

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

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This research was carried out with restricted access to the Bureau of Labor Statistics (BLS) data. The views expressed here do not reflect the views of the BLS or the U.S. government.

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RESEARCH QUESTION

Building on the empirical literature leveraging defense spending Bartik shocks (Nakamura and Steinsson, [2014](#); Auerbach et al., [2020](#)), we break down regional (MSA) employment multipliers, asking:

- Does reducing constraints at small firms constitute an important employment channel?
 - For time, not presenting today
- Are MSA-level employment effects mostly direct (contractors) or indirect (others)?
- What are the direct employment effects on contractor establishments themselves?

→ Micro-level data necessary to answer these questions

LITERATURE REVIEW

- **Regional-level effects in the U.S.:**

- Nakamura and Steinsson (2014), Dupor and Guerrero (2017), Demyanyk et al. (2019), Auerbach et al. (2020), Juarros (2022), Muratori et al. (2023).

- **Firm-level effects in the U.S.:**

- **Compustat:** Hebous and Zimmermann (2020) (investment); Budrys (2022) (investment, sales, profits)
- **Orbis:** Juarros (2022) (revenues, credit, investment of *non*-contractors)
- **NETS** (employment): Barrot and Nanda (2023) (Quick-Payments); Choi et al. (2023) (Political Connections).

- **Firm-level effects outside U.S.:**

- Gugler et al. (2020) (Austria), Ferraz et al. (2021) (Brazil), Gabriel (2024) (Portugal), di Giovanni et al. (2023) (Spain), Lee (2024) (Korea).

CONTRIBUTIONS TO THE LITERATURE

- **Regional Multipliers and Spillovers**

- ***Small*** vs. ***medium*** vs ***large*** firms' employment (not today)
 - Large firms account for about 80% of the local multiplier, similar share of spending
 - No clear evidence of dramatically larger hiring per dollar among small firms
- ***Contractor*** vs. ***local spillover*** employment.
 - Majority (60%) of employment gains at contractors
 - Local spillovers slightly negative (insignificant) on impact, then grow

- **Establishment-Level Evidence in the U.S.**

- Construct of a new set of **unanticipated contracts**.
- Establishment-level estimates of effects of procurement contracts (i.e., **direct effect**).
 - Persistent positive direct impacts on employment
 - Establishment-level impacts small, roughly $\frac{1}{6}$ of total effect on contractors

CONTRACTORS VS. NON-CONTRACTORS: NEED FOR MICRODATA

- What share of regional employment impact comes from contractor firms?
 - Auerbach et al., [2020](#) look at other *industries*, we look at other *firms*
- Requires identifying, summing contractor firm employment (not publicly available)
- Obtained access to BLS LDBE establishment microdata (basis of QCEW)
 - Limited to 42 states + DC granting access
- Match FPDS to LDBE by cleaned establishment name and location (typically county)
 - Computationally very intensive!

METHODOLOGY

- Estimate regional multipliers with standard approach (Auerbach et al., 2020)
- Baseline regression equation (Jordà (2005) local projections):

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

- Identification: Shift-share design (**Bartik instrument**):

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

- Focus on **defense** contracts $D_{\ell,t}$ (*exogeneity*)

- **Data:**

- FPDS (universe of federal contracts) gives $D_{\ell,t}$, s_{ℓ} , D_t
- BLS LDBE (universe of employment counts in covered states) gives $E_{\ell,t}$
- Matched FPDS + LDBE gives $E_{\ell,t}^{\text{Contractors}}$, $E_{\ell,t}^{\text{Non-Contractors}}$
- Baseline **sample**: 2006-2019 - 262 MSAs

REGIONAL MULTIPLIERS IN LDBE SAMPLE

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

Notes: Estimates obtained using restricted data from the BLS on private employment (left panel) and the public BEA data on total employment (right panel). Sample period restricted to match data availability from the BLS: 2006–2019; 262 MSAs (42 states + DC)

