace{\alpha^h_i + \alpha^h_{s,t} + \alpha^h_{\ell,t}}_{\text{Fixed Effects}} + \upsilon_{i,t+h} \quad h = 0, 1, ..., H, \tag{7}$$

where $E_{i,t+h}$ denotes the h-period ahead number of employees; $\varepsilon^g_{i,t}$ denotes the dollar value of unanticipated contracts awarded to establishment i in quarter t, while $\tilde{G}_{i,t}$ indicates the dollar value of potentially anticipated contracts.[35] "*Lags*" refers to lags of shocks and outcomes.[36] α^h_i represents an establishment fixed effect, $\alpha^h_{s,t}$ is a sector-time fixed effect intended to absorb any sectoral business-cycle effects.[37] Lastly, $\alpha^h_{\ell,t}$ represents a state-time fixed effect, capturing regional business-cycle effects within a state. Our sample is composed of 5,142 establishments between 2006 and 2019.

Identification In light of the considerations in Section II, we define as unanticipated ($\varepsilon^g_{i,t}$) a specific subset of contracts which meet four conditions: (i) newly awarded (not modifications of existing agreements), (ii) standalone contracts, i.e., definitive contracts (not part of an ongoing series of purchases), (iii) competed, and (iv) have at least two bidders.[38] Conditions (iii) and (iv) are similar to those imposed by Hebous and Zimmermann (2020) while condition (i) was introduced in

[35] Both are expressed in units of \$1,000,000 of 2008 dollars.

[36] In particular, Lags $:= \sum_{j=1}^{4} \left\{ \rho^h_j \cdot \varepsilon^g_{i,t-j} + \gamma^h_j \cdot \tilde{G}_{i,t-j} + \phi^h_j \cdot (E_{i,t-j} - E_{i,t-1-j}) \right\}$.

[37] For example, if a sector is experiencing growth in a particular year due to breakthroughs, and contractors are winning more federal contracts as a result, the significance of β^h could erroneously attribute this growth to procurement effects rather than the underlying sectoral boom.

[38] Contracting officers have indicated that even a single-offer scenario, if open to full competition, is treated as competitive, since it potentially pressures the bidder to refine their proposal in anticipation of additional bids. However, they have also communicated that the number of bids is a good indicator of competitiveness, bolstering our confidence in the unanticipated nature of competed definitive contract awards with multiple bidders.

Budrys (2022), both in the context of publicly traded Compustat companies. Condition (ii) imposes a novel additional restriction.

Only 5% of total federal spending is then classified as unanticipated by meeting conditions (i) through (iv). This underscores the need to employ an instrument when analyzing regional effects of procurement spending, as is commonly done in the fiscal policy literature.

The median unanticipated contract is for $114,900 of goods or services. The top categories of services purchased via unanticipated contracts are construction services and defense R&D, while the top categories of goods are food products and manufacturing goods related to defense hardware. The median duration of an unanticipated contract for a service is 283 days, while the median duration of an unanticipated contract for the purchase of goods is much shorter: 79 days.[39]

In the next paragraphs, we further clarify how our specification and contract definition allow us to address concerns about selection bias and foresight.

Selection Bias In the context of federal purchases, Nekarda and Ramey (2011) highlight that industry technological progress can endogenously drive medium-term changes in industry-level government purchases (Perotti, 2007), i.e. there may be reverse causality in the relationship between government contracts and firm innovation and growth. Indeed, government purchases driven by technological progress not only occur frequently, but they are specifically regulated by FAR: sole-source acquisition procedures (FAR 6.302-1-a).

Our empirical strategy addresses this concern in two ways. First, conditions (iii) and (iv) ensure that unanticipated contracts are unlikely to be awarded because an establishment introduced a new, innovative product; such acquisitions fall into the non-competed category and are therefore excluded. Second, by including establishment fixed effects, we purge time-invariant differences in productivity or efficiency across contractors, so our estimates are not mechanically driven by establishments that systematically win more contracts and exhibit higher employment growth.

Foresight Establishments might anticipate the effects of contracts for two reasons: first, the contract is not a new one or is part of an existing ongoing relationship with the government; second,

[39] More descriptive statistics of unanticipated contracts are available in Appendix C.3.

establishments learn about a contract opportunity well ahead of the award notice and anticipate winning the contract.[40]

First, we address concerns about foresight by focusing solely on the effects of new standalone contracts that have been highly competed. Second, we verify that the median number of days between when firms learn about the existence of a contract opportunity (i.e., pre-solicitation) and the award notice is just 20 days. Therefore, any potential anticipation behavior is negligible at a quarterly frequency, which is what we use in our establishment-level analysis (see Appendix C.1). Third, we argue that our identified unanticipated contracts, $\varepsilon^g_{i,t}$, behave as one-time demand shocks: total government contracts jump on impact one to one with the size of the contract, there is no anticipation, and there is no persistence. To show this, we estimate the following equation by OLS:

$$G_{i,t+h} = \psi^h \cdot \varepsilon^g_{i,t} + \pi^h_0 \cdot \tilde{G}_{i,t} + \text{Lags} + \lambda^h_i + \lambda^h_{s,t} + \lambda^h_{\ell,t} + u_{i,t+h}, \qquad h = -8, ..., -1, 0, 1, ..., 8,$$

which coincides with our baseline Equation (8) but omits lags of employment growth.[41] We plot the OLS estimates of ψ^h in Figure 5 for both positive and negative horizons.

On impact, the estimated coefficient jumps to 1, indicating that all contemporaneous variation in contracts originates from the unanticipated component, because we control for contemporaneous values of potentially anticipated contracts. Contracts also show little persistence: serial correlation is absorbed by lags of contracts. Finally, anticipation horizons beyond four-quarter leads ($h < -4$) do not predict subsequent contract awards; additional contracts (anticipated or unanticipated) are not awarded in response to unanticipated awards in preceding quarters.

Therefore, unanticipated contracts in our specification behave as quasi-random, one-time demand shocks that can be used to study the causal effects of government purchases at the establishment level.

[40] Hebous and Zimmermann (2020) and Budrys (2022) also highlight the potential problem of contract anticipation by large *public* firms and show that future competed contract awards and/or award notices do not cause any effect on the current stock prices of the future awardee, mitigating this concern.

[41] In particular, here we have: $\text{Lags} := \sum_{j=1}^{4} \left\{ \rho^h_j \cdot \varepsilon^g_{i,t-j} + \gamma^h_j \cdot \tilde{G}_{i,t-j} \right\}$.

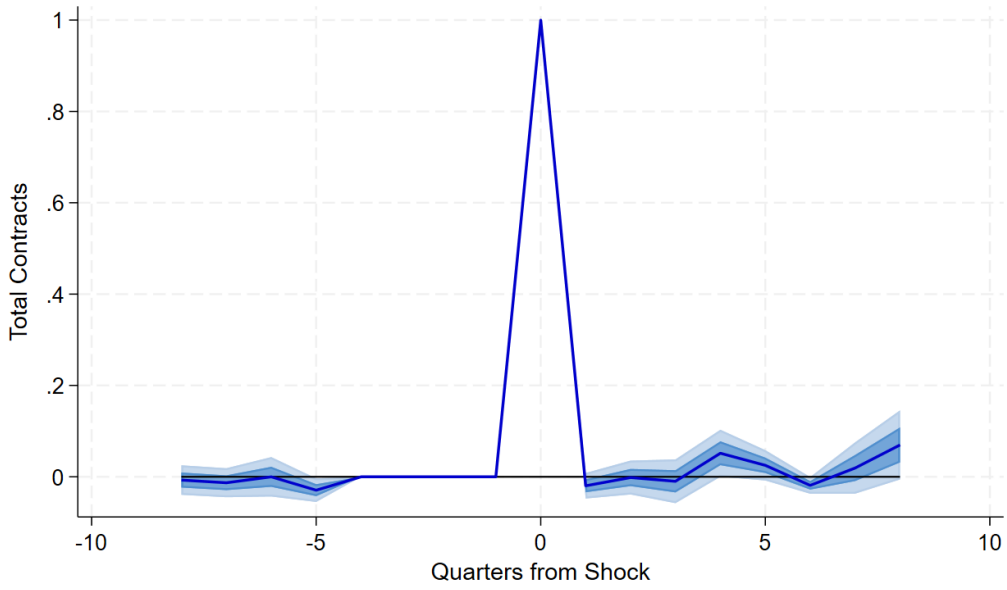


FIGURE 5 — UNANTICIPATED CONTRACTS BEHAVE AS ONE-TIME (DEMAND) SHOCKS

Representativeness Readers may worry that the effects of unanticipated contracts could differ materially from those of the regional level defense contracts studied above because the two may target different types of establishments. We address this concern on three grounds.

First, establishments that receive at least one unanticipated contract account for a large share (about 80%) of aggregate federal procurement value over our sample. Thus, recipients of unanticipated contracts are not a highly selected subgroup but are broadly representative of the universe of federal contractors, mitigating external-validity concerns. In other words, the contractors analyzed here plausibly overlap with those receiving awards during regional shocks to regional defense spending, as studied in the previous sections.

Second, we include unanticipated contracts awarded by any federal agency in our analysis. Any differences in employment effects between defense and non-defense purchases are likely driven by sectoral composition rather than by the purchasing agency. We therefore include all federal unanticipated contracts to maximize sample size and precision. Notwithstanding, about two-thirds of unanticipated contracts in our sample originate from the Department of Defense.

Lastly, we rule out the possibility that unanticipated contracts are awarded exclusively to establishments belonging to very small firms, even though most of the effects of regional shocks appear

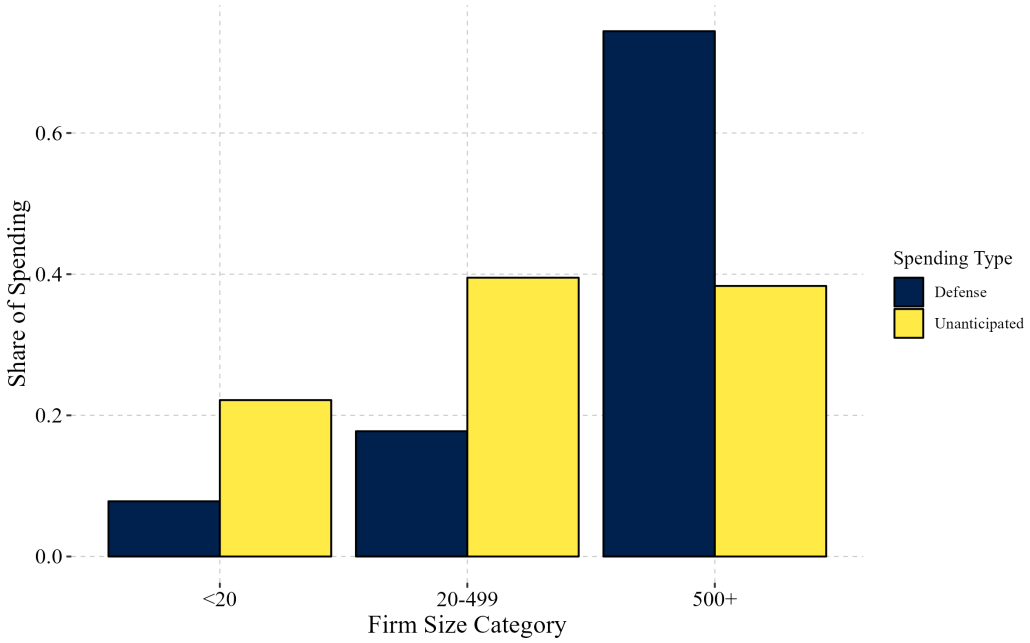


FIGURE 6 — SIZE DISTRIBUTION BY SPENDING TYPE

Notes: Firm sizes are assigned by matching FPDS DUNS to NETS and cumulating employment of establishments to top-level headquarters of different establishments, as described in Section IV. Size bins correspond to Business Dynamics Statistics categories. Firm sizes are measured in the quarter of a contract award.

to be driven by establishments of large firms (Table 5) and, to a lesser extent, by establishments of medium firms (Table A11). Figure 6 shows that, although the firm-size distributions for unanticipated versus general defense contracts differ somewhat, awards are spread across the firm-size distribution and are not disproportionately tilted toward small firms.

