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

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Abstract

Using matched U.S. contract and employment data, we show that the employment effects of defense procurement are costly, concentrated, and slow to diffuse, yet over time they can support industrial capacity and regional economic development. 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, spillovers emerge gradually and account for half the regional effect by year three, suggesting delayed but persistent medium-term gains across industries. Within contractors, only 15% of job creation occurs at recipient establishments, underscoring the role of subcontracting and input-output linkages.

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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 as a defense strategy, but also as a tool for long-term economic development. Yet despite its scale and policy importance, the economic consequences of procurement, and particularly its labor market effects, remain poorly quantified, and the channels through which job gains are realized are not well understood.

In the United States, 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 aggregate and regional employment multipliers from defense spending have been studied, less is known about what originates these job gains. In particular, little evidence exists on how effects unfold across contractors and non-contractors, whether employment gains originate primarily from direct recipients or broader 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 over time they can support industrial capacity and regional economic development. 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 defense-intensive sectors of defense-intensive sectors. On impact, employment in non-contracting

firms is crowded out, but 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 subcontracting and production linkages in diffusing procurement-driven employment growth.

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 macroeconomic effects, we focus on employment as the primary outcome. For this purpose, we draw 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 match the universe of defense

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

[2] 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.

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 (LDBE) also enable us to analyze the direct effects of procurement on contract recipients at the establishment level.

We begin by estimating the average regional employment response to defense procurement shocks using a Bartik-style instrument that adopts the shift-share framework of Nakamura and Steinsson (2014). Our approach isolates exogenous variation in regional procurement exposure by interacting national changes in defense spending with historical regional shares of federal contract awards (Demyanyk, Loutskina, and Murphy, 2019; Auerbach, Gorodnichenko, and Murphy, 2020; 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 fiscally costly, averaging roughly $284,000 per job-year. This high cost reflects the elevated wage structure of defense-intensive sectors (Bartal and Becard, 2024) and underscores the limits of procurement as a tool for short-run employment generation.

We start by breaking 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 intensive 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 Bartik

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 an even smaller share of the resulting employment gains. 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.

We break down the regional employment response by contractor status using restricted matched data from the Bureau of Labor Statistics (BLS) that allow us to distinguish between firms with a history of receiving defense contracts ("contractors") and those without ("non-contractors"). This disaggregation reveals important dynamics: 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 production.

We further decompose the contractor-side employment response by isolating the effects on establishments that directly receive contract awards. Using 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; Barattieri, Cacciatore, and Traum, 2023), at the state level (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 several contributions to this literature. First, we show that defense procurement is a relatively costly source of job creation compared with other types of spending or similar programs in earlier periods. In particular, we estimate a one-year cost-per-job of about $290,000 (2008 dollars). By contrast, the review by Chodorow-Reich (2019) reports estimates for ARRA spending in the range of $25,000–$125,000 per job[4]. We argue that these differences are likely due to defense contractors being concentrated in higher-paying jobs (Bartal and Becard, 2024). Moreover, 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. Second, we contribute by providing a novel breakdown of the employment multiplier by firm

[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

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 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, 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 DoD contracts are likely to be fully unanticipated. 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. Section 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 (G), while the right panel shows its share of GDP. On average, procurement spending constitutes about 16% of G 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.[5]

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 action.[6] 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.[7]

The richness of FPDS data enables research at highly disaggregated levels, including sub-industries (six-digit NAICS codes), regions (MSAs and counties), and firms (public compa-

[5] More than 90% of U.S. procurement spending corresponds to contracts with a primary place of performance in the U.S.

[6] 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).

[7] 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.

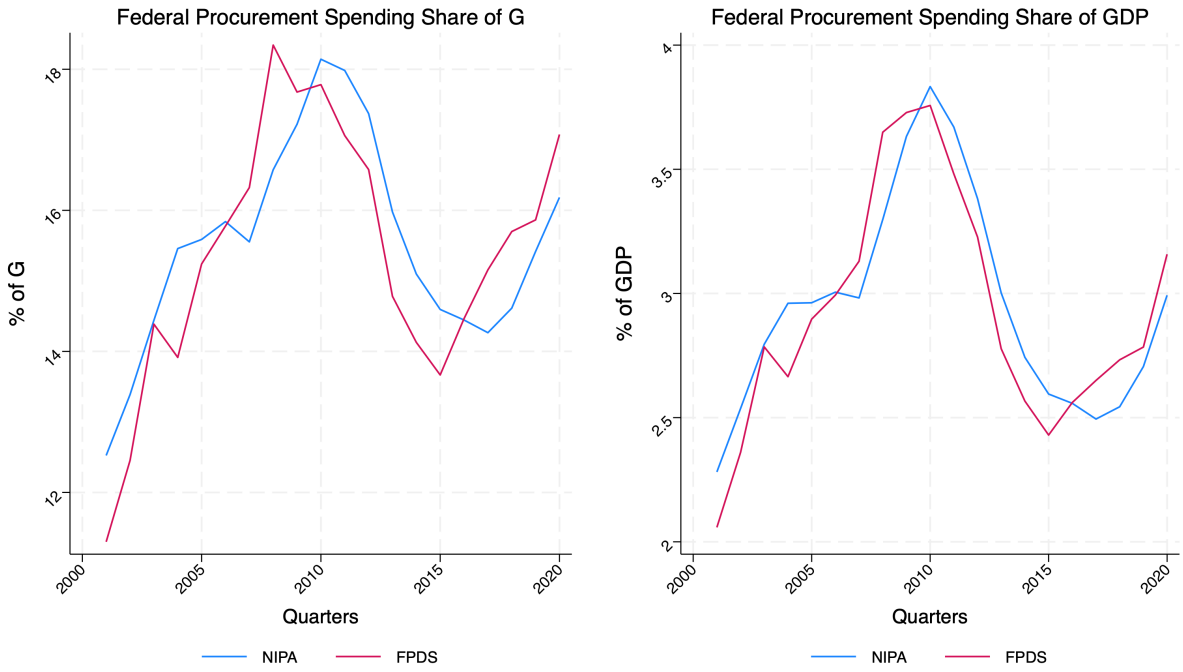


FIGURE 1 — FEDERAL PROCUREMENT SPENDING SHARES

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.

nies). In this paper, 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.[8]

Two-thirds of all contracts are awarded by the Department of Defense (DoD). Regional-

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

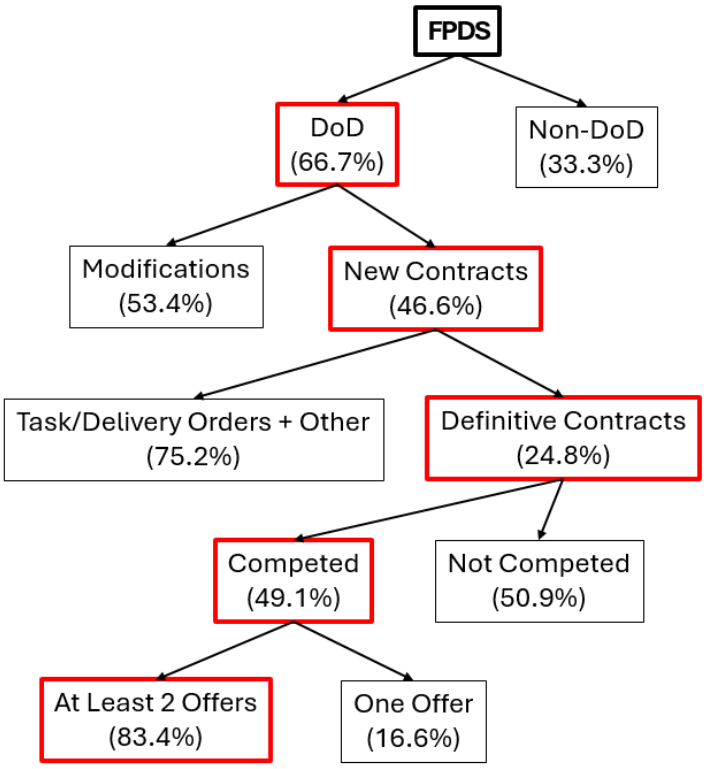


FIGURE 2 — BREAKDOWN OF FPDS CONTRACTS

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

level analyses 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).[9] Conversely, only 24.8% of new defense contracts represent standalone contracts, which are not part

[9]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.

of any ongoing relationship with the government. These are technically referred to as "definitive contracts." 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.[10] 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.[11] 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.[12] 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.[13] Thus, analyses conducted at

[10] See FAR 6.302-1-a.