- Multipliers from BLS micro data consistent with estimates from public data
- Estimates smaller but similar in magnitude to AGM20 (different sample)
- **High cost per job:** Contractors hire highly skilled workers (Bartal and Becard, 2024)

CROWDING-IN AFTER INITIAL YEAR

- We aggregate QCEW data by regions and years:

$$E_{\ell,t} = \underbrace{E_{\ell,t}^{\text{Contractors}}}_{\text{Matched Firms}} + E_{\ell,t}^{\text{Non-Contractors}}$$

<i>Horizon</i>	<i>Contractors</i>			<i>Non Contractors</i>		
	<i>Coefficient</i> (β_h^c)	<i>p</i>	<i>Fraction</i>	<i>Coefficient</i> (β_h^{nc})	<i>p</i>	<i>Fraction</i>
<i>impact</i>	0.036 (0.017)	0.040	306.8%	-0.024 (0.019)	0.205	-206.8%
<i>1 year</i>	0.063 (0.029)	0.032	64.4%	0.035 (0.016)	0.028	35.6%
<i>2 years</i>	0.052 (0.026)	0.044	51.4%	0.049 (0.023)	0.034	48.6%
<i>3 years</i>	0.056 (0.031)	0.074	47.8%	0.061 (0.029)	0.037	52.2%

Notes: Sample period 2006–2019; 262 MSAs.

WHY LOOK AT ESTABLISHMENT LEVEL?

- Contractor employment gains reflect direct hiring impact *plus* spillovers (e.g., input-output linkages, subcontracting, local demand).
- **Isolating the direct channel:**
 - Are recipient establishments actually expanding their payroll?
 - Or are contractor effects largely driven by input-output linkages and subcontracting?
- **Lower bound:** Establishment-level estimates give us a **conservative benchmark** for the strength of the direct effect
- **Key contribution:** Quantify how much of the employment multiplier operates through **recipient establishments** themselves

ESTABLISHMENT-LEVEL IDENTIFICATION CHALLENGES

Defense spending shocks exogenous at MSA level, but individual contracts not exogenous:

1. Timing/Anticipation

- E.g. Government buys firm-specific new products (i.e., sole sourcing)
- E.g. Contract is a modification or a child contract of a larger parent contract
- Use **institutional knowledge** to identify **unanticipated** contracts

2. Selection into treatment

- E.g. More productive firms may win more contracts
- Control for firm FE and limit sample to firms that won at least 1 unanticipated contract

3. Reverse causality

- E.g. More contracts may be awarded during regional/sectoral booms
- Control for time-industry and time-state fixed effects

UNANTICIPATED CONTRACTS MEET FOUR CONDITIONS

- Building on Hebous and Zimmermann, 2020 and Budrys, 2022, break down total value of contracts:

$$G_{i,t} = \tilde{G}_{i,t} + \varepsilon_{i,t}$$

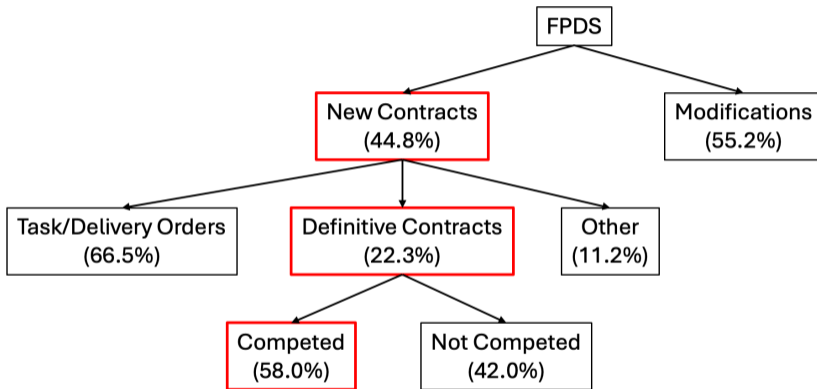
- $\varepsilon_{i,t}$: dollar value of **unanticipated** contracts.
- $\tilde{G}_{i,t}$: dollar value of **potentially anticipated** contracts.