Taken together, these points lead us to view the contractors in our establishment-level analysis as broadly representative and plausibly the same establishments that would receive awards during a regional shock.

Empirical Results

We estimate Equation (8) via OLS for each horizon h from 0 (impact) to 8 (two years). The OLS estimates of β^h can be interpreted as impulse response functions (IRF) of the effect of an extra dollar of spending on establishment-level employment. The middle panel of Figure 7 shows the results.

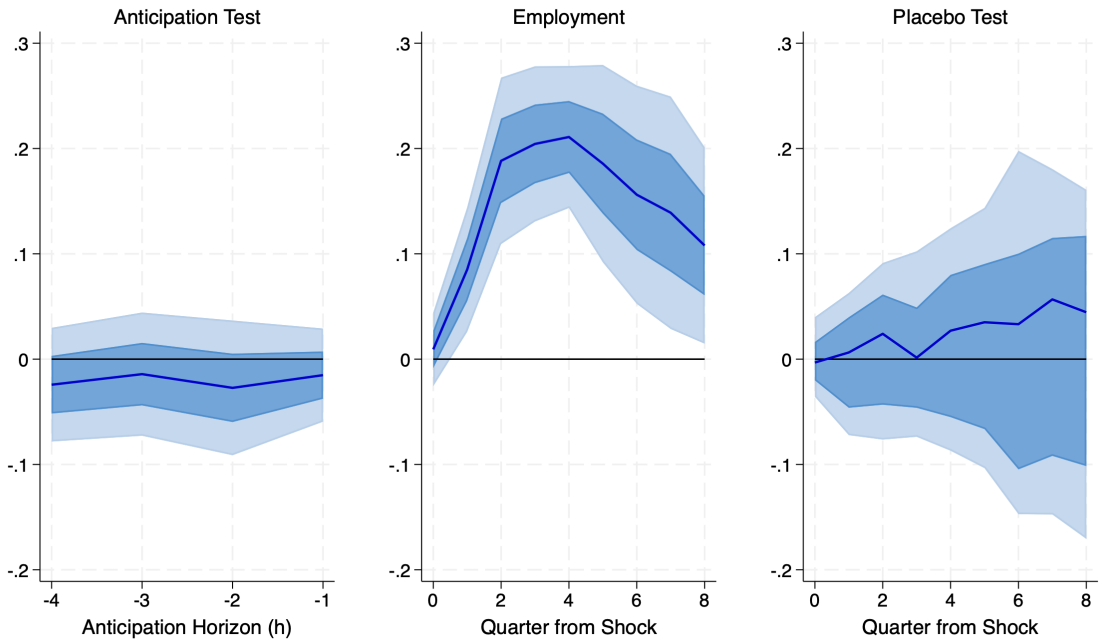


FIGURE 7 — EFFECT OF UNANTICIPATED CONTRACTS ON ESTABLISHMENTS' GROWTH

Notes: Sample: 2006:1 to 2019:4 ($T = 56$) – number of establishments is $N = 5,142$. Robust standard errors are clustered at the state level. Small bands represent 68% confidence intervals, and large bands represent 95% confidence intervals.

The middle panel displays a significant and positive effect of contracts on establishments. The maximum response to changes in employment from the shock occurs at horizon 4, one year after the shock. As part of our matched establishments are part of large contractors, this dynamic response is consistent with the dynamic response of employment at the regional level, where contracts appear to have limited effects within one year after the shock.

Moreover, notice that the effects of an unanticipated contract are quite persistent and appear to survive even after eight quarters after the shock. Since 75% of unanticipated contracts for services have a duration shorter than five quarters, while 75% of unanticipated contracts for goods have a duration shorter than three quarters (see Appendix C.3), the effects of contracts appear to be very persistent and survive even after the termination of the contract. The persistence of the effects of procurement contracts on employment is consistent with previous findings in the literature such as Ferraz, Finan, and Szerman (2021) for Brazil, Lee (2024) for South Korea, and Gabriel (2024) for Portugal.

Nevertheless, we find a small quantitative response. In fact, the peak effect at horizon four is 0.21, which means that after one year, \$1M of contract generates 0.21 more jobs, on average. Following Chodorow-Reich (2019), we calculate the number of job-years by cumulating the impulse response function and then dividing by four, since our data are measured at the quarterly frequency.[42] We obtain a value of 0.322 job-years per million dollars over the first two years, which corresponds to a cost-per-job (with average duration of one year) of \$3,110,000. At the end of the section, we provide a comparison to the regional level estimates.

To complement these employment effects, in Appendix C.6, we examine the response of total and average wages at the establishment level. While total wages increase significantly following the receipt of an unanticipated contract, consistent with the rise in employment, the average wage per worker does not respond. This suggests that the adjustment occurs primarily along the extensive margin rather than the intensive margin. In other words, establishments expand headcount but do not significantly alter compensation for existing workers. This finding complements our earlier result that most job creation occurs outside the specific establishment receiving the contract, highlighting that the direct effects of procurement on labor costs are driven by broader organizational growth rather than wage renegotiation. It is also consistent with prior evidence from procurement cuts under the Budget Control Act, where employment, not wages, absorbed most of the shock (Komarek, Butts, and Wagner, 2022).

Anticipation Test We carry out an anticipation test to rule out the possibility that our shocks are anticipated.

In particular, we re-estimate Equation (8) for $h = 0$, while shifting forward the shocks $\varepsilon_{i,t}$ by either one, two, three, or four periods, to empirically test the presence of anticipatory behavior. This approach is consistent with Nekarda and Ramey (2011) test of whether leads of sectoral government purchases affect current outcomes. We thus estimate:

$$Y_{i,t} - Y_{i,t-1} = \beta^h \cdot \underbrace{\varepsilon^G_{i,t+h}}_{\text{Future Shock}} + \text{Same Controls as in Baseline} + \upsilon_{i,t} \quad h = 1, 2, 3, 4., \tag{8}$$

[42] See page 16, section IV.A in Chodorow-Reich (2019).

In particular, we are interested in the effect of future shocks $\varepsilon_{i,t+\tau}$, with $\tau = 1, \ldots, 4$, on current employment changes: $Y_{i,t} - Y_{i,t-1}$. If shocks were anticipated, we would expect to see a significant effect of future shocks on current employment growth, with a magnitude similar to that observed for current shocks (middle panel). We report the OLS estimates of the effect of $\varepsilon_{i,t+\tau}$ on current changes in employment for $\tau = 1, \ldots, 4$ in the left panel of Figure 7.

Notice that future shocks have neither a meaningful nor a statistically significant effect on current employment changes at any point in the anticipation horizon. The result of this test rules out the possibility that contracts that we have classified as "unanticipated" are in fact anticipated by establishments.

Placebo We then carry out a placebo test to rule out the possibility that our specification is picking up some unknown source of spurious correlation. We do so by reshuffling the timing of the shocks within each establishment. The new synthetic shock for establishment i in quarter t is denoted by

$$\forall (i,t)\ \hat{\varepsilon}_{i,t} = \varepsilon_{i,\tau} \quad \text{with } \tau \in \{2006{:}1,\ldots,2019{:}4\}.$$

We then re-estimate Equation (8), replacing the original shocks, $\varepsilon_{i,t}$, with the synthetic shocks, $\hat{\varepsilon}_{i,t}$, to carry out a placebo test. If our specification is capturing a spurious correlation instead of a causal effect of contracts on employment, we would expect to see positive and significant results even in response to synthetic shocks. On the contrary, the placebo test is passed only if the synthetic shocks have no significant effect on employment.

The right panel of Figure 7 shows the estimate of β^h using synthetic shocks that do not produce any significant effect on employment growth. This suggests that the original shocks are indeed capturing the causal effect of (unanticipated) contracts on establishments' employment growth.

Breaking Down the Employment Multiplier into Direct and Indirect Effects As discussed above, we estimate 0.322 job-years per $1 million two years after the shock at the establishment level. We now benchmark this estimate against regional-level responses to a regional shock in the harmonized QCEW+BDS+LAUS+LDBE sample, restricting attention to the set of regions that

contain the establishments analyzed here.

In particular, Table 7 reports a response of 0.601 job-years per $1 million on impact (horizon 0). The additional response at horizon 1 (one year later) is 2.235, so the cumulative effect through two years (horizons 0–1) is $0.601 + 2.235 = 2.836$ job-years per $1 million. From Table 8, we infer that the cumulative fraction of the employment response through one year that accrues to contractors is approximately 78%.[43] Therefore, the cumulative number of job-years per $1 million within two years that is attributable to contractors is $0.78 \times 2.836 = 2.212$ job-years per $1 million.

A back-of-the-envelope calculation then implies that direct establishment-level impacts account for approximately 14.5% of the contractors' regional response:

$$\text{Direct Establishment Share of Contractor Response} = \frac{0.322 \frac{\text{job-years}}{\$1\text{M}}}{0.78 \times 2.836 \frac{\text{job-years}}{\$1\text{M}}} = 14.5\%,$$

with the remainder attributable to spillovers within the contractor sphere—most plausibly subcontracting and input-supplier channels. In other words, at least 14.5% of the contractors' response is accounted for by direct effects on winning establishments.

Two caveats are worth bearing in mind. First, because the regional analysis is annual while the establishment analysis is quarterly, we align horizons by using the cumulative regional response through horizon 1 (covering the first two years after the shock), which most closely matches the eight-quarter window in the establishment analysis. Moreover, we re-estimate the establishment-level effects at annual frequency, mirroring the regional specification, using the lower-quality National Establishment Time Series (NETS) data in Appendix C.7, and we obtain similar magnitudes.

Second, because subcontracting is anecdotally a pervasive phenomenon in federal procurement, our estimate is a lower bound on the direct effect: part of each award may be executed by other establishments. The gap between the establishment-level effect and the contractor component of the regional response is consistent with subcontracting and input-supplier channels. Nevertheless, the precisely estimated establishment-level effects indicate that a substantial share

[43] Cumulate the contractors' employment multiplier in Table 8 through horizon 1: $0.040 + 0.055 = 0.095$. Divide by the cumulative total response in Table 7 through horizon 1: $0.026 + 0.096 = 0.122$. The ratio $0.095/0.122 \approx 0.78$ (i.e., 78%).

of contractor hiring occurs within recipient establishments.

VII. Conclusion

This paper examines how defense procurement spending affects employment across U.S. regions and establishments. Although defense contracting is a major channel of federal spending and a central tool in contemporary industrial and fiscal policy, we know little about which firms drive its employment effects or how quickly these effects emerge. Using establishment-level matched contract and employment data, we construct region-level measures of exposure to procurement shocks and break down the resulting employment multiplier by firm size and contractor status. This decomposition reveals the channels through which defense spending influences aggregate job creation while also highlighting the limits of procurement as an effective tool for stimulating short-run employment.