[11] 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.

[12] 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).

[13] Further details on contract solicitations are provided in Appendix B.1.

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.

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. At the regional level, however, the use of a Bartik shock provides a way to isolate the unanticipated component of procurement spending by exploiting variation in regional contract exposure induced by national changes in defense expenditures. As highlighted in our breakdown (Figure 2), this institutional feature of procurement contracting reinforces the case for adopting a Bartik instrument when analyzing regional effects of procurement spending. We therefore follow the existing literature and employ a Bartik instrument 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 Bartik shock.

In sum, this section motivates a two-tiered empirical strategy. At the regional level, we exploit variation in contract exposure using a Bartik 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.

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.

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 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.[14] Second, we obtained access to firm-level microdata covering 42 states and DC

[14]The dataset has been recently discontinued amid budget cuts but it is still available for download in the

from the Longitudinal Database of Establishments (LDBE), the BLS microdata used to produce the public QCEW.[15] 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 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.

archive.

[15] 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.

Estimation of the Regional Employment Multiplier Following the regional multiplier literature, we estimate this equation:[16]

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

where $E_{\ell,t}$ represents employment in region ℓ and year t, and $Y_{\ell,t-1}$ is annual regional wages and salaries.[17] The terms $\alpha_{t,h}$ and $\lambda_{\ell,h}$ are time and location fixed effects, respectively (specific to the horizon of the estimate h). The government spending measure $G_{\ell,t}$ represents defense contracts from FPDS, aggregated by region-year.[18]

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).

To address these concerns, papers in the regional multiplier literature have (i) focused on changes in total *defense* spending, as national-level changes are mostly driven by exogenous geo-political factors, and (ii) used a shift-share design (Bartik, 1991). In our context, the Bartik-

[16] See Nakamura and Steinsson (2014), Demyanyk, Loutskina, and Murphy (2019) and Auerbach, Gorodnichenko, and Murphy (2020).

[17] 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).

[18] 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.

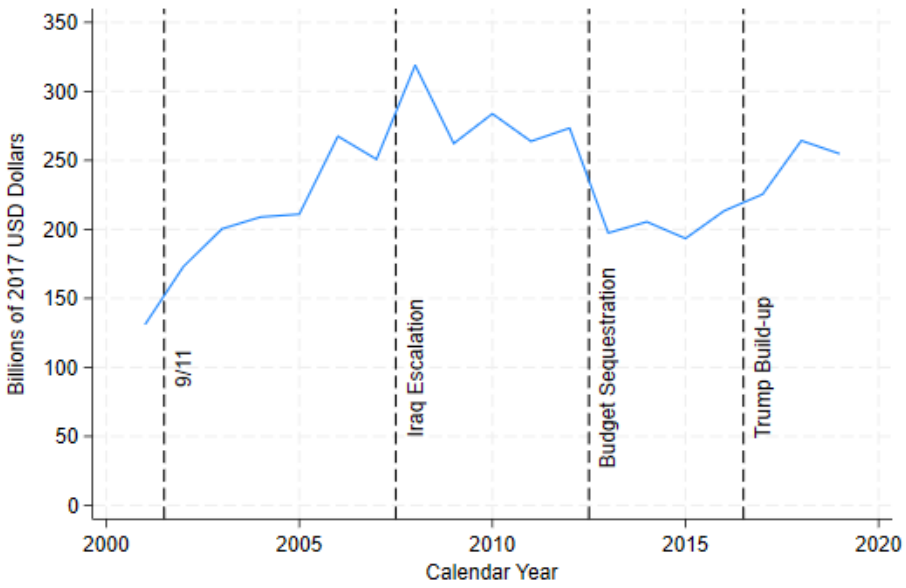


FIGURE 3 — DEFENSE CONTRACTS IN MSAS BY YEAR

type instrument is defined as:

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

where $G_{t+h} - G_{t-1}$ represents the aggregate change in defense contracts (*shifts* in the time series depicted in Figure 3), and s_ℓ denotes the long-run exposure of regions to defense spending (*shares* depicted in Figure 4).

Shifts Figure 3 shows the real value of defense contracts directed to MSAs from 2001 to 2019. Our shifts are constructed as differences in the level of this variable.

In the first part of the sample, defense spending increased dramatically following the 9/11 terrorist attacks, due to the ensuing wars in Afghanistan and Iraq. Second, in the aftermath of the financial crisis and the worsening government deficit, several budget cuts were planned by the Obama administration in 2011. However, these were repeatedly delayed until March 2013, when *budget sequestrations* were finally implemented.[19] Following the Russian invasion

[19]This is consistent with both the large negative shock recorded in the Ramey and Zubairy (2018) defense news shock series in 2013 and the fiscal consolidation narrative dataset in Alesina, Favero, and Giavazzi (2014), which

of Crimea in 2014, and the establishment of unified Republican control of Congress and the Presidency, after President Trump's election in 2016, a substantial increase in defense procurement spending reversed the downward trend caused by the sequestrations. Overall, exogenous events drive the shifts in the instrument.[20] This is relevant since, with exogenous shifts, identification can be achieved even with potentially endogenous shares (Borusyak, Hull, and Jaravel, 2022).[21]

Shares The instrument relies on the long-run shares of defense contracts, s_ℓ, to distribute national-level shifts across regions. The shares are constructed as the average regional share of total defense procurement over the sample period, 2001–2019:

$$s_\ell := \frac{1}{T} \sum_{t=2001}^{2019} \frac{G_{\ell,t}}{\sum_\ell G_{\ell,t}}.$$

As noted above, when shifts are exogenous, the strict requirement of exogenous shares is not necessary for identification. However, 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 shares to improve efficiency and ensure robustness (Goldsmith-Pinkham, Sorkin, and Swift, 2020). Figure 4 illustrates their geographic distribution.

The MSA with the largest share of contracts is Washington–Arlington–Alexandria, accounting for about 12% of DoD 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 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.[22]

lists the Budget Sequestration Act as an exogenous expenditure-based fiscal consolidation.

[20] See Amodeo and Briganti (2025) for a full narrative of major events driving the path of defense spending in the U.S. post-2000 sample.

[21] Relying on exogenous time-variation from the shifts is important to achieve identification, even if the baseline specification (Equation (8)) includes time fixed-effects which purge the identifying variation from unit-invariant time components (i.e., aggregate levels).

[22] Other large MSAs include Fort Worth-Dallas (about 7%, Naval Air Station Joint Reserve Base Fort Worth,

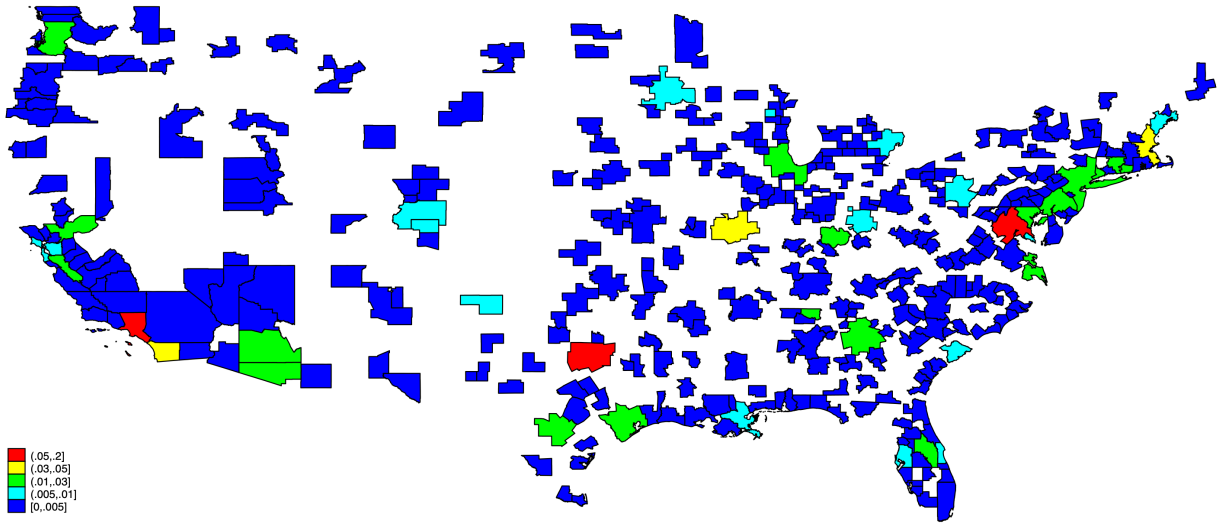


FIGURE 4 — MSA DISTRIBUTION OF LONG-RUN SHARES OF DEFENSE CONTRACTS

Notes: The figure omits Hawaii and Alaska. Red means long-run share 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%.