UNANTICIPATED CONTRACTS:

We label contracts as "unanticipated" if they are:

- I. competed (Hebous and Zimmermann (2020))
- II. at least two bidders (Hebous and Zimmermann (2020))
- III. newly awarded (Budrys (2022))
- IV. standalone ("definitive") contracts (New)

ONLY 6% OF CONTRACTS ARE UNANTICIPATED



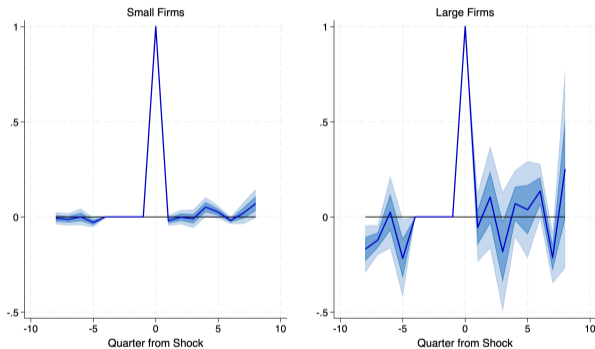
Notes: Data refers to averages over fiscal year shares. Fiscal years span from 2006 to 2019 to maintain consistency with the rest of the analysis in this paper. Shares refer to contract values, not the number of contracts (i.e., dollar-weighted shares).

EMPIRICAL SPECIFICATION

$$\begin{aligned}
 E_{i,t+h} - E_{i,t-1} &= \beta^h \cdot \underbrace{\varepsilon_{i,t}}_{\text{Shock}_t} + \gamma^h \cdot \tilde{G}_{i,t} + \dots \\
 &\dots + \underbrace{\sum_{j=1}^4 \left\{ \rho_j^h \cdot \varepsilon_{i,t-j} + \gamma_j^h \cdot \tilde{G}_{i,t-j} + \phi_j^h \cdot (E_{i,t-j} - E_{i,t-1-j}) \right\}}_{\text{Lags (Control for serial correlation and pre-trends)}} + \dots \\
 &\dots + \underbrace{\alpha_i^h + \alpha_{s,t}^h + \alpha_{\ell,t}^h}_{\text{Fixed Effects}} + \varepsilon_{i,t+h} \quad h = 0, 1, \dots, H \tag{1}
 \end{aligned}$$

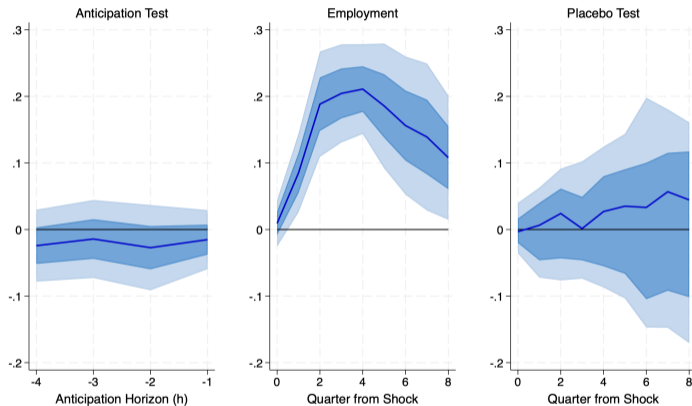
- $E_{i,t}$ is **Employment** of firm i in quarter t
- **Fixed Effects:** α_i^h firm; $\alpha_{s,t}^h$ sector-time; $\alpha_{\ell,t}^h$: state-time.
- Addition of **lags** is key to control for dynamic effects (e.g. past realizations of shocks).