At the regional level, we find that defense procurement shocks lead to modest but sustained employment gains. The three-year employment multiplier is approximately 0.1, meaning that a shock equivalent to 1% of regional wages and salaries results in a 0.1% increase in regional employment. Although economically meaningful, these gains come at a high fiscal cost of about $290,000 per job-year, reflecting the high wages in defense-related industries. This aligns with recent concerns over the cost-effectiveness and regressivity of procurement-driven job creation and highlights the limits of procurement as a rapid employment stimulus.

We further find that the aggregate employment response is heavily driven by large firms. Small and medium-sized enterprises receive a relatively small share of contract dollars and contribute even less to net job creation. Moreover, employment gains primarily occur through the expansion of existing establishments, rather than through new firm entry or business formation. This suggests that procurement reinforces the position of already existing contractors rather than catalyzing broader local growth through firm dynamism.

Using restricted microdata from the Bureau of Labor Statistics, we are able to distinguish employment effects across contracting and non-contracting firms within a region. This breakdown

reveals meaningful dynamics: employment at non-contracting firms initially declines, suggesting short-run crowding out, but recovers by year one. By the second year, non-contractors account for nearly half of the regional employment response, and by the third year, they exceed the contribution of contractors. These patterns indicate that while procurement does not operate effectively as a short-run stimulus, it can support medium-term employment growth for both contractors and non-contractors alike, and thus across industries regardless of their direct linkages to defense spending.

Further leveraging the micro-level data, we isolate the direct effects of contracts on the establishments that receive them. We find that employment gains at these winning establishments account for only about 15% of the total contractors' effect, despite being persistent and extending well beyond the median contract duration. The remaining employment gains are concentrated among other defense contractors, suggesting that procurement stimulates broader firm-level responses through subcontracting networks, supply chain integration, and other indirect linkages. These findings underscore the importance of accounting for intra-industry spillovers and input-output connections when evaluating the employment effects of defense procurement.

These findings carry important implications for policy, especially as many countries expand defense budgets in response to rising geopolitical tensions. While not well-suited as a rapid stimulus tool, defense procurement remains a powerful lever for long-term industrial and regional development. Beyond its employment effects, procurement can support innovation, productivity growth, and the diffusion of technological capabilities in strategic sectors. In our data, regional unemployment falls, labor force participation rises, and non-contractor employment expands one to two years after a procurement shock, which suggests broader macroeconomic benefits that materialize gradually and extend beyond the directly targeted firms.

Policymakers should therefore recognize both the limits and the potential of procurement: its employment effects are costly, concentrated, and slow to diffuse, yet they can play a role in shaping regional economic development.

Appendix

A Extra: Regional Employment Multipliers

A.1. LAUS: Unemployment versus Labor Force Participation

We re-estimate Equation (1) substituting employment with either unemployment or labor-force levels from the BLS Local Area Unemployment Statistics (LAUS). The results are shown in Table A1.

TABLE A1 — BASELINE ESTIMATES WITH LAUS DATA

Employment data from LAUS										
	Unemployment					Labor Force				
Horizon	Coefficient	p	Effective F	Job-Years ($1M)	Cost-per-Job ($)	Coefficient	p	Effective F	Job-Years ($1M)	Cost-per-Job ($)
impact	0.212	0.256	29.232	0.371	$2,696,413	0.029	0.143	29.232	0.834	$1,198,829
	(0.186)			(0.326)	($2,368,343)	(0.020)			(0.568)	($815,970)
1 year	-0.201	0.057	53.120	-0.352	$(2,840,907)	0.075	0.038	53.120	2.165	$461,937
	(0.105)			(0.184)	($1,487,100)	(0.036)			(1.037)	($221,317)
2 years	-0.184	0.040	27.603	-0.322	$(3,103,027)	0.068	0.056	27.603	1.963	$509,388
	(0.089)			(0.156)	($1,504,555)	(0.036)			(1.026)	($266,241)
3 years	-0.195	0.078	21.063	-0.342	$(2,925,322)	0.073	0.077	21.063	2.118	$472,238
	(0.111)			(0.194)	($1,656,749)	(0.041)			(1.192)	($265,880)

Notes. Sample: 2001-2019 - 358 MSAs (Harmonized QCEW+BDS+LAUS Sample). Cost-per-Job is constructed as before, replacing employment with unemployment or labor-force statistics. All other details match Table 2.

The left panel of the table shows that unemployment and labor force coefficients are statistically significant after the impact, but insignificant on impact, mirroring the dynamics of the baseline employment results reported in Table 2. Specifically, the left panel shows negative changes in the number of unemployed individuals, indicating that regional shocks to defense purchases reduce unemployment, while the right panel documents a significant and growing increase in the labor force.

To gauge magnitudes, we express the multipliers in job-years and cost-per-job, following the same procedure used for employment but replacing employment with unemployment or labor-force levels in the conversion factor. The corresponding figures appear in the *Job-Years* and *Cost-per-Job* columns of Table A1. Three years after the regional shock, it costs $472,000 (2008 dollars) to add one person to the labor force, while it is much more expensive to reduce the number of unemployed. The positive employment multiplier originates primarily from new entrants into the

regional labor force. Notice that the overall magnitudes appear smaller than those reported using high-quality administrative data on employment, which may reflect difficulties in timely tracking of changes in unemployment statistics in the LAUS methodologies.

A.2. Effects on Number of Firms

In principle, a regional shock could increase the number of firms in a region if it represents a sufficiently large demand shock. Consequently, the positive employment multiplier might arise from the creation of new businesses (the *extensive margin*) rather than from the expansion of the workforce within existing businesses (the *intensive margin*).

To investigate this possibility, we use data from the Business Dynamics Statistics (BDS), which report the number of firms in each MSA–year pair. We re-estimate Equation (1), replacing employment with the firm count, to assess whether the extensive margin contributes to the positive employment multiplier. The results are presented in Table A2.

TABLE A2 — EXTENSIVE MARGIN: NUMBER OF FIRMS

Number of Firms from BDS					
Horizon	*Coefficient*	*p*	*Effective F*	*Firms Year ($1M)*	*Cost per Firm ($)*
impact	-0.005	0.756	29.232	-0.006	$(170,298,832)
	(0.017)			(0.019)	($546,542,528)
1 year	0.023	0.382	53.120	0.025	$40,784,724
	(0.026)			(0.028)	($46,605,776)
2 years	0.024	0.363	27.603	0.026	$37,820,644
	(0.027)			(0.029)	($41,520,636)
3 years	0.029	0.318	21.063	0.032	$31,613,240
	(0.029)			(0.032)	($31,616,600)

Notes: Sample: 2001-2019 - 358 MSAs (Harmonized QCEW+BDS+LAUS Sample). Cost per firm-year is constructed as before, replacing employment with the firm count. All other details match Table 2. No cost-per-firm year is calculated for negative point estimates.

We find no meaningful effect of the regional shock on the number of firms and we conclude that additional defense spending does not generate significant job creation through the establishment of new firms: the extensive margin is negligible. Instead, the positive employment multiplier appears to originate from existing firms hiring additional workers from the labor force and the pool of unemployed.

A.3. Robustness using Largest Available Samples

TABLE A3 — REGIONAL EMPLOYMENT MULTIPLIERS FROM BEA DATA

BEA Total Employment - Largest Sample: 2001–2019; 380 MSAs					
Horizon	*Coefficient*	*p*	*Effective F*	*Job-Years ($1M)*	*Cost-per-Job ($)*
impact	0.020	0.004	13.643	0.655	$1,527,213
	(0.007)			(0.227)	($529,936)
1 year	0.095	0.000	93.200	3.136	$318,887
	(0.025)			(0.816)	($82,970)
2 years	0.081	0.002	52.473	2.665	$375,188
	(0.026)			(0.851)	($119,836)
3 years	0.114	0.018	10.553	3.750	$266,650
	(0.048)			(1.577)	($112,133)

Notes: Robustness of Table 2 (baseline regional employment multipliers). Largest available sample from LDBE.

TABLE A4 — REGIONAL EMPLOYMENT MULTIPLIERS FROM LDBE DATA

LDBE Employment - Largest Sample: 2006–2019; 262 MSAs						
	Employment from LDBE (Restricted QCEW from BLS)					Employment from Public BEA
Horizon	*Coefficient*	*p*	*Effective F*	*Job-Years/$1M*	*Cost-per-Job ($)*	*Job-Years/$1M*
impact	0.012	0.557	10.836	0.275	$3,634,722	0.181
	(0.020)			(0.468)	($6,183,501)	(0.728)
1 year	0.098	0.003	10.554	2.282	$438,176	2.693
	(0.033)			(0.773)	($148,471)	(1.181)
2 years	0.100	0.019	7.125	2.348	$425,853	2.745
	(0.042)			(0.994)	($180,200)	(1.391)
3 years	0.117	0.037	5.466	2.727	$366,739	3.134
	(0.056)			(1.300)	($174,830)	(1.732)

Notes: Robustness of Table 7 (regional employment multipliers using restricted micro-data aggregated to MSAs). Largest available sample from LDBE.

TABLE A5 — MULTIPLIER BREAKDOWN: CONTRACTORS VS NON-CONTRACTORS FROM LDBE

LDBE Employment - Largest Sample: 2006–2019; 262 MSAs						
	Contractors			*Non Contractors*		
Horizon	*Coefficient*	*p*	*Fraction*	*Coefficient*	*p*	*Fraction*
impact	0.036	0.040	306.8%	-0.024	0.205	-206.8%
	(0.017)			(0.019)		
1 year	0.063	0.032	64.4%	0.035	0.028	35.6%
	(0.029)			(0.016)		
2 years	0.052	0.044	51.4%	0.049	0.034	48.6%
	(0.026)			(0.023)		
3 years	0.056	0.074	47.8%	0.061	0.037	52.2%
	(0.031)			(0.029)		

Notes. Robustness of Table 8 (breakdown of regional employment multipliers using restricted micro-data aggregated to MSAs). Largest available sample from LDBE.

TABLE A6 — REGIONAL EMPLOYMENT MULTIPLIERS FROM BDS DATA

BDS PRIVATE EMPLOYMENT - LARGEST SAMPLE: 2001-2019; 373 MSAS					
Horizon	*Coefficient*	*p*	*Effective F*	*Job-Years ($1M)*	*Cost-per-Job ($)*
impact	0.040	0.074	9.843	0.837	$1,194,908
	(0.022)			(0.467)	($667,073)
1 year	0.100	0.003	68.547	2.085	$479,521
	(0.033)			(0.693)	($159,427)
2 years	0.110	0.005	32.324	2.289	$436,902
	(0.039)			(0.808)	($154,168)
3 years	0.134	0.025	9.325	2.775	$360,299
	(0.059)			(1.233)	($160,050)

Notes: Robustness of Table 4 (regional employment multipliers using BDS data). Largest available sample from BDS.

TABLE A7 — MULTIPLIER BREAKDOWN: SMALL VS MEDIUM VS LARGE FROM BDS DATA

Breakdown by Size - Largest Sample: 2001-2019; 373 MSAs									
	Small Firms			*Medium Firms*			*Large Firms*		
Horizon	*Coefficient*	*p*	*Fraction (%)*	*Coefficient*	*p*	*Fraction (%)*	*Coefficient*	*p*	*Fraction (%)*
impact	-0.001	0.821	-2.1%	-0.003	0.717	-6.6%	0.044	0.035	108.8%
	(0.004)			(0.007)			(0.021)		
1 year	0.005	0.325	4.9%	0.001	0.908	1.3%	0.094	0.004	93.7%
	(0.005)			(0.012)			(0.032)		
2 years	0.004	0.439	3.7%	0.009	0.368	7.9%	0.097	0.005	88.4%
	(0.005)			(0.010)			(0.034)		
3 years	0.005	0.432	3.5%	0.008	0.473	6.1%	0.121	0.034	90.5%
	(0.006)			(0.011)			(0.057)		

Notes: Robustness of Table 5 (breakdown of regional employment multipliers by firm size using BDS data). Largest available sample from BDS.