Thus the geographic allocation of national funds across regions is plausibly pre-determined relative to current economic conditions.

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

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, impact multipliers 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

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.

TABLE 2 — REGIONAL EMPLOYMENT MULTIPLIERS - BASELINE ESTIMATES

Response of Total Employment from (Public) BEA Data					
	IV: Bartik Instrument				
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. 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.

as follows:[23]

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

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.

The three-year employment multiplier implies a cost-per-job of $283,746 in 2008 dollars.[24] For context, Chodorow-Reich (2019) review ARRA transfers and report a cost-per-job range (at the two-year horizon) between $25,000 and $125,000. Table 3 summarizes cost-per-job estimates from the literature, including both defense spending shocks and other forms of fiscal spending such as the ARRA.[25].

As shown in the Table, estimates of the cost-per-job of additional defense spending in recent years are substantially higher than corresponding estimates based on ARRA transfers. This should not come as a surprise given the different nature of the fiscal shock and also the

[23] Construction of job-years consistent with Chodorow-Reich (2019) and Muratori, Juarros, and Valderrama (2023).

[24] We use 2008 as the base year to make our estimates comparable to papers on ARRA transfers, which typically use nominal contemporaneous dollars.

[25] 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*
Demyanyk, Loutskina, and Murphy (2019)	Defense Contracts	2007-2009	828 US CBSAs	0.53	$188,889
Auerbach, Gorodnichenko, and Murphy (2020)	Defense Contracts	2001-2016	383 US MSAs	0.58	$188,679
Muratori, Juarros, and Valderrama (2023)	Defense Contracts	1979-2019	US MSAs	1.03	$97,087
Nakamura and Steinsson (2014)	Defense Contracts	1966-2006	US States	2.25	$44,311
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 & Mehkari (2016)	ARRA Subcomponents	2008-2010	US Commuting Reg.	0.95	$104,931
Adelino, Cunha, and Ferreira (2017)	Local Spending	2007-2013	Municipality	-	$25,000
Chodorow-Reich (2019)	ARRA Transfers	2008-2010	US States	2.01	$49,750
Dupor & McCrory (2018)	ARRA Subcomponents	2008-2010	US Commuting Reg.	1.85	$54,054

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, which uses the Bartik instrument. Geography refers to main estimates.

difference geographical aggregation. For example, DLM19 and AGM20 report a long-run cost-per-job of about $188,000 in 2001 dollars (roughly $227,000 in 2008 dollars), close to our baseline estimate of $283,000. This evidence suggests that military procurement in the 21st century has been less effective in stimulating employment than other types of spending or earlier defense outlays.

A likely explanation can be inferred from an important insight in Bartal and Becard (2024): U.S. defense contractors are now concentrated in high-paying industries, employing highly educated professionals such as aerospace and software engineers, mathematicians, and cryptographers. As a result, defense procurement tends to generate specialized, high-wage jobs, driving up the cost-per-job, especially as we focus on the most recent years.

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 unem-

ployment 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 Bartik 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 Bartik shock, which coincides with our horizon.

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.

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

TABLE 4 — ESTIMATES FROM BUSINESS DYNAMICS STATISTICS CONSISTENT WITH BASELINE

Response of Private Employment from (Public) BDS Data				
	IV: Bartik Instrument			
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.

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 Bartik 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.[26] We decompose the regional BDS employment growth rate into three size categories:

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

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 Bartik 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. 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

[26] 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 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).

robust to using different samples.[27]

Breaking Down Government Spending by Firm Size We next show that the stronger response of large firms in Table 5 owes to Bartik 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 Bartik 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 B.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).[28] We are able to match 97.6% of total FPDS defense contracts (by value) to firms in NETS using this procedure.

[27] 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.

[28] 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 Bartik shocks mostly affect contract awards to large firms.

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

The regional employment multiplier estimated in Table 2 could be driven by (i) firms directly engaging in military contracting, (ii) firms indirectly engaging in military contracting (e.g., subcontracting and input-output connections) and/or (iii) firms totally unrelated with military contracting but affected by the Bartik shock through indirect demand spillovers at the regional

level, such as non-tradable services like dining.

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.[29] 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 ac-

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

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.

counts for privately owned firms.[30] The precision of the estimates as well as the effective F decrease, which is not surprising given the smaller sample size. Nonetheless, the magnitude of the effective F is reassuringly high after one year from 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.[31]

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

[31] 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.

Finally, we want to make sure that estimates obtained using the smaller harmonized sample are consistent with those ones estimated from the larger and more representative harmonized QCEW+BDS+LAUS sample. Therefore, we compare the estimates of job-years presented in blue in Table 7 with the baseline estimates presented above in Table 2. Notice that the dynamics of the estimates are consistent with each other: magnitudes and statistical significance are weaker on impact and grow over time. Concerning magnitudes, at horizon one, when the F-statistic is almost 30 in the smaller sample, the estimated job-years per million dollars of contracts is 2.2 per million dollars when using LDBE data and 2.7 when using the public BEA data in the smaller sample, while it is 3.4 in the larger sample (see column 5 and year 1 effect in Table 2). Therefore, most differences in the estimates of employment multipliers between Table 7 and the baseline estimates of Table 2, are primarily driven by sample differences and even then the differences are not large. We see this result as a reassuring robustness check: a much smaller sample is still able to produce comparable estimates.

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 match-

ing is restricted to establishments in MSAs located entirely within the 42 signatory states (plus Washington, D.C.) to which we were granted data access[32], observed between 2006 and 2019. It is carried out using a string-matching algorithm by cleaned firm name within a county and year.[33]

Having constructed MSA-annual time series of defense contractors' and non-contractors' employment we can 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 the 2SLS estimate of β_h:

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

In particular, the 2SLS estimate of β^{c}_h 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^{nc}_h represents the broader indirect effect of defense contracts on firms

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

[33]Our matching algorithm is able to match almost all defense contractors. In fact, the total aggregated amount of defense spending resulting from aggregating contracts awarded to our set of matched defense contractors accounts for more than 90% of total defense spending for each state included in the analysis, on average.

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).

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. 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.[34]

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

[34] 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.

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 Bartik 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 Bartik shock. Accordingly, we use high-precision 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 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).[35] 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.[36] 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 string-matching algorithm. 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 their time series, (ii) receive their only contract shock in their first four observed quarters, so we cannot control

[35] 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.

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

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. We noticed that beyond 27 employees, the size distribution becomes excessively skewed with extremely high values for the number of employees; therefore, we cap the employment of establishments at 150 employees. This leaves us with 5,142 establishments in the analysis sample.[37] 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 B.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.[38]

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

[37] Details on our sample of establishments are discussed in Appendix B.2.

[38] 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.

divided 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:

$$E_{i,t+h} - E_{i,t-1} = \beta^{h} \cdot \varepsilon^{g}_{i,t} + \gamma^{h}_{0} \cdot \tilde{G}_{i,t} + \text{Lags} + \underbrace{\alpha^{h}_{i} + \alpha^{h}_{s,t} + \alpha^{h}_{\ell,t}}_{\text{Fixed Effects}} + \upsilon_{i,t+h} \quad h = 0, 1, ..., H, \qquad (7)$$

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.[39] "*Lags*" refers to lags of shocks and outcomes.[40] 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.[41] 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.[42] Conditions (iii)

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

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

[41] 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.

[42] 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.

and (iv) are similar to those imposed by Hebous and Zimmermann (2020) while condition (i) was introduced in 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 a Bartik 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.[43]

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

[43] More descriptive statistics of unanticipated contracts are available in Appendix B.3.

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, establishments learn about a contract opportunity well ahead of the award notice and anticipate winning the contract.[44]

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., presolicitation) 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 B.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.[45] 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; unanticipated contracts are

[44] 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.