UNANTICIPATED SHOCK: MOSTLY ONE-TIME SHOCK



- Estimate Lag-augmented LP of $G_{i,t}$ on $\varepsilon_{i,t}$ **Econometric Specification**
- $\varepsilon_{i,t}$ accounts for virtually all $G_{i,t}$ at the shock date
- Small impacts of unanticipated contract on future $G_{i,t}$
- Budrys (2022) shows competed DoD contracts lead to abnormal returns (unanticipated by markets)
- **Real Example: Small Service Provider**

ESTABLISHMENT-LEVEL EMPLOYMENT EFFECTS



Notes: Establishments are observed from 2006:1 to 2019:4, i.e., $T = 56$. The number of establishments is $N = 5,142$. Standard errors are clustered at the state level. Small bands represent 68% confidence intervals, and large bands represent 95% confidence intervals.

DIRECT SHARE OF REGIONAL IMPACTS

Cumulating employment responses up to 2 years (8 quarters) after the shock:

$$\frac{\text{Direct Effect on Contractors (Establishment Level Estimates)}}{\text{Total Effects on Contractors (Regional Level Estimates)}} \approx \frac{0.32 \frac{\text{job-years}}{\$1\text{M}}}{0.78 \times 2.836 \frac{\text{job-years}}{\$1\text{M}}} \approx 14.5\%$$

Why so small? Possibly:

- Occasional contractors remain "contractors" but really capture spillovers
- Non-recipient current contractors benefiting from demand spillovers
- Subcontracting or within-firm (outside establishment) spillovers

CONCLUSION

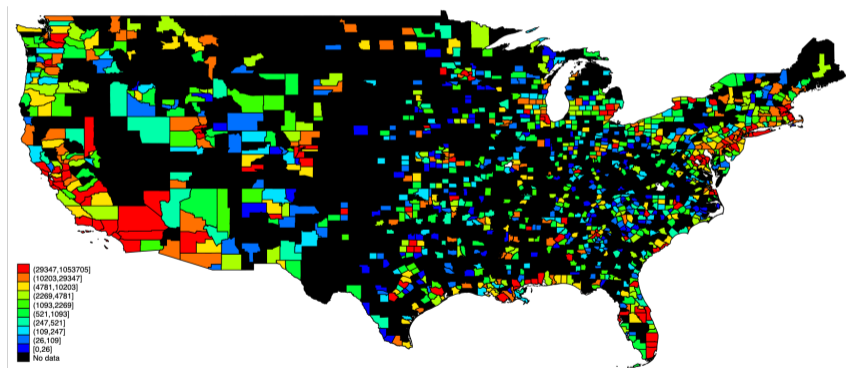
- **Small firms do not play an outsized role in employment gains**
 - Large majority ($\approx 80\%$) of spending & emp. growth at large firms
- **Regional employment multipliers are positive but concentrated**
 - 60% of job gains come from **contractors**; spillovers grow over time
- **Establishment-level data reveals only a fraction of contractor impacts are direct**
 - Direct contract recipients increase employment
 - Direct impacts small: $\approx 15\%$ of **contractor job gains** are direct establishment hiring
- In summary:
 - Procurement generates local employment, but cost-per-job is high
 - Large firms dominate spending and employment creation
 - Contractors hire immediately, by 2 years out non-contractor impact just as large
 - Direct establishment effect very small, suggesting large spillovers among contractors

Questions?

Thank You!

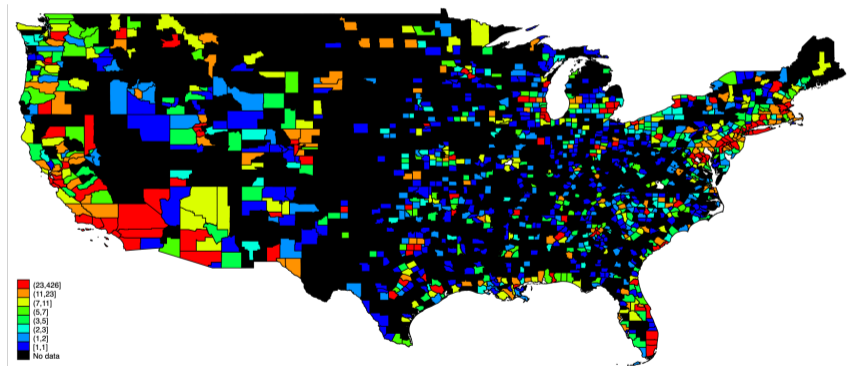
SPATIAL DISTRIBUTION [BACK](#)