TABLE A8 — UNEMPLOYMENT AND LABOR FORCE FROM LAUS DATA

Employment from LAUS - Largest Sample: 2001-2019; 366 MSAs										
	Unemployment					*Labor Force*				
Horizon	*Coefficient*	*p*	*Effective F*	*Job-Years ($1M)*	*Cost-per-Job ($)*	*Coefficient*	*p*	*Effective F*	*Job-Years ($1M)*	*Cost-per-Job ($)*
impact	0.212	0.256	29.232	0.371	$2,696,413	0.029	0.143	29.232	0.834	$1,198,829
	(0.186)			(0.326)	($2,368,343)	(0.020)			(0.568)	($815,970)
1 year	-0.201	0.057	53.120	-0.352	$(2,840,907)	0.075	0.038	53.120	2.165	$461,937
	(0.105)			(0.184)	($1,487,100)	(0.036)			(1.037)	($221,317)
2 years	-0.184	0.040	27.603	-0.322	$(3,103,027)	0.068	0.056	27.603	1.963	$ 509,388
	(0.089)			(0.156)	($1,504,555)	(0.036)			(1.026)	($266,241)
3 years	-0.195	0.078	21.063	-0.342	$(2,925,322)	0.073	0.077	21.063	2.118	$ 472,238
	(0.111)			(0.194)	($1,656,749)	(0.041)			(1.192)	($265,880)

Notes: Robustness of Table A1 (effect on unemployment and labor force). Largest available sample from LAUS.

A.4. Robustness using Harmonized QCEW+BDS+LAUS+LDBE: Smallest Sample

TABLE A9 — REGIONAL EMPLOYMENT MULTIPLIERS FROM BEA

BEA Employment - Smallest Sample: 2006–2019; 254 MSAs					
Horizon	*Coefficient*	*p*	*Effective F*	*Job-Years ($1M)*	*Cost-per-Job ($)*
impact	0.024	0.229	10.019	0.762	$1,311,855
	(0.020)			(0.632)	($1,088,125)
1 year	0.085	0.037	29.845	2.755	$362,987
	(0.041)			(1.318)	($173,608)
2 years	0.083	0.091	7.532	2.670	$374,548
	(0.049)			(1.573)	($220,603)
3 years	0.092	0.111	6.561	2.989	$334,577
	(0.058)			(1.867)	($208,961)

Notes: Robustness of Table 2 (baseline regional employment multipliers). Sample: Harmonized QCEW+BDS+LAUS+LDBE Dataset.

TABLE A10 — REGIONAL EMPLOYMENT MULTIPLIERS FROM BDS

BDS Private Employment - Smallest Sample: 2006–2019; 254 MSAs					
Horizon	*Coefficient*	*p*	*Effective F*	*Job-Years ($1M)*	*Cost-per-Job ($)*
impact	0.046	0.281	10.019	0.921	$1,085,957
	(0.042)			(0.853)	($1,005,478)
1 year	0.074	0.055	29.845	1.500	$666,831
	(0.039)			(0.778)	($346,073)
2 years	0.114	0.006	7.532	2.300	$434,829
	(0.041)			(0.823)	($155,623)
3 years	0.116	0.022	6.561	2.341	$427,197
	(0.051)			(1.018)	($185,696)

Notes: Robustness of Table 4 (regional employment multipliers using BDS data). Sample: Harmonized QCEW+BDS+LAUS+LDBE Dataset.

TABLE A11 — MULTIPLIER BREAKDOWN: SMALL VS MEDIUM VS LARGE

Breakdown by Size - Smallest Sample: 2001-2019; 254 MSAs									
	Small Firms			*Medium Firms*			*Large Firms*		
Horizon	*Coefficient*	*p*	*Fraction (%)*	*Coefficient*	*p*	*Fraction (%)*	*Coefficient*	*p*	*Fraction (%)*
impact	0.003	0.717	6.4%	0.014	0.371	31.4%	0.028	0.460	62.2%
	(0.008)			(0.016)			(0.038)		
1 year	0.007	0.145	9.6%	0.016	0.231	21.9%	0.051	0.162	68.6%
	(0.005)			(0.014)			(0.036)		
2 years	0.004	0.393	3.9%	0.026	0.099	22.4%	0.084	0.014	73.8%
	(0.005)			(0.015)			(0.034)		
3 years	0.005	0.290	4.6%	0.027	0.079	23.3%	0.084	0.050	72.1%
	(0.005)			(0.015)			(0.043)		

Notes: Robustness of Table 5 (breakdown of regional employment multipliers by firm size using BDS data). Harmonized QCEW+BDS+LAUS+LDBE Dataset.

B Extra: Robustness of Baseline Multiplier Estimates

B.1. Extra on Table 2

In this section we carry out several robustness checks to corroborate the validity of our baseline estimates of employment multipliers reported in Table 2.

First Stage. In this section, we report the results of the first stage of our baseline estimation of employment multipliers (Equation (1)). The first-stage regression is specified as follows:

$$\underbrace{\frac{G_{\ell,t+h} - G_{\ell,t-1}}{Y_{\ell,t-1}}}_{\text{RHS of Equation (1)}} = \phi_h \cdot \underbrace{\frac{\exp_\ell \cdot (G_{t+h} - G_{t-1})}{Y_{\ell,t-1}}}_{Z_{\ell,t+h}} + \underbrace{\lambda_{\ell,h} + \alpha_{t,h}}_{\text{FEs}} + e_{\ell,t+h}.$$

Values of the OLS-estimated first-stage coefficients (ϕ_h) are reported in Table B1, along with robust and clustered standard errors. As expected, the instrument is strong, with effective F-statistics exceeding 23, consistent with the baseline results reported in Table 2. Moreover, also as expected, the coefficient on impact is statistically insignificantly different than 1.

Lastly, Figure B1 reports a bin-scatter plot of the residualized first-stage regressions by horizon h. By the Frisch–Waugh–Lovell theorem, the slope of the fitted line coincides with the estimates of ϕ_h reported in Table B1. Given our sample size, each point in the graph represents an average of roughly 21 observations. The strong fit is consistent with the low p-values in Table B1 and the

TABLE B1 — FIRST STAGE COEFFICIENTS OF EQUATION (1)

Horizon	*Coefficient* (ϕ_h)	p
impact	0.981	0.000
	(0.176)	
1 year	1.506	0.000
	(0.200)	
2 years	1.638	0.000
	(0.301)	
3 years	1.887	0.000
	(0.396)	

Notes: Sample: 2001-2019 - 358 MSAs (QCEW-BDS-LAUS Harmonized Dataset). All rest is identical to Table 2.

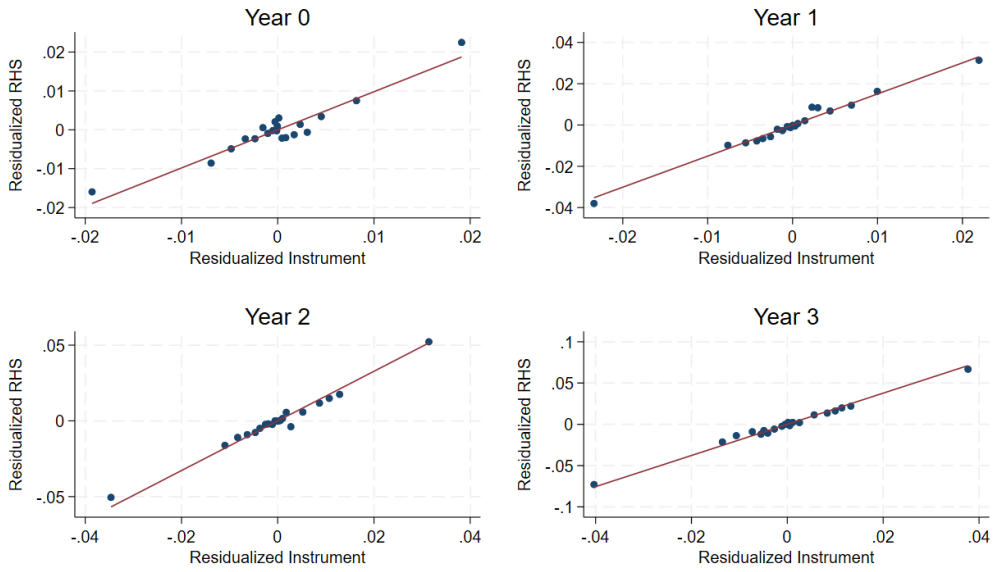


FIGURE B1 — BIN-SCATTER PLOT OF (RESIDUALIZED) FIRST-STAGE REGRESSIONS

relatively high effective F-statistics reported in the baseline results (Table 2).

Instrument Validity. We check whether our baseline instrument is explained by lagged outcomes at any horizon. This exercise can be viewed as a balance test on pre-treatment trends, which provides evidence on the plausibility of the parallel trends assumption. If regions with higher exposure (i.e., high instrument values) were systematically growing faster than average in the past, then identification would be undermined, as the instrument would also capture endogenous placement of contracts rather than exogenous shocks. In that case, it would also be necessary to control for lagged employment growth in the baseline analysis to mitigate this source of endogeneity.

In practice, we estimate via OLS using our baseline harmonized BEA-BDS-LAUS sample the following equation:

$$\underbrace{\frac{\exp_\ell \cdot (G_{t+h} - G_{t-1})}{Y_{\ell,t-1}}}_{Z_{\ell,t+h}} = \rho_h \cdot \underbrace{\frac{E_{\ell,t-1} - E_{\ell,t-2}}{E_{\ell,t-2}}}_{\text{Lagged LHS}} + \underbrace{\omega_{\ell,h} + \pi_{t,h}}_{\text{FEs}} + \upsilon_{\ell,t+h}$$

Results are reported in Table **??**. Notice that lagged employment growth–the horizon 0 variable in our baseline regression, Equation (1)–does not predict values of the instrument at any horizon

TABLE B2 — IS OUR INSTRUMENT EXPLAINED BY LAGGED EMPLOYMENT GROWTH?

LHS: Instrument - RHS: Lagged employment growth		
Horizon	*Coefficient* (ρ_h)	*pvalue*
impact	0.010	0.182
	(0.008)	
1 year	0.010	0.431
	(0.012)	
2 years	0.003	0.879
	(0.018)	
3 years	-0.019	0.397
	(0.023)	

Notes: Sample: 2001-2019 - 358 MSAs (QCEW-BDS-LAUS Harmonized Dataset). All rest is identical to Table 2.

h. This result supports our identification strategy, providing evidence of the absence of pre-trends in regions with high exposure to defense contracts.

Second Stage. Having discussed the validity of our procedure, we now provide information on the second-stage regression. In Figure B2, we report a bin-scatter plot of the residualized variables used in the second stage. Each point in the graph represents an average of 21 observations, and the slope of the fitted line corresponds to the estimated employment multiplier coefficients, β_h, reported in Table 2, by the Frisch–Waugh–Lovell theorem.

The graph suggests that most of the identifying variation originates from the comparison of the 21 regions with high exposure to defense procurement and the rest of the sample. This is consistent with the fact that roughly 21 MSAs are substantially exposed to defense procurement, largely due to their ability to attract contracts through the presence of historical military installations (see Figure 4). Accordingly, our IV panel local projection regression can be interpreted as a generalized difference-in-differences framework with continuous treatment, comparing regions with high versus low exposure to aggregate shocks in defense contracts.