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

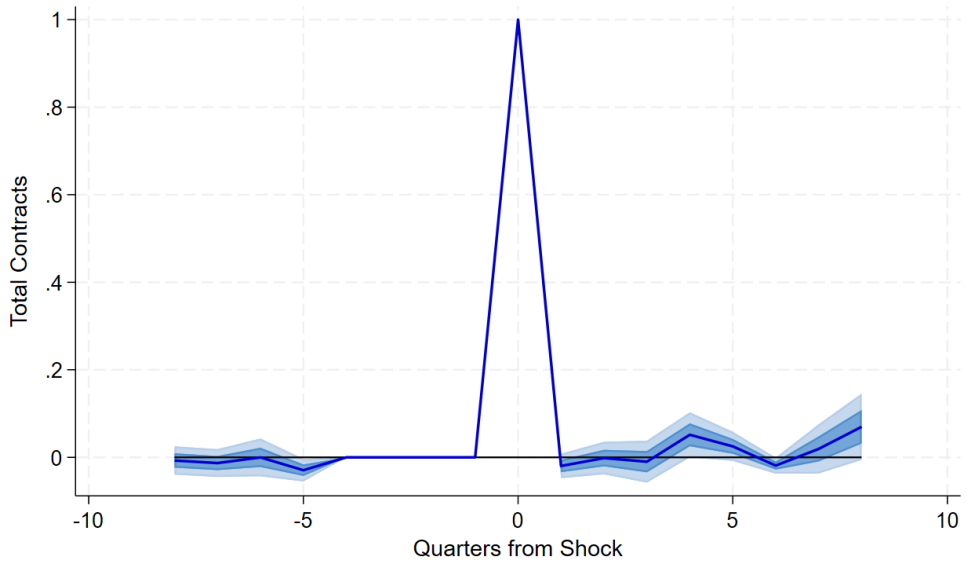


FIGURE 5 — UNANTICIPATED CONTRACTS BEHAVE AS ONE-TIME (DEMAND) SHOCKS

not awarded in response to 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.

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 Bartik shocks to regional defense spending, as studied in the previous sections.

Second, we include unanticipated contracts awarded by any federal agency—not only the Department of Defense—in our analysis. Any differences in employment effects between de-

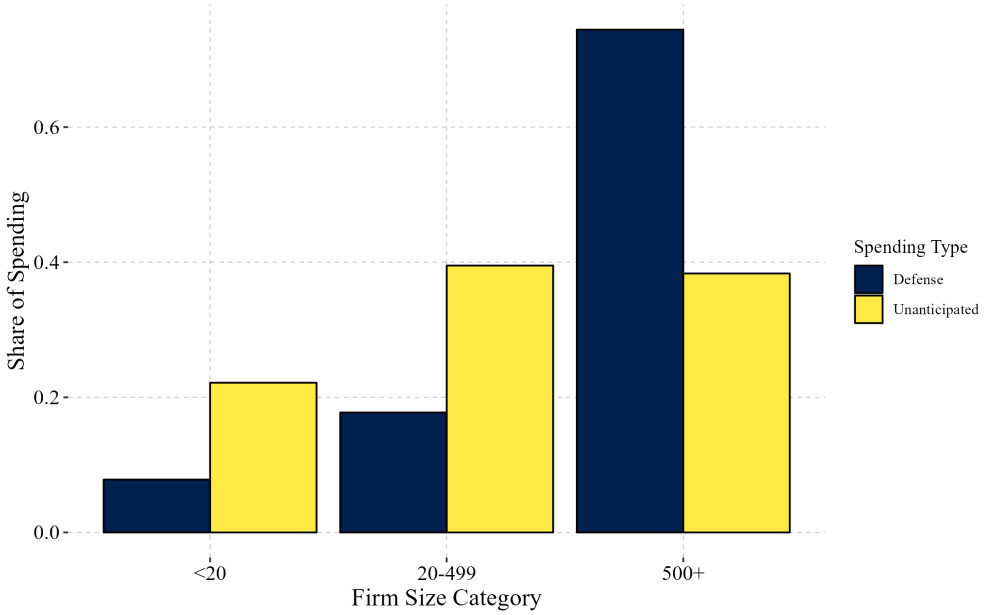


FIGURE 6 — SIZE DISTRIBUTION BY SPENDING TYPE

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.

fense 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. Nevertheless, 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 Bartik shocks appear 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 Bartik shock.

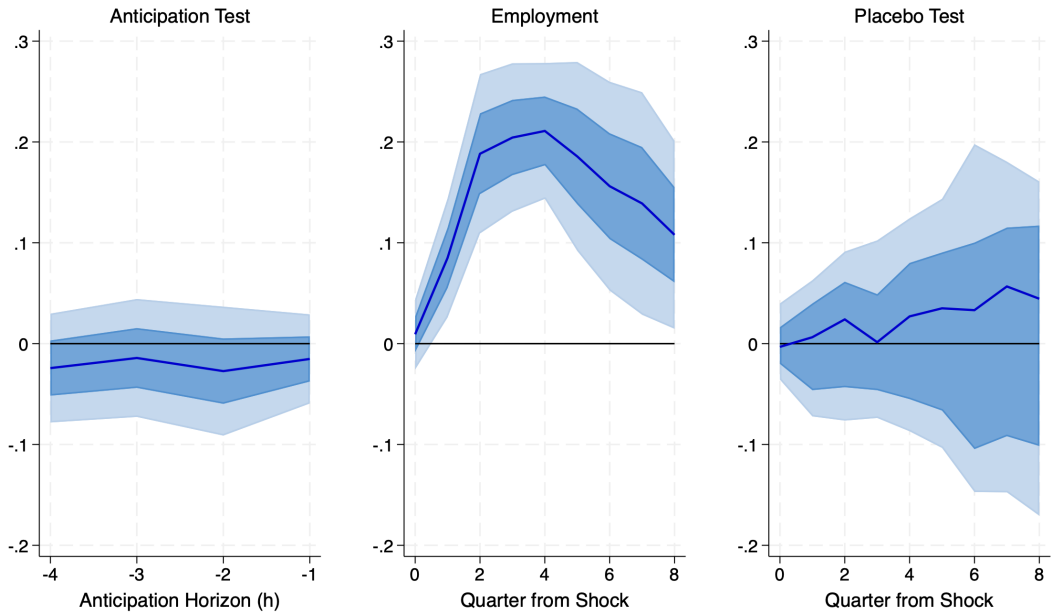


FIGURE 7 — EFFECT OF UNANTICIPATED CONTRACTS ON ESTABLISHMENTS' GROWTH

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.

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.

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 B.3), the effects of contracts appear to be very persistent and survive even after the termination of the contract.[46]

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.[47] 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 will provide a comparison to the regional level estimates.

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:

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

In particular, we are interested in the effect of future shocks $\varepsilon_{i,t+\tau}$, with $\tau = 1, \dots, 4$, 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 $\tau = 1, \dots, 4$ in the left panel of Figure 7.

[46] The persistence of the effects of procurement contracts on employment is consistent with previous findings in the literature (Ferraz, Finan, and Szerman (2021) for Brazil, Lee (2024) for South Korea, and Gabriel (2024) for Portugal).

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

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

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

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 Bartik 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%.[48] 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, when we re-estimate the establishment-level effects at annual frequency—mirroring the regional specification—using the lower-quality NETS data, we obtain similar magnitudes (Appendix B.7). 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 of contractor hiring occurs within recipient establishments.

[48] 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%).

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 matched contract and employment data, we construct region-level measures of exposure to procurement shocks and break down the resulting employment response 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 $284,000 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 along the intensive margin, through expansion by 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 begins to recover over time. 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—suggesting 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 industrial capacity and 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 Bartik 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 Bartik 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 Bartik 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 Bartik 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					
	IV: Bartik Instrument				
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**

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

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 — LAUS: UNEMPLOYMENT AND LABOR FORCE

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					
	IV: Bartik Instrument				
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 Establishment-level Results

B.1. Solicitations

Contracts awarded competitively are solicited on a government website, Federal Bizz Opportunities, 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 B1 summarizes the competitive procurement timeline process.

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 B2 shows the box-plot of the (unweighted) number of days from the oldest pre-award notice to the award notice.

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

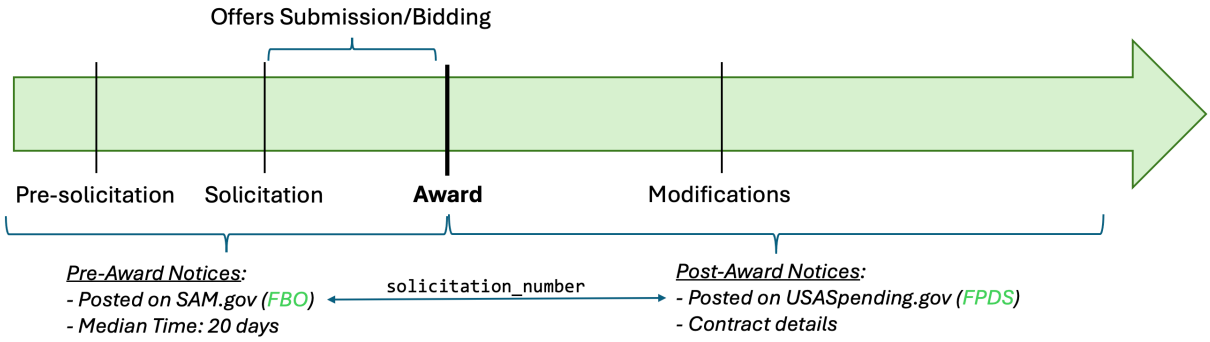


FIGURE B1 — TIMELINE OF COMPETED CONTRACTS

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.