FIGURE: AVERAGE \$ VALUE OF CONTRACTS 2016-18 (THOUSANDS)

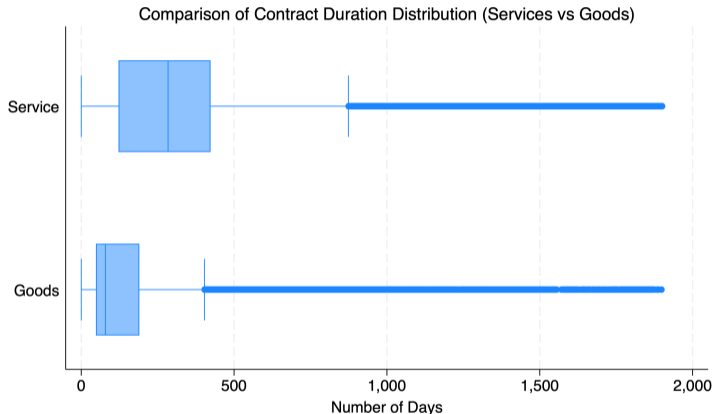


SPATIAL DISTRIBUTION [BACK](#)

FIGURE: AVERAGE # OF CONTRACTS 2016-18



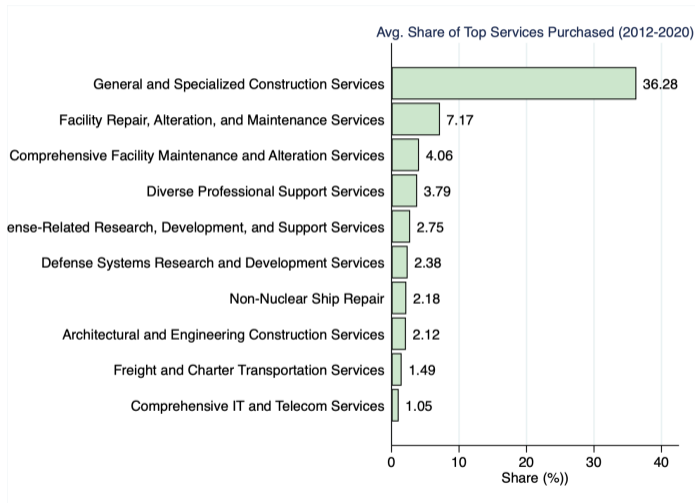
UNANTICIPATED CONTRACTS DURATION

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- **Services:** median 283 days - interquartile range [121,423] days.
- **Goods:** median 79 days - interquartile range [48-190] days.

TOP SERVICES PURCHASED

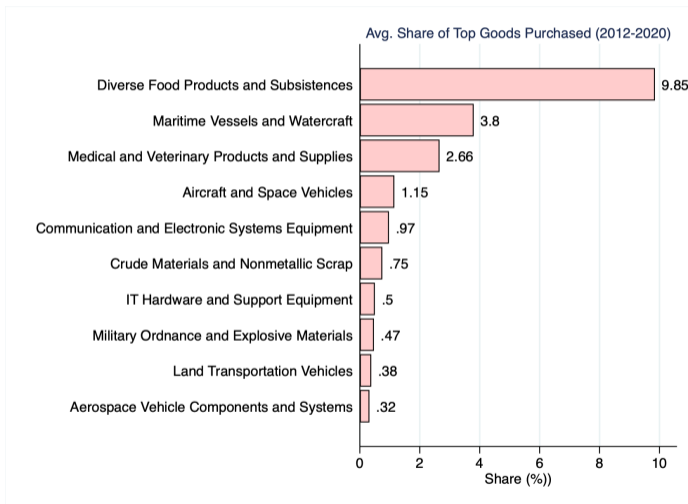
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Top Goods Purchased