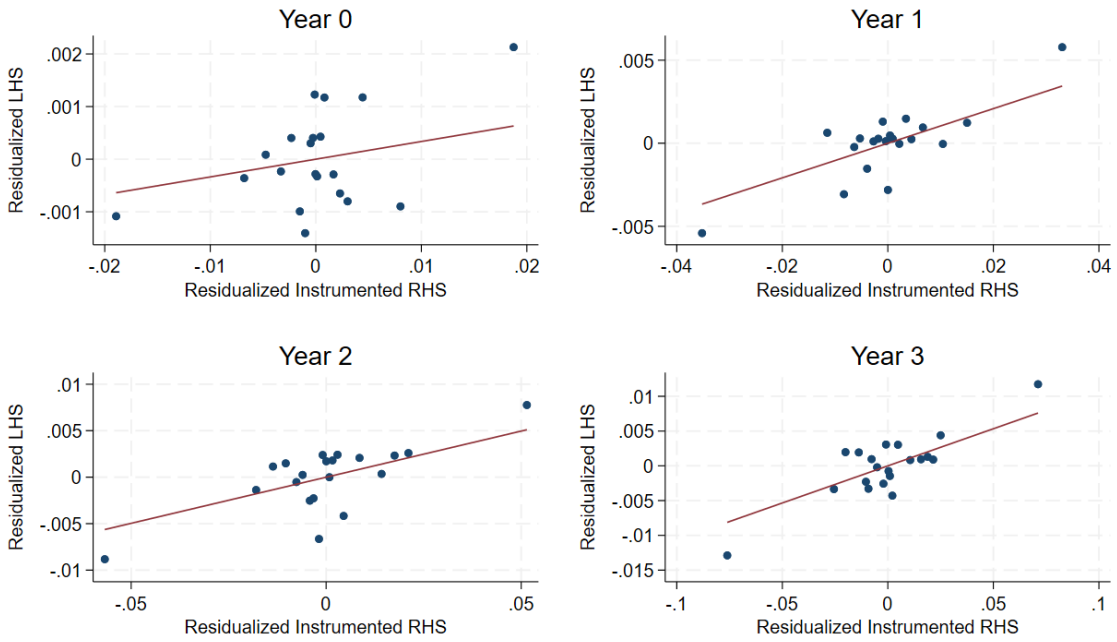


FIGURE B2 — BIN-SCATTER PLOT OF (RESIDUALIZED) SECOND-STAGE REGRESSIONS

B.2. Employment Multiplier Using an Exact Shift-Share Instrument

In this section, we show that our baseline estimates of employment multipliers (Section III) remain unchanged when we replace the original instrument with an exact shift-share instrument.

Shift-Share Interpretation of Regional Government Spending Shocks Following Goldsmith-Pinkham, Sorkin, and Swift (2020) and Borusyak, Hull, and Jaravel (2022), a generic shift-share (Bartik) instrument can be written as:

$$Z_{\ell,t} = \sum_{s=1}^{S} w_{\ell,s} \cdot g_{s,t},$$

where $w_{\ell,s}$ denotes the exposure (or "share") of region ℓ to sector s, and $g_{s,t}$ is a sector-level shock (the "shift").

In our setting, the number of sectors reduces to one ($S = 1$), namely government spending. The instrument then simplifies to:

$$Z_{\ell,t} = w_{\ell} \cdot g_{t},$$

where g_t is the aggregate government spending shock and w_ℓ is a fixed measure of region ℓ's baseline exposure to government spending.

This representation highlights that our design is a special case shift-share instrument: all regions face the same aggregate shock g_t, and cross-sectional variation arises solely from differences in w_ℓ. In contrast to the canonical Bartik setting, there are not multiple quasi-random shocks across sectors. The credibility of the design thus hinges entirely on the exogeneity of g_t and the predetermined nature of the exposure measure w_ℓ.

The exact shift-share instrument in this setting can be expressed in terms of government purchases over output:[44]

$$\tilde{Z}_{\ell,t+h} := \underbrace{\left(\frac{1}{19}\sum_{\tau=2001}^{2019}\frac{G_{\ell,\tau}}{Y_{\ell,\tau}}\right)}_{\text{Share}(w_\ell)} \cdot \underbrace{\frac{G_{t+h}-G_{t-1}}{Y_{t-1}}}_{\text{Shift}(g_t)}.$$

where Y_{t-1} denotes the aggregate value of the normalizing regional activity variable—in our case, wages and salaries, defined as $Y_t := \sum_\ell Y_{\ell,t}$.

Identification. Identification requires the exogeneity of both the shifts and the shares. National shifts in defense expenditure are largely driven by geopolitical factors, as discussed in the paper. One potential concern is small-sample bias, since the time series dimension is relatively short ($T = 19$). For instance, the early 2000s were characterized by a military build-up following the 9/11 Terrorist Attacks, which coincided with the economic slowdown after the Dotcom crash. However, this coincidence does not hold systematically: defense spending cuts in 2013 due to sequestration occurred despite modest growth (nominal GDP grew by about 2%), while the military build-up of the first Trump presidency overlapped with strong economic growth. Thus, there is no clear countercyclical pattern between national defense spending shifts and national economic growth in recent years. Moreover, the time fixed effects in our baseline specification absorb any remaining aggregate variation, fully addressing these concerns.

The main source of variation in our data is cross-sectional, given the 358 MSAs in our harmonized baseline sample. Hence, the exogeneity of the shares is central for identification. In the

[44] We thank Gabriel Chodorow-Reich for raising this point when discussing the paper at the 2025 NBER Conference: Fiscal Dynamics of State and Local Governments.

canonical shift-share framework, shares are constructed as the time-average of the ratio of regional defense contracts to regional wages and salaries:

$$w_\ell := \frac{1}{19} \sum_{t=2001}^{2019} \frac{G_{\ell,t}}{Y_{\ell,t}}.$$

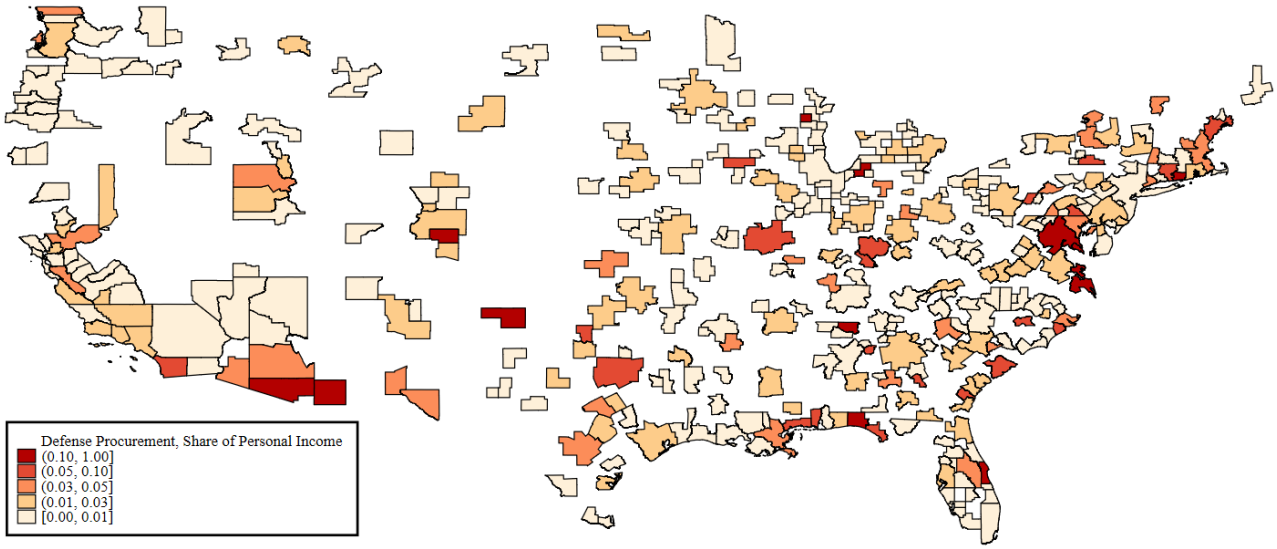


FIGURE B3 — MSA Geographic Distribution of Long-Run Shares of Defense Contracts w_ℓ

Notes: There are 380 MSAs. The figure omits Hawaii and Alaska.

By contrast, our baseline instrument redistributes national changes in defense spending according to the time-average fraction of defense contracts flowing into each region (i.e, regional exposure):

$$\exp_\ell := \frac{1}{19} \sum_{t=2001}^{2019} \frac{G_{\ell,t}}{G_t}.$$

We first show that these two definitions of shares are highly correlated. In particular, Figure B4 presents a bin-scatter of the log of the baseline analysis exposures ($\exp_\ell$) against the log of canonical shift-share shares (w_ℓ). We take logs to mitigate the influence of the thick right tails in the shares distribution. The figure reveals a strong positive correlation: regions that historically received a large fraction of total defense contracts (high-exposure) are also those with high contract-to-wage ratios (high-shares).

Second, we assess whether high-share regions differ systematically in employment growth. We

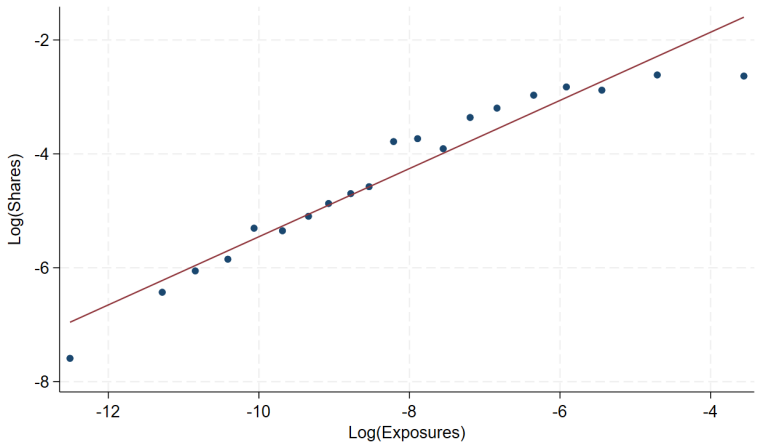


FIGURE B4 — (LOG)EXPOSURE AND (LOG)SHARES ARE HIGHLY CORRELATED

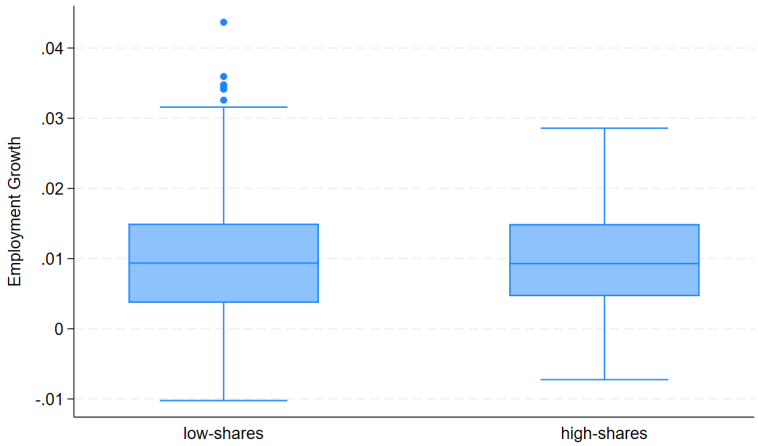


FIGURE B5 — HIGH- AND LOW-SHARE REGIONS HAVE SIMILAR ANNUAL EMPLOYMENT GROWTH RATES

compute the average annual employment growth rate

$$\frac{1}{19}\sum_{t=2001}^{2019}\frac{E_{\ell,t}-E_{\ell,t-1}}{E_{\ell,t-1}}$$

for high- and low-share regions, defining "high share" as above the 75th percentile of the share distribution (0.039, i.e., defense contracts equal to at least 3.9% of wages and salaries; the median share is 1.2%). Figure B5 compares the distributions of average annual employment growth rates for high- and low-share regions. The figure shows no systematic difference. A cross-sectional regression of average employment growth on a constant and a high-share dummy confirms the absence of any statistically significant gap.