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.

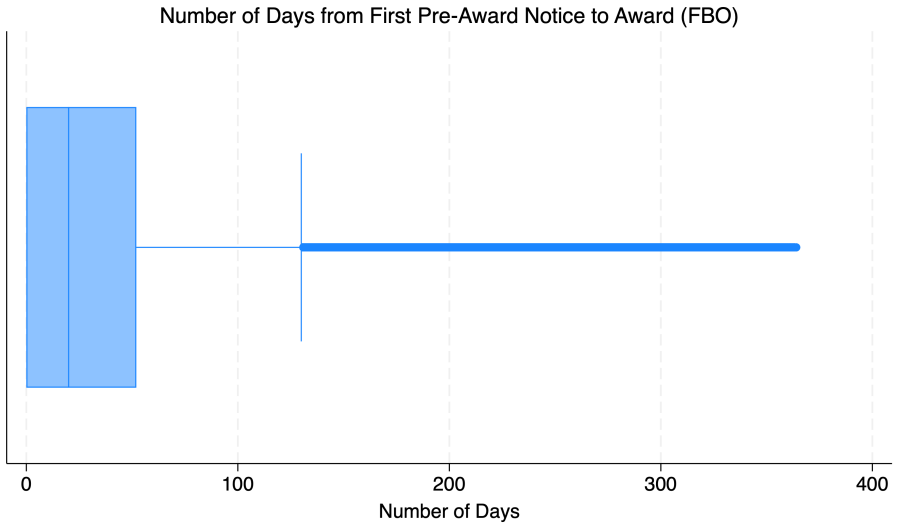


FIGURE B2 — BOX-PLOT OF NUMBER OF DAYS FROM OLDEST PRE-AWARD NOTICE TO AWARD

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.

B.2. Matched Sample

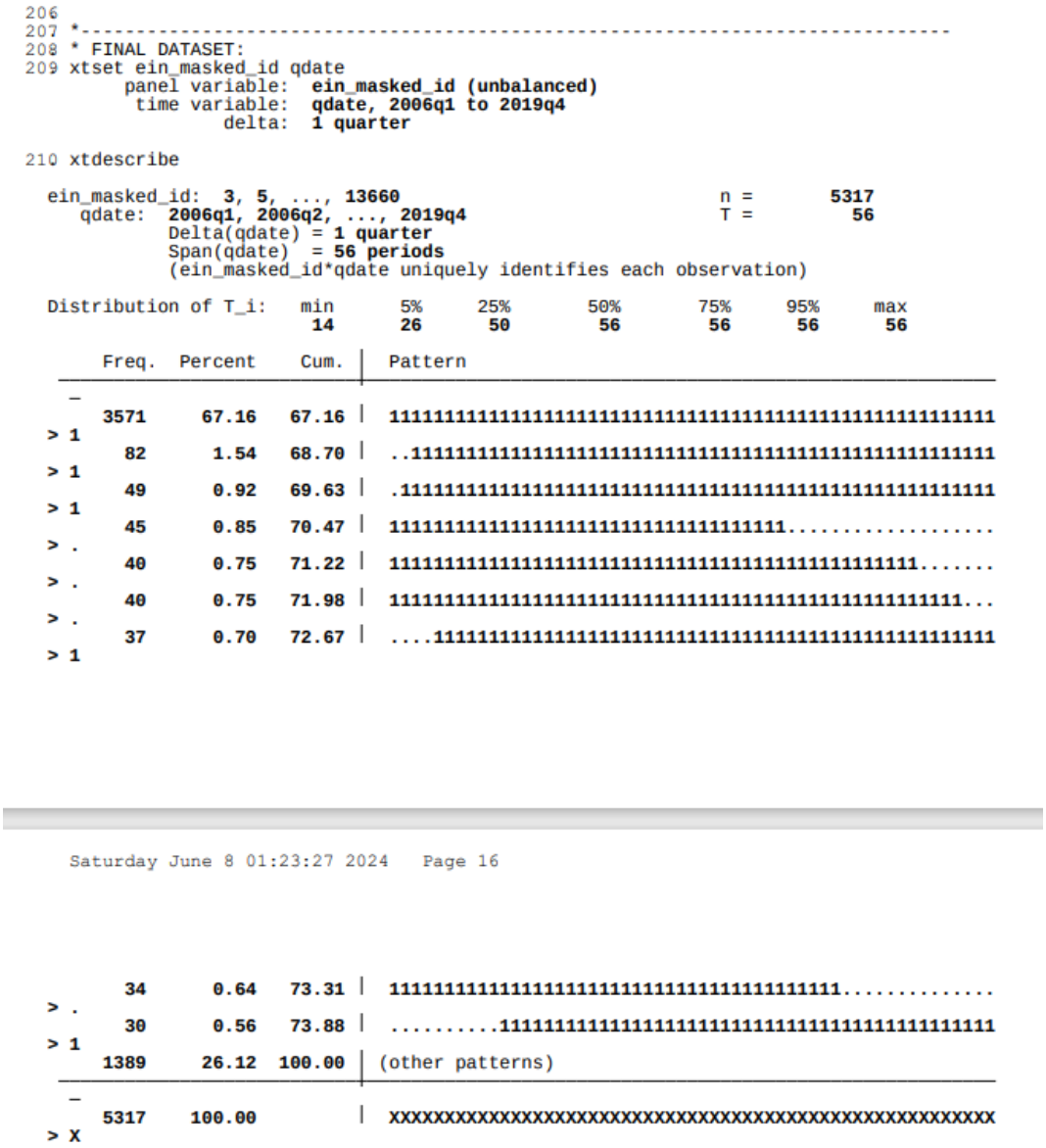
We merge contractors who receive at least one unpredictable contract with establishment-level outcomes from the QCEW.

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; and (v) removing firms with fewer than one employee on average.

The resulting dataset is an (unbalanced) panel dataset with $N = 5317$ firms observed from 2006:1 to 2019:4, $T = 56$. Figure B3 presents a screenshot from the log file of the `xtset` and `xtdescribe` commands.

Next, we calculate the distribution of unpredictable contract sizes in our final sample. Fig-



```
206
207 *-------------------------------------------------------------------------------
208 * FINAL DATASET:
209 xtset ein_masked_id qdate
        panel variable:  ein_masked_id (unbalanced)
         time variable:  qdate, 2006q1 to 2019q4
                 delta:  1 quarter

210 xtdescribe

  ein_masked_id:  3, 5, ..., 13660                        n =       5317
     qdate:  2006q1, 2006q2, ..., 2019q4                  T =         56
             Delta(qdate) = 1 quarter
             Span(qdate)  = 56 periods
             (ein_masked_id*qdate uniquely identifies each observation)

  Distribution of T_i:   min      5%     25%       50%       75%     95%     max
                          14      26      50        56        56      56      56

      Freq.  Percent    Cum. |  Pattern
 ---------------------------+-------------------------------------------------
      3571    67.16   67.16 |  11111111111111111111111111111111111111111111111111
> 1
        82     1.54   68.70 |  ..11111111111111111111111111111111111111111111111111
> 1
        49     0.92   69.63 |  .11111111111111111111111111111111111111111111111111
> 1
        45     0.85   70.47 |  1111111111111111111111111111111111....................
> .
        40     0.75   71.22 |  1111111111111111111111111111111111111111111111.......
> .
        40     0.75   71.98 |  1111111111111111111111111111111111111111111111111...
> .
        37     0.70   72.67 |  ....11111111111111111111111111111111111111111111111
> 1
```



```
        34     0.64   73.31 |  1111111111111111111111111111111111111111.............
> .
        30     0.56   73.88 |  ..........11111111111111111111111111111111111111111
> 1
      1389    26.12  100.00 |  (other patterns)
 ---------------------------+-------------------------------------------------
      5317   100.00         |  XXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXX
> X
```