TOP GOODS PURCHASED

BACK



Top Services Purchased

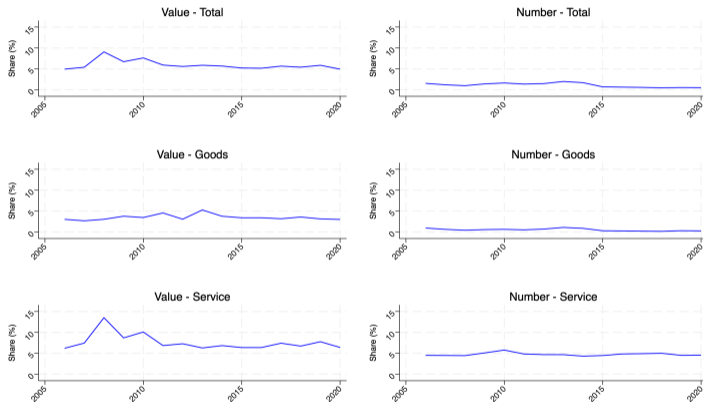
UNANTICIPATED SHOCK: MOSTLY ONE-TIME SHOCK

Specification:

$$\begin{aligned}
 G_{i,t+h} - G_{i,t-1} &= \underbrace{\lambda_i^h + \lambda_{s,t}^h + \lambda_{\ell,t}^h}_{\text{Fixed Effects}} + \underbrace{\gamma_0^h \cdot \varepsilon_{i,t}}_{\text{Unantic. Contracts}} + \dots \\
 &\dots + \underbrace{\sum_{j=1}^4 (\gamma_j^h \cdot z_{i,t-j} + \phi_j^h \cdot (G_{i,t-j} - G_{i,t-j-1}))}_{\text{Lags}} + \dots \\
 &\dots + u_{i,t+h} \quad h = 0, 1, \dots, H
 \end{aligned}$$

Back to Presentation

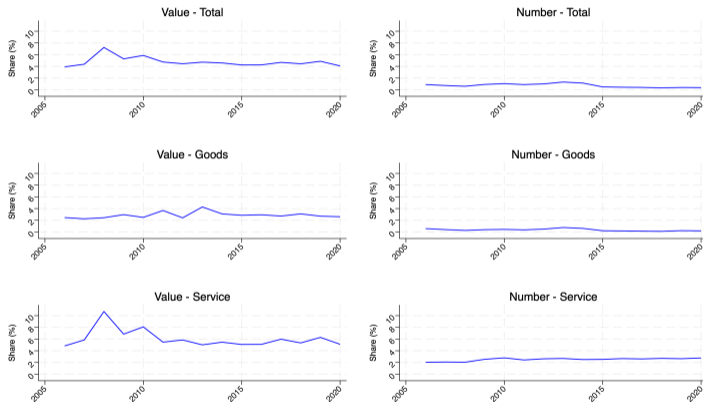
HOW MANY CONTRACTS ARE UNANTICIPATED?