TABLE B3 — REGIONAL EMPLOYMENT MULTIPLIERS - SHIFT-SHARE INSTRUMENTATION

Response of Total Employment from (Public) BEA Data					
	IV: Shift-Share Instrument				
Horizon	*Coefficient* (β_h)	*p*	*Effective F*	*Job-Years ($1M)*	*Cost-per-Job ($)*
impact	0.029	0.037	25.636	0.971	$1,030,040
	(0.014)			(0.464)	($492,296)
1 year	0.095	0.010	66.461	3.128	$319,701
	(0.036)			(1.205)	($123,147)
2 years	0.091	0.023	32.734	2.988	$334,660
	(0.040)			(1.304)	($146,073)
3 years	0.097	0.036	25.287	3.192	$313,282
	(0.046)			(1.518)	($149,023)

Notes: Sample: 2001-2019; 358 MSAs (QCEW+BDS+LAUS Harmonized Sample). GDP price deflator from BEA, base year 2008. Robust standard errors in parentheses, clustered at the MSA level. Montiel Olea and Pflueger (2013) effective F is calculated with `weakivtest`. Number of Job-Years refers to one million $. Standard error of cost-per-jobs are obtained with the Δ-method.

In summary, regions with higher baseline exposure to defense contracts coincide with high-share regions (Figure B4). Crucially, they do not exhibit systematically different average employment growth rates (Figure B5). Moreover, even if such differences existed, our baseline regression includes region fixed effects, which absorb any time-invariant heterogeneity across MSAs.

Robustness Estimates After having clarified the origin of the exogenous variation in our shift-share design, we re-estimate equation (1) by instrumenting the RHS with $\tilde{Z}_{\ell,t+h}$, the exact shift-share instrument, reporting the results in Table B3.

Employment multipliers (β_h) are estimated precisely at all horizons and their values are similar to those ones obtained from of the baseline analysis (Table 2). Similarly, the effective F-statistics are consistent with those ones obtained with the instrument reported in the paper. The conversion factor used to transform multipliers into estimates of job-years is not affected by the choice of the instrument. By consequence, both values of job-years and cost-per-jobs are similar to the baseline estimates reported in the paper.

Overall, adopting an exact shift-share instrument approach, yields identical estimates to our baseline approach based on Auerbach, Gorodnichenko, and Murphy (2020).

C Extra Establishment-level Results

C.1. Solicitations

Contracts awarded competitively are solicited on a government website, Federal Business Opportunities (FedBizOpps or FBO), now migrated to SAM.gov. Contracts solicitation allows any potential vendor to view the contract opportunity on the website and participate in the auction or negotiation. Usually, agencies post a "pre-solicitation" notice, informing vendors about the possibility that a contract opportunity may arise. Contracts are then officially solicited on the same website. In this period, contractors can submit offers in the form of (i) bids (i.e. either one or two steps sealed bidding) or, when the nature of the product is more complex, written proposals (i.e. contract by negotiations). Once the offer periods expires, awardee are competitively selected. All pre-award notices are gathered daily on SAM.gov. Following Gonzalez-Lira, Carril, and Walker (2021) approach, we download all daily solicitations posted on SAM.gov from fiscal year 2006 to fiscal year 2020, and then use information from the (i) solicitation number, (ii) awarding sub-agency name and (iii) fiscal year to identify unique contracts solicitations and reconstruct the entire pre-award sorted history: from the oldest pre-award notice to the award notice. Figure C1 summarizes the competitive procurement timeline process.

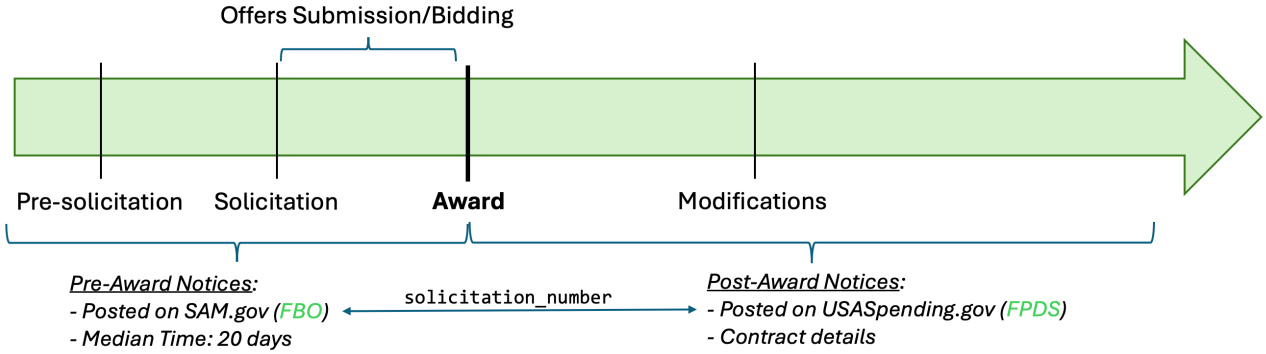


FIGURE C1 — TIMELINE OF COMPETED CONTRACTS

Notes: Once the contract is awarded, all detailed contract information is recorded in FPDS by the responsible federal contracting officer. Several "*post-award* actions follow the award, known as contract-modifications. Frequent examples of contract modifications are options to buy more from the government, extra-costs for extra work, appropriations of extra funds and contracts termination.

We keep all award histories from fiscal year 2006 to fiscal year 2019 to be consistent with the sample choice of the paper and then we analyze the number of days from the oldest pre-solicitation to the award notice, dropping solicitations which either (i) lack an award notice or (ii) consist only of a single notice. Figure C2 shows the box-plot of the (unweighted) number of days from the oldest pre-award notice to the award notice.

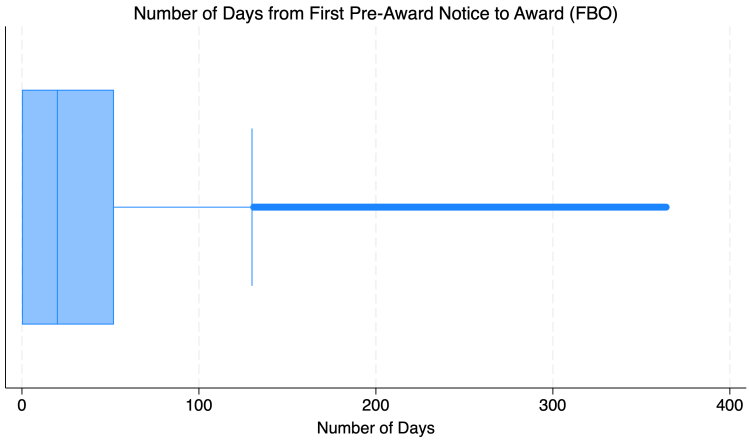


FIGURE C2 — BOX-PLOT OF NUMBER OF DAYS FROM OLDEST PRE-AWARD NOTICE TO AWARD

Notes: Distribution is not weighted by the value of a contract. Data source is the universe of federal procurement solicitation from FBO (Federal-Bizz-Opportunities.gov), now migrated to SAM.gov.

We find that the median time taken from the first '*pre-award*" notice (e.g. pre-solicitation) and the award notice for any competed federal contracts is 20 days, while for 75% of contracts this interval of time is 52 days, that is, well below the quarterly frequency used in the paper.

→ In light of the short time period between pre-solicitations and award date, we use the award date available from FPDS, a much more complete and comprehensive dataset than FBO, to identify the timing of the award. We address potential anticipation effects owing to the pre-award solicitations period by carrying out anticipation tests in the main body of the paper.

C.2. Matched Sample

We merge contractors who receive at least one unpredictable contract with establishment-level outcomes from the QCEW.

First, we construct a list of contractors that received at least one unpredictable contract in a

given year and county. Since the recipient-county field is not highly populated in the FPDS, we use the recipient zip code, which is almost never missing, to assign a geographic location to a contractor for a given year. We then use an official zipcode-to-county crosswalk to map zip codes to counties. Second, we split the QCEW into year-county sub-samples, which report all establishment names. Almost all firms, identified by a unique employer identification number (EIN), appear to have a single establishment within a county. Third, we use a string-matching algorithm (`reclink`) to match all firms from our dataset of DUNS numbers that win an unpredictable contract with the universe of firm/EIN names within a given year and county from the QCEW.

Matched Sample Descriptive Statistics: We were able to match 13,662 establishments. The data cleaning process involved: (i) removing observations with incomplete histories, i.e., time series with gaps in the outcome variables; (ii) excluding firms with fewer than 13 quarters of observations (four quarters of lags, eight quarters for the impulse response function horizon, and one quarter for the shock); (iii) excluding firms whose first unpredictable contract appears before the fifth observation, as we control for four lags; (iv) excluding firms whose first unpredictable contract appears in the last eight quarters observed, as we assess the impulse response function with an eight-quarter horizon; (v) removing firms with fewer than one employee on average; and (vi) removing establishments with more than 150 employees. The resulting dataset is an (unbalanced) panel dataset with $N = 5,142$ firms observed from 2006:1 to 2019:4, $T = 56$. The median contract size is \$114,900, while the mean is much larger, around \$700,000, indicating a very long right tail in the contract distribution, consistent with the findings in Cox et al. (2024).

C.3. Products Purchased via Unpredictable Contracts

In this Appendix section we provide more details on the types of products purchased via unpredictable contracts.

Product Categories Following Muratori, Juarros, and Valderrama (2023) we use the four-digit product category available from FPDS to distinguish between goods and service and aggregate

products at 2-digits. Figure C3 shows the average fraction of total unpredictable contracts spent on the top ten service categories, where the average is taken over fiscal years. Similarly, Figure C4 shows the fraction of unpredictable contracts spent on the top ten goods categories.

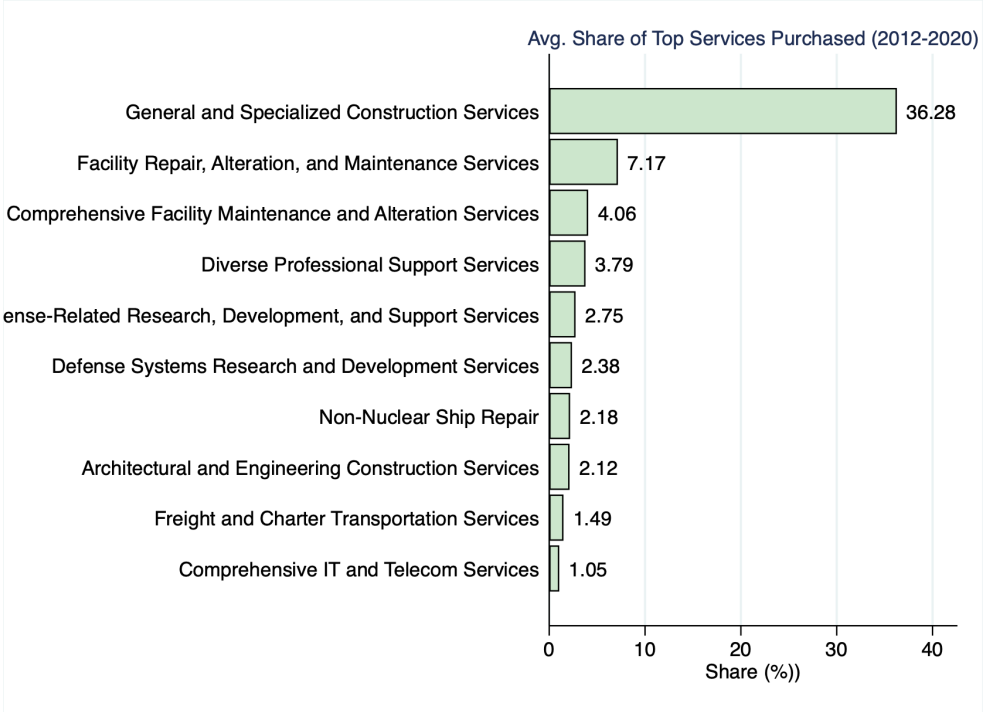


FIGURE C3 — TOP 10 SERVICES - FRACTION OF UNPREDICTABLE CONTRACTS

Almost half of all spending via unpredictable contracts are represented by construction-related services: general and specialized construction services (36.28%), facility repair, alteration and maintenance services (7.17%) and comprehensive facility maintenance and alteration services (4.06%). Moreover, more than 5% of spending originates from defense related R&D services.