FIGURE B3 — MATCHED (POLISHED) SAMPLE

Notes: The EIN is masked using a simple `egen masked_ein_id = group(ein)` command for security reasons: the BLS did not clear the output of the log file when EINs were not masked.

ure B4 shows the output of the `sum, d` command in Stata, taken from the log file. 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).

```
100 * Analyze shock distribution of matched firms:
101 sum EPSit if EPSit>0, d
```

(sum) EPSit

Percentiles		Smallest		
1%	**2263.2**	**.01**		
5%	**4660**	**37.32**		
10%	**7416**	**49**	Obs	**10,843**
25%	**25000**	**49.9**	Sum of Wgt.	**10,843**
50%	**114900**		Mean	**691194.9**
		Largest	Std. Dev.	**2728904**
75%	**399578.1**	**6.10e+07**		
90%	**1293665**	**6.19e+07**	Variance	**7.45e+12**
95%	**2894197**	**6.33e+07**	Skewness	**15.44963**
99%	**1.03e+07**	**1.14e+08**	Kurtosis	**410.5306**

FIGURE B4 — IN-SAMPLE DISTRIBUTION OF CONTRACTS/SHOCKS SIZE

B.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 B5 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 B6 shows the fraction of unpredictable contracts spent on the top ten goods categories.

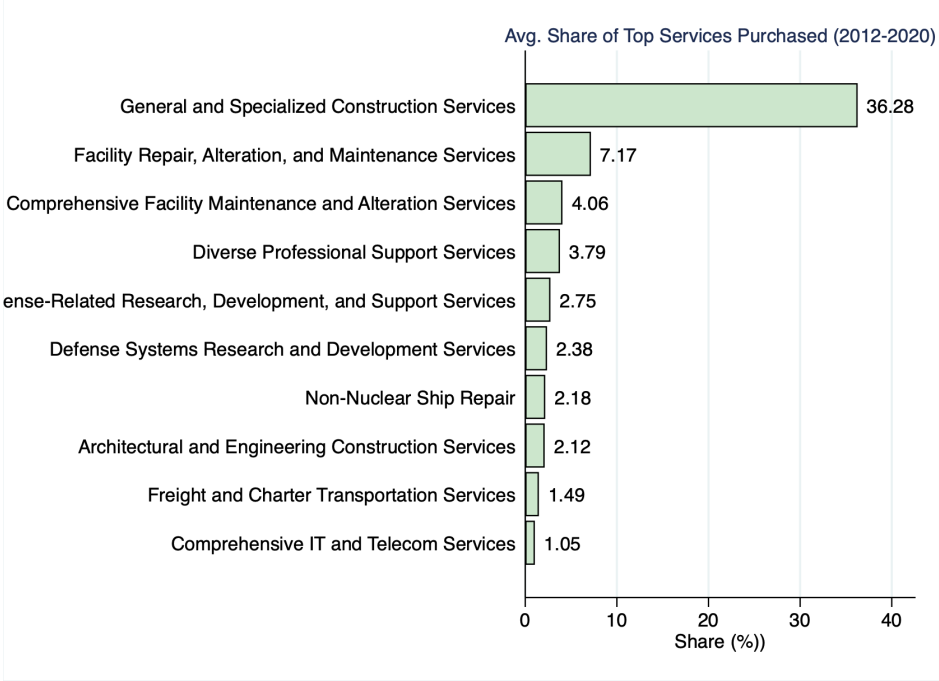


FIGURE B5 — TOP 10 SERVICES - FRACTION OF UNPREDICTABLE CONTRACTS

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

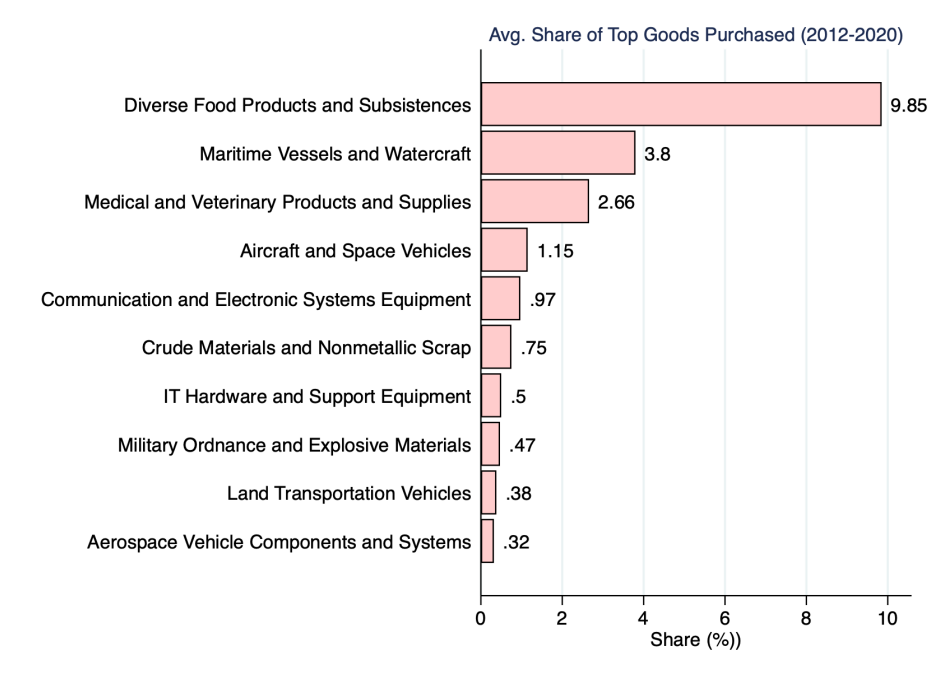


FIGURE B6 — TOP 10 GOODS - FRACTION OF UNPREDICTABLE CONTRACTS

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%).

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 B7 the box-whiskers plots of the duration (number of days) of unpredictable contracts by spending category.

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

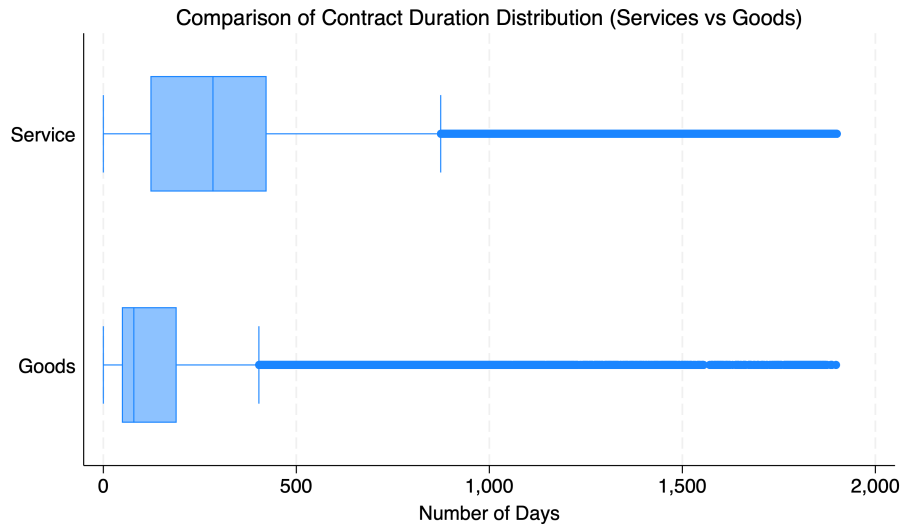


FIGURE B7 — IN-SAMPLE (UNWEIGHTED) DISTRIBUTION OF CONTRACTS' DURATION

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.

Goods vs. Services Finally, we document that contractors tend to specialize as either service providers or goods suppliers. To show this, we compute the share of each establishment's total award value accounted for by services, $G^s_{i,t}/G_{i,t}$. Values near one indicate specialization in services, while values near zero indicate specialization in goods. Figure B8 plots the distribution of establishments' average service shares, where the average is taken within establishments over fiscal years.

The distribution is strongly bimodal, with mass concentrated near 0% and 100%, indicating that establishments typically specialize as either goods suppliers or service providers. The mode at 100% (services) is higher, implying a larger number of service providers in our sample, which is consistent with the relatively large share of procurement spending on services reported in Figure B5 and Figure B6.