SHARE OF NUMBER/VALUE OF UNANTICIPATED CONTRACTS AND BREAKDOWN INTO SERVICE AND GOODS ACROSS FIRMS WHICH RECEIVE AT LEAST ONE SHOCK. Across all G

[Back to Presentation](#)

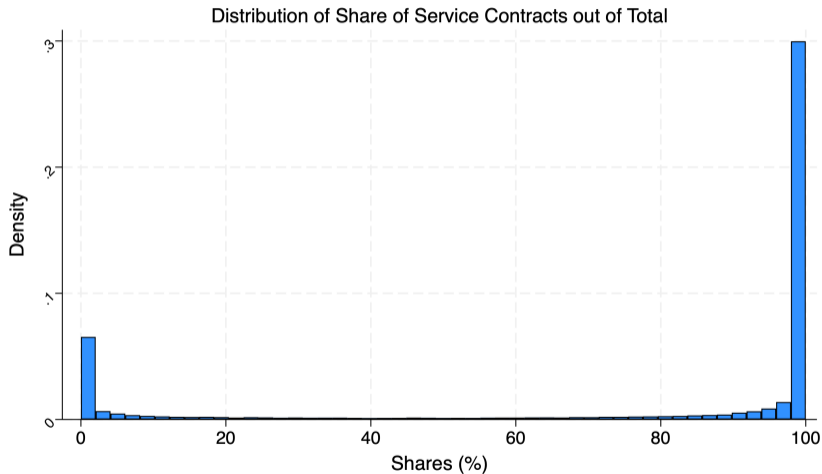
HOW MANY CONTRACTS ARE UNANTICIPATED?



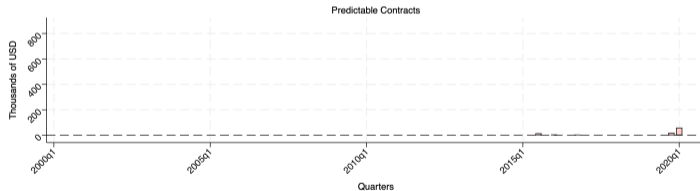
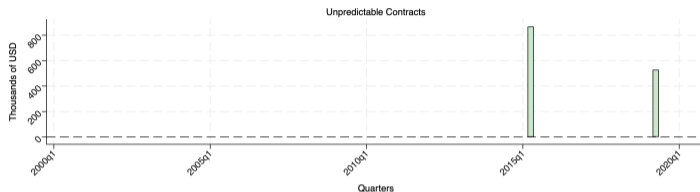
SHARE OF UNANTICIPATED CONTRACTS OUT OF TOTAL

Only Across Shocked Firms

SHARE OF SERVICE CONTRACTS OUT OF TOTAL

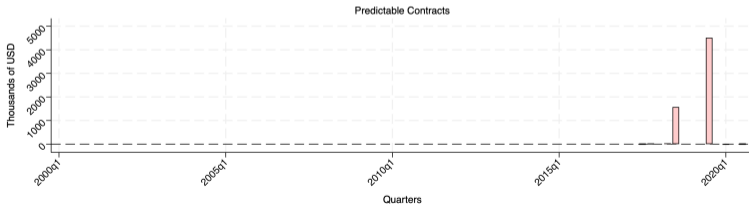
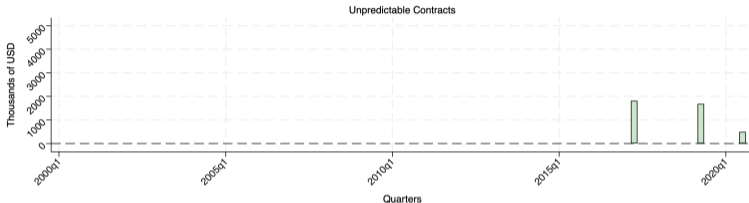


EXAMPLE - SMALL SERVICE PROVIDER

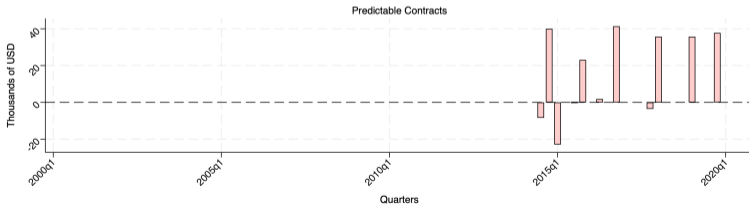