Concerning goods, almost 10% of unpredictable contracts are spent on food products used, for instance, to supply military basis. Manufacturing goods strictly related to defense hardware accounts for about 7% of spending via unpredictable contracts: maritime vessels and watercraft (3.8%), aircraft and space vehicles (1.15%), communications and electronic equipment (0.97%), military ordnance and explosive materials (0.47%), land vehicles (0.38%) and aerospace vehicles components and systems (0.32%).

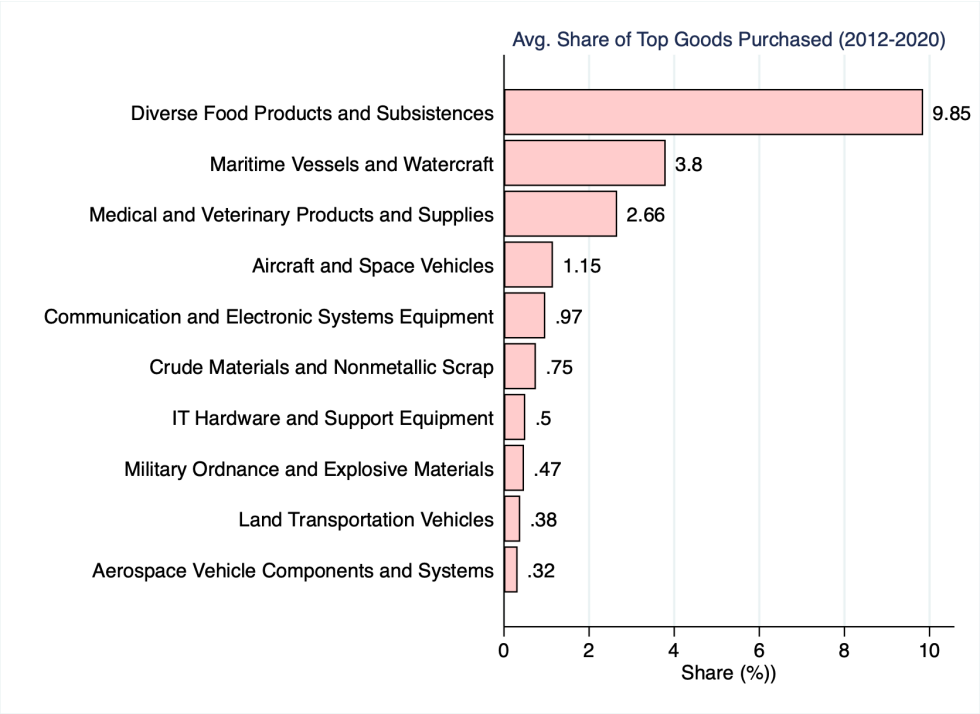


FIGURE C4 — TOP 10 GOODS - FRACTION OF UNPREDICTABLE CONTRACTS

Duration of Unpredictable Contracts Every contract in FPDS reports a period of performance start date and a period of performance current end date. We take the difference in days between the two to calculate the duration of all unpredictable contracts. We then plot in Figure C5 the box-whiskers plots of the duration (number of days) of unpredictable contracts by spending category.

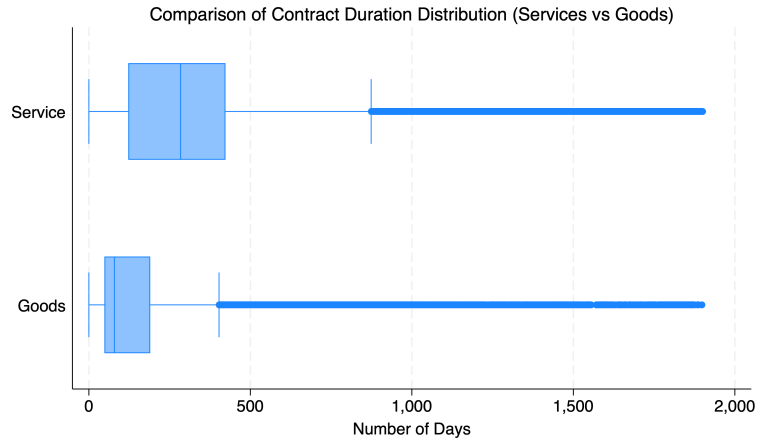


FIGURE C5 — IN-SAMPLE (UNWEIGHTED) DISTRIBUTION OF CONTRACTS' DURATION

Notice that service contracts tend to have a longer duration than contracts for goods. In the case of services the first quartile is 121 days, the median is 283 days, and the third quartile is 423

days. In the case of goods, the first quartile is 48 days, the median is 79 days, and the third quartile is 190 days.

C.4. Analysis by Quartile of Small Establishments

We subdivide the sample of small establishments by analyzing each quartile of their size distribution separately. Establishments in the first quartile have between 1 and 6 employees, establishments in the second quartile have between 6 and 13 employees, and establishments in the third quartile have between 13 and 28 employees. The fourth quartile is characterized by much greater dispersion in the number of employees: while the first three quartiles range from 1 to 28 employees, the last quartile ranges from 28 to 150, thus including much larger establishments.

Therefore, we re-estimate Equation (8) for each quartile of the establishments' size distribution separately to explore the robustness of the result across the sample. Figure C6 shows the IRFs of employment growth for each quartile. Note that the results appear to be robust across all four quartiles of the size distribution.

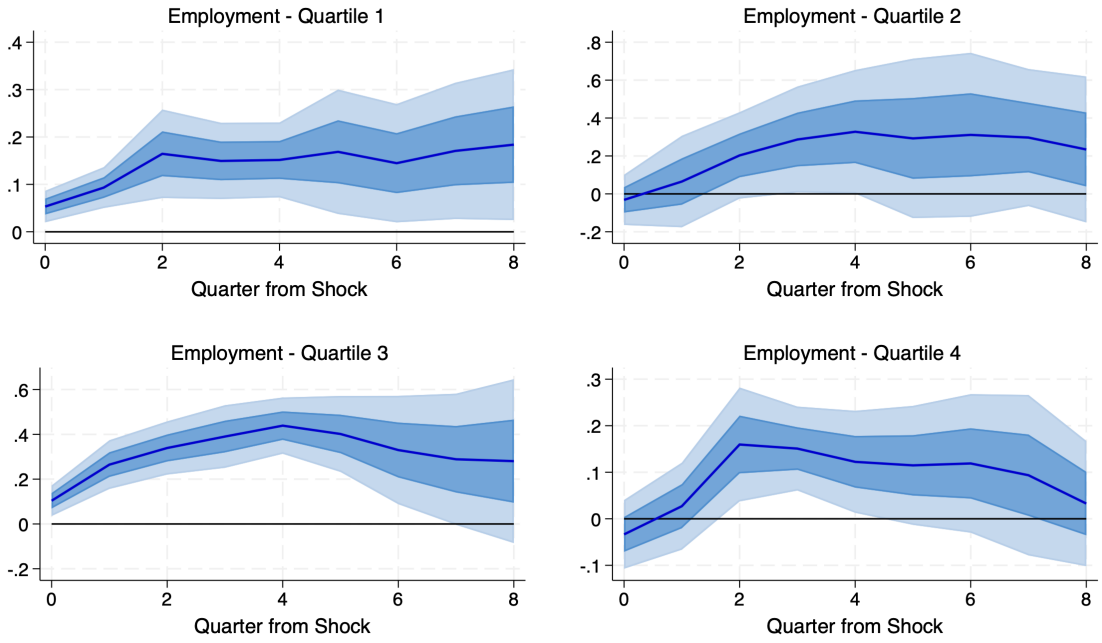


FIGURE C6 — EMPLOYMENT - QUARTILES OF SIZE DISTRIBUTION OF ESTABLISHMENTS

C.5. Time-Varying Productivity Shocks

The inclusion of establishment-fixed effects in the baseline equation (8) removes only the effects of systematic differences in productivity levels across establishments, while our use of highly competed newly awarded definitive contracts rules out the possibility that contracts are awarded in response to the development of innovative products (i.e., sole sourcing). However, they are not capable of controlling for time-varying productivity shocks that make establishments temporarily more productive. Therefore, we are concerned that establishments might win contracts in response to temporary productivity shocks, which make them capable of outbidding their competitors and, consequently, outgrowing them (i.e., omitted variable bias).

To address this concern, we re-estimate Equation (8) by augmenting the specification with four lags of wage-per-worker. According to Neoclassical theory, the marginal product of labor is equal to the real (product) wage. Consequently, changes in wage-per-worker should reflect changes in productivity levels. Thus, using lags of wage-per-worker enables us to control for time-varying productivity shocks.

Results are reported in Figure C7, where it is clear that the response of establishments' employment is robust to the inclusion of lags of wage-per-worker in the specification.

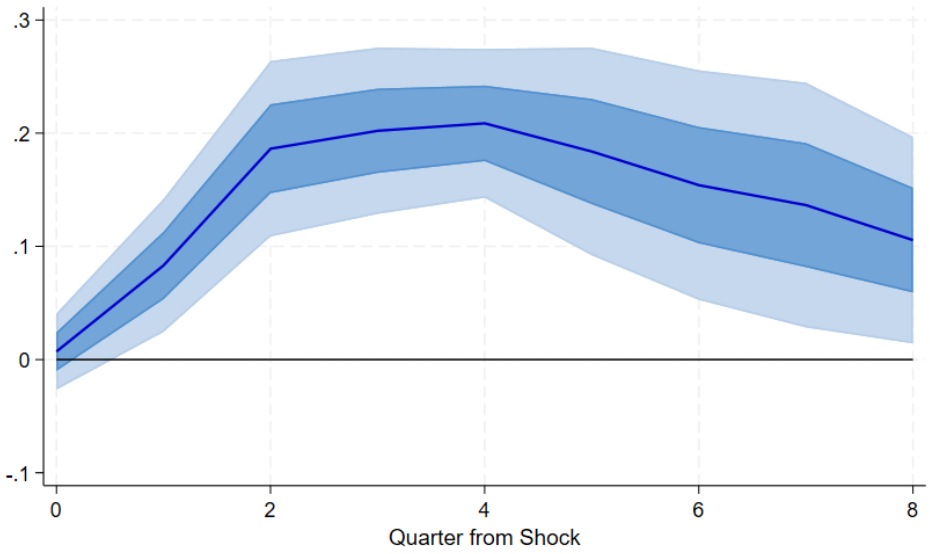


FIGURE C7 — RESPONSE OF ESTABLISHMENT'S EMPLOYMENT CONTROLLING FOR LAGS OF AVERAGE WAGE

C.6. Response of Wages

The restricted QCEW data also provide quarterly values of total wages paid by the establishment. We re-estimate Equation (8) using changes in total wages as the outcome variable. We report the results in Figure C8.