Accordingly, we estimate the baseline equation (8) by decomposing unanticipated awards

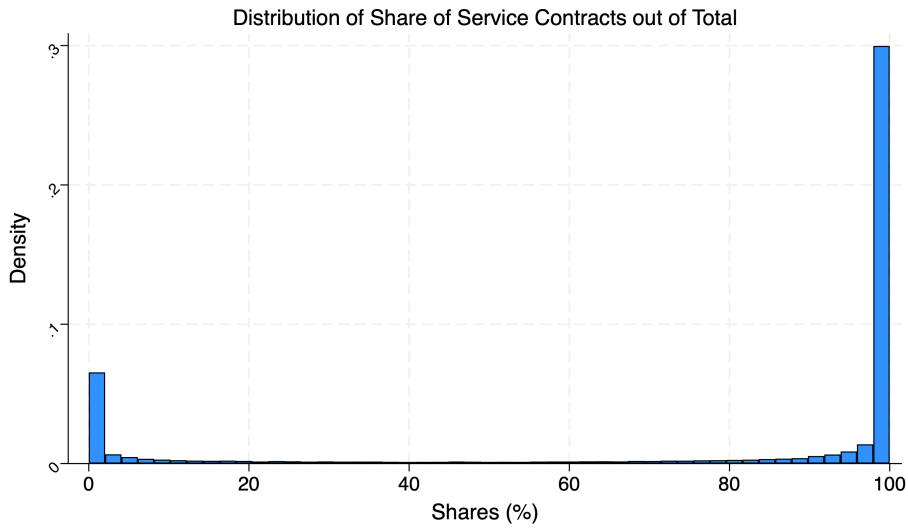


FIGURE B8 — DISTRIBUTION OF ESTABLISHMENT-LEVEL SERVICE SHARE OF AWARDS

into their services and goods components:

$$\varepsilon^{g}_{i,t} = \underbrace{\varepsilon^{g,s}_{i,t}}_{\text{Services}} + \underbrace{\varepsilon^{g,g}_{i,t}}_{\text{Goods}} .$$

This allows us to test whether the establishment-level effects of unanticipated contracts differ between service providers and goods suppliers. We report the OLS estimates in Figure B9.

Although the point estimate for goods suppliers is somewhat larger than that for service providers, the difference is not statistically significant. Given the limited variation in unanticipated goods awards, the goods estimate is relatively imprecise. Overall, we find no clear statistical difference between the establishment-level effects of unanticipated goods and services contracts. On the contrary, Muratori, Juarros, and Valderrama (2023) find that employment multipliers are higher for services than for goods contracts at the regional level. In turn, this suggests that their results are likely driven by industry input-output spillovers rather than establishment-specific features.

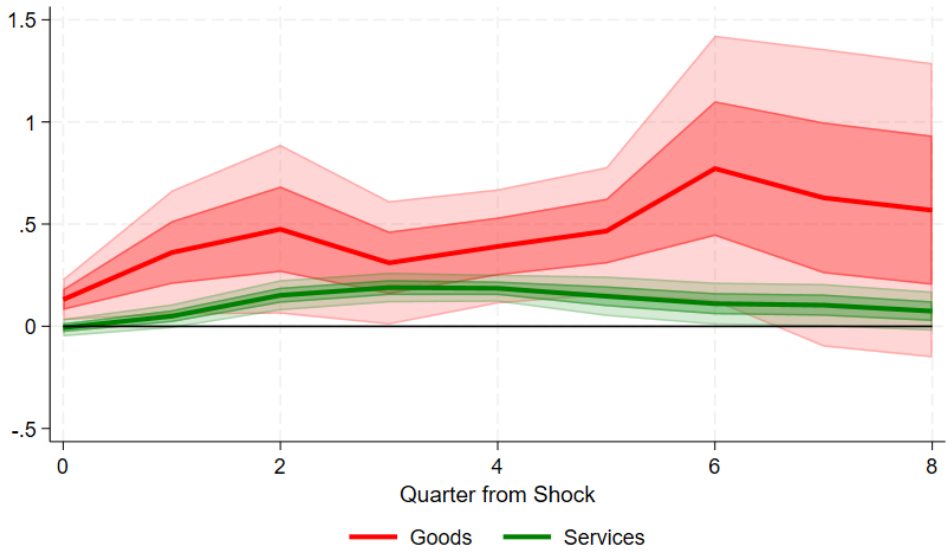


FIGURE B9 — EFFECTS OF UNANTICIPATED CONTRACTS: GOODS SUPPLIERS VS. SERVICE PROVIDERS

B.4. Analysis by Quartile of Small Establishments

We subdivide the sample of small establishments by analyzing each quartile of their size distribution separately. The size distribution of establishments in the sample is summarized in Figure B10, which reports a screenshot from the log file showing the output of the `sum, d` command in Stata.

```
180         replace large_firms_oecd = 1 if emp_pre_first_contract > 150
  (175 real changes made)

181         sum emp_pre_first_contract if large_firms_oecd == 0, d

                    emp_pre_first_contract
-------------------------------------------------------------
      Percentiles      Smallest
 1%             1             1
 5%             2             1
10%             3             1       Obs                5,142
25%      5.666667             1       Sum of Wgt.        5,142

50%            13                     Mean            21.63049
                        Largest       Std. Dev.       24.66049
75%            27      148.3333
90%            53      148.3333       Variance        608.1399
95%            76      148.6667       Skewness        2.234772
99%           120      148.6667       Kurtosis        8.522913

182
```

FIGURE B10 — IN-SAMPLE DISTRIBUTION OF NUMBER OF EMPLOYEES IN THE SAMPLE

Notes: size is measured in the quarter before the receipt of the first contract award.

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 B11 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.

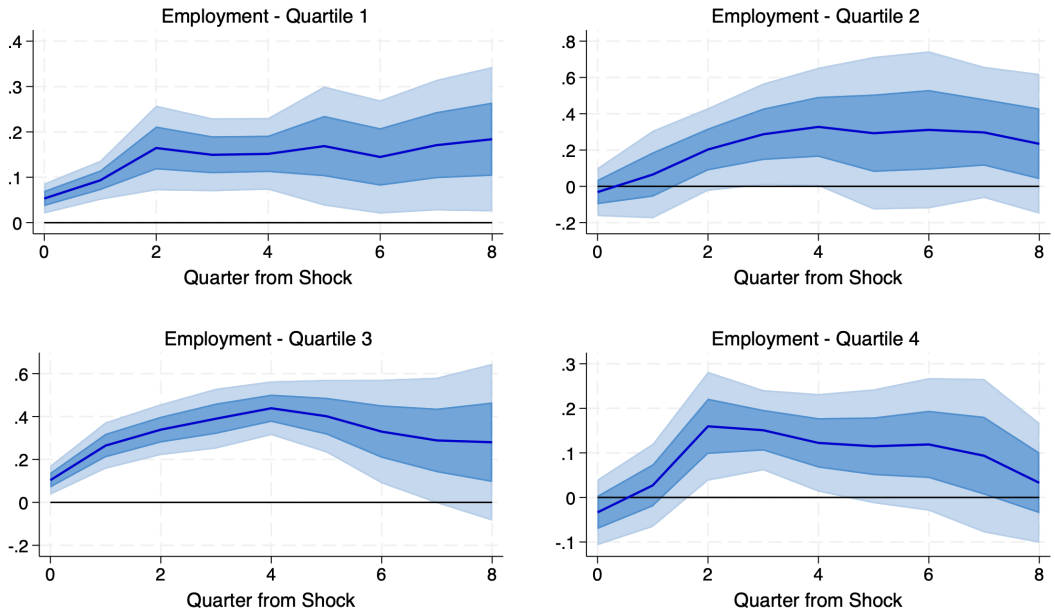


FIGURE B11 — EMPLOYMENT - QUARTILES OF SIZE DISTRIBUTION OF ESTABLISHMENTS

B.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 B12, where it is clear that the response of establishments' employment is robust to the inclusion of lags of wage-per-worker in the specification.