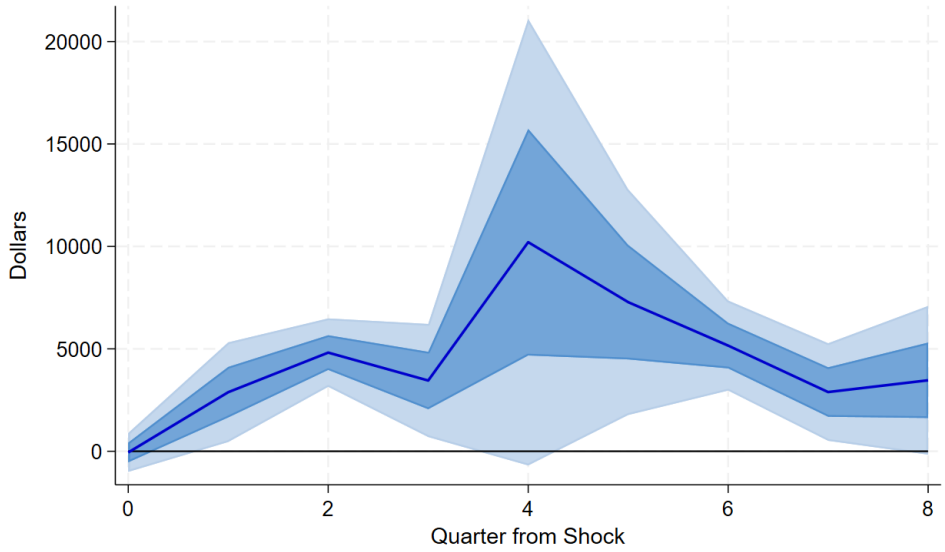


FIGURE C8 — RESPONSE OF TOTAL WAGES

Notes: The unit on the left axis is dollars, in response to a $1 million worth unanticipated contract.

Not surprisingly, unanticipated contracts have positive and significant effects on total wages, consistently with the positive and significant response of employment.

Second, we ask whether unanticipated contracts have any meaningful effect on the average wage paid to employees of winning establishments. In particular, we study the response of the average wage, or wage-per-worker, using the same specification as Equation (8). Results are reported in Figure C9, which displays no significant effect on wage-per-worker.

Our finding that employment, rather than wages, is the primary adjustment margin is consistent with Komarek, Butts, and Wagner (2022), who study procurement cuts under the Budget Control Act and also find that local labor markets primarily respond through employment.

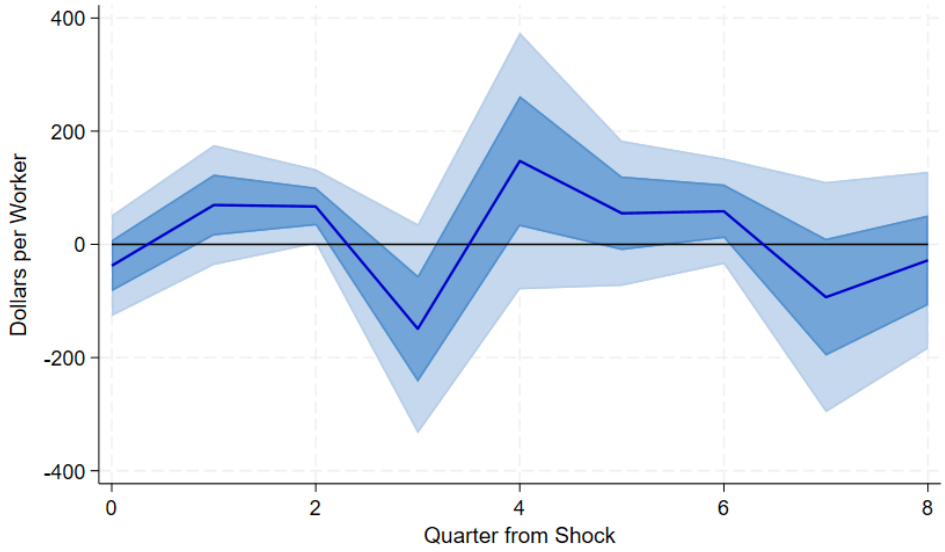


FIGURE C9 — RESPONSE OF AVERAGE WAGE

C.7. NETS Analysis

To corroborate the establishment-level employment responses to procurement contracts presented in Section VI, we construct a panel dataset linking federal contract records from the Federal Procurement Data System (FPDS) to detailed employment microdata from the National Establishment Time Series (NETS).[45] This micro-level analysis allows us to directly estimate the effect of procurement inflows on employment using an alternative dataset to the inaccessible BLS dataset we use in our main analysis.

Given well-documented limitations of the NETS dataset, including inflated establishment counts, inconsistencies in employment reporting over time, and potential misclassification of firm ownership and geographic location, we implement a comprehensive series of cleaning and consolidation steps to construct a reliable panel of establishments. Establishments are defined as unique combinations of firm ownership, geographic location (ZIP code), and partial address, following the approaches by Barnatchez, Crane, and Decker (2017), Crane and Decker (2019), and Choi, Penciakova, and Saffie (2023). Firm ownership is traced through a recursive mapping of headquarters

[45] NETS data were obtained by Ricardo Duque Gabriel under the purview of the Board of Governors' license agreement with the data provider. The remaining co-authors did not have any unauthorized access to NETS data while working on this paper. We thank Joonkyu Choi and Leland Crane for sharing insights and code to harmonize NETS with BDS.

identifiers, resolving chains of ownership to identify ultimate parent firms. Observations listing themselves as their own headquarters are adjusted by deducting one employee, in line with standard practice to address over-reporting. After that, we retain only establishments with positive employment. Sectors not covered by the Business Dynamics Statistics (BDS), such as education and public administration and observations with fewer than 10 or more than 1,000 employees are excluded (Barnatchez, Crane, and Decker, 2017). Finally, we exclude from the sample all establishments with at least one imputed observation for employment. The cleaning assumptions are designed to ensure consistency with prior literature while preserving the granularity required for establishment-level analysis.

We use panel local projections to estimate the effect of $1 of unanticipated contracts on employment (Jordà, 2005). In particular, given that NETS data is of annual frequency, we adapt Equation (8) and estimate via OLS the following equation:

$$E_{i,t+h} - E_{i,t-1} = \beta^h \cdot \varepsilon^g_{i,t} + \gamma^h_0 \cdot \tilde{G}_{i,t} + \text{Lags} + \underbrace{\alpha^h_i + \alpha^h_{s,t} + \alpha^h_{\ell,t}}_{\text{Fixed Effects}} + \upsilon_{i,t+h} \quad h = 0, 1, 2, 3, \tag{9}$$

where $E_{i,t+h}$ denotes the h-period ahead number of employees; $\varepsilon^g_{i,t}$ denotes the dollar value of unanticipated contracts awarded to establishment i in year t, while $\tilde{G}_{i,t}$ indicates the dollar value of potentially anticipated contracts. Both are expressed in units of $1,000,000 of 2008 dollars. $\text{Lags} := \sum_{j=1}^{3} \left\{ \rho^h_j \cdot \varepsilon^g_{i,t-j} + \gamma^h_j \cdot \tilde{G}_{i,t-j} + \phi^h_j \cdot (E_{i,t-j} - E_{i,t-1-j}) \right\}$. α^h_i represents an establishment fixed effect, $\alpha^h_{s,t}$ is a sector-time fixed effect intended to absorb any sectoral business-cycle effects. Lastly, $\alpha^h_{\ell,t}$ represents a state-time fixed effect, capturing regional business-cycle effects within a state. Our sample is composed of 28,393 establishments between 2006 and 2019.

The OLS estimates of β^h can be interpreted as impulse response functions (IRF) of the effect of an extra dollar of spending on establishment-level employment. Table C1 presents the results.

To validate the main results using an alternative data source, Figure C10 replicates the anticipation and placebo analyses from Figure 7 using the NETS dataset. While the estimates are notably noisier, consistent with the known limitations of NETS, the magnitude of the effects remains remarkably similar. This alignment reinforces the credibility of the coefficient used in the

TABLE C1 — EMPLOYMENT RESPONSE: NETS SAMPLE

Horizon	*Coefficient* (β_h^c)	p
impact	0.013	0.531
	(0.020)	
1 year	0.150	0.160
	(0.106)	
2 years	0.173	0.219
	(0.140)	
3 years	0.176	0.220
	(0.143)	

Notes: Sample: 2006–2019. 28,393 establishments. Coefficients are from Equation (9).

breakdown between the direct and indirect effect.

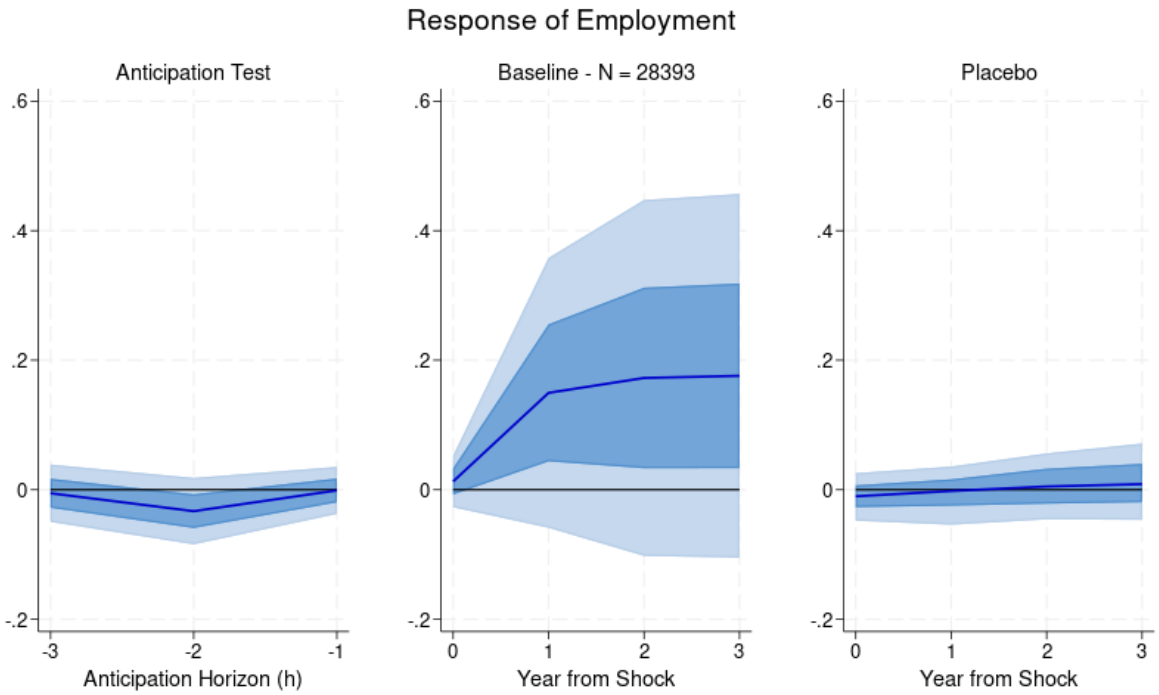


FIGURE C10 — EMPLOYMENT EFFECTS: NETS SAMPLE

Notes: Firms are observed from 2006 to 2019. The number of establishments is $N = 28,393$. Standard errors are clustered at the state level. Small bands represent 68% confidence intervals, and large bands represent 95% confidence intervals.

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