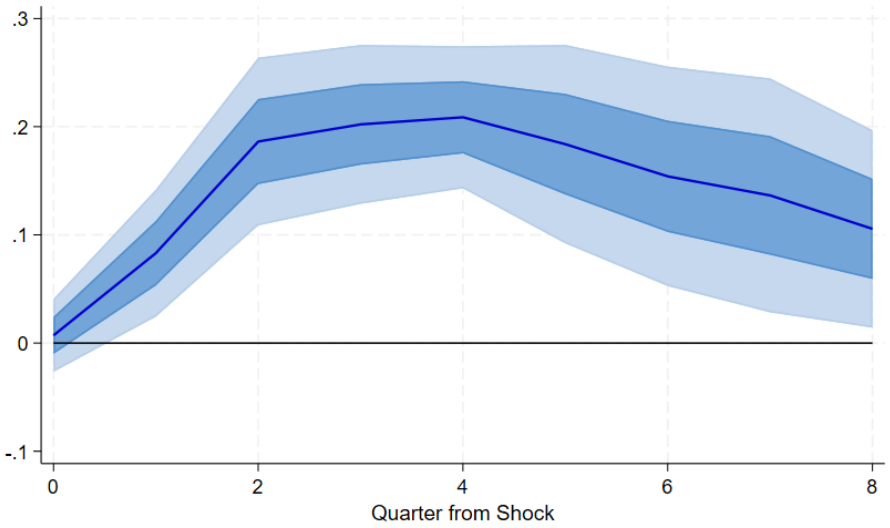


FIGURE B12 — RESPONSE OF ESTABLISHMENT'S EMPLOYMENT CONTROLLING FOR LAGS OF AVERAGE WAGE

B.6. Response of Average Wages

We study the response of the average wage, or wage-per-worker, using the same specification as Equation (8). Results are reported in Figure B13, which displays no significant effect on wage-per-worker.

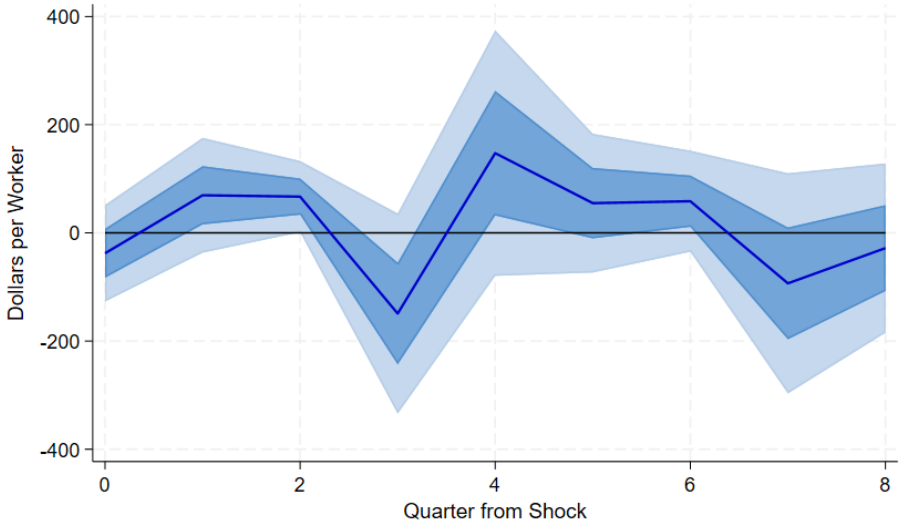


FIGURE B13 — RESPONSE OF AVERAGE WAGE

B.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).[49] 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 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

[49] 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.

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. 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 B1 presents the results.

TABLE B1 — EMPLOYMENT RESPONSE: NETS SAMPLE

Horizon	*Coefficient* (β^c_h)	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).

To validate the main results using an alternative data source, Figure B14 replicates the an-

ticipation 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 breakdown between the direct and indirect effect.

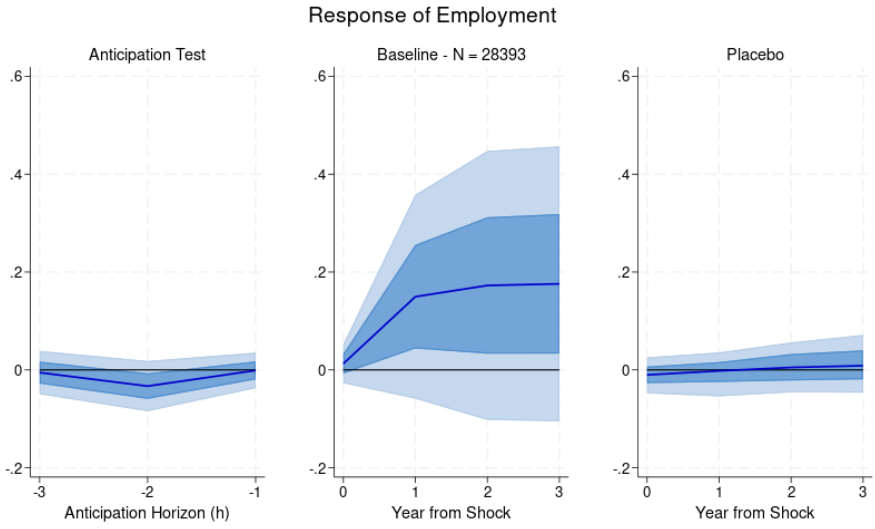


FIGURE B14 — EMPLOYMENT EFFECTS: NETS SAMPLE

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.

B.3.1 NETS Data Cleaning and Construction - for internal consumption only

We construct a panel of U.S. establishments using the National Establishment Time Series (NETS) database, following cleaning and ownership assignment procedures inspired by Choi, Penciakova, and Saffie (2023) and Crane and Decker (2019). The process is divided into two major stages: cleaning and assembling the NETS panel, and merging it with government procurement data from FPDS.

Step 1: Cleaning the NETS DUNS-Level Panel - `clean_nets_aggregate_firm.do`

1. **Load Employment Data**: Start with `nets_emp_long.dta`, retaining years 1999–2019.
2. **Merge Headquarters Information**: Merge HQ identifiers (HQDuns) to DUNS-year pairs:
   - DUNS with a unique HQ across all years are merged directly.
   - DUNS with time-varying HQs are merged year-by-year.
3. **Merge Location and Firm Characteristics**:
   - Merge CBSA, public/private status, and contractor status.
   - Merge ultimate HQ identifier (firmid) using recursive merging steps.
   - Merge ZIP code and first five address characters.
   - Merge 6-digit NAICS codes by DUNS-year.
4. **Construct Establishments (estabid)**:
   - Group DUNS by firmid, ZIP code, and address prefix.
   - If that fails, fallback to firmid-ZIP-NAICS combinations.
5. **Merge Contractor Status and Build Crosswalks**:
   - Merge contractor status from FPDS-linked dataset.
   - Generate crosswalk: DUNS-year → firmid, estabid, contractor.
6. **Assign Firm Size Buckets**:

- Classify firms into size bins based on total firm-year employment: small (1–19), medium (20–499), large (500+).

7. **Collapse to Establishment-Year Level**:
   - Aggregate all DUNS in an estabid by year.
   - Sum employment, take the first ZIP, NAICS, CBSA, etc.
   - Flag if the establishment is its own HQ.
8. **Final Cleaning Adjustments**:
   - Subtract one employee if the establishment is its own HQ (as in Crane and Decker, 2019).
   - Drop establishments with zero employment.
   - Generate NAICS2/NAICS3 and flag sector indicators (e.g., Manufacturing, Education, Public Admin).
9. **Save Final Panel**:
   - Store cleaned establishment panel as `estabid_panel_clean.dta`.

Step 2: Merging NETS with FPDS Procurement Data - `Regression_Analysis_NETS.do`

1. **Collapse FPDS to Annual Level**:
   - Collapse quarterly contract data to yearly aggregates by DUNS.
   - Create three files covering 2000–09, 2010–14, and 2015–20.
2. **Merge FPDS with NETS Crosswalk**:
   - Use the DUNS-year to firmid/estabid crosswalk.
   - Retain only matched observations.
3. **Collapse to firmid and estabid Levels**:
   - Aggregate FPDS data to firmid-year and estabid-year.
4. **Filter NETS Panel to FPDS-Matched Establishments**:

- Filter `estabid_panel_clean.dta` to only those with FPDS matches.
- Merge procurement variables into NETS panel.
- Replace missing procurement variables with zero.

5. **Deflate Procurement Values**:
   - Adjust `git` and `EPSit` variables using a price index (base year: 2008).
6. **Final Variable Generation**:
   - Generate state and industry fixed effect groupings.
   - Normalize identifiers (e.g., rename `estabid` to `ein_masked_id`).
   - Compute total firm employment across establishments.
7. **Save Final Analysis Datasets**:
   - Save datasets for analysis at both the firm and establishment level:
     - `Analysis_NETS_Micro.dta` - using `estabid_panel_clean.dta` from Step 1 - used for our analysis
     - `Analysis_NETS_Micro_allestabid.dta` - using all establishments (not cleaned) from Step 1
     - `Analysis_NETS_firmid.dta` - aggregating at the firm instead of establishment level

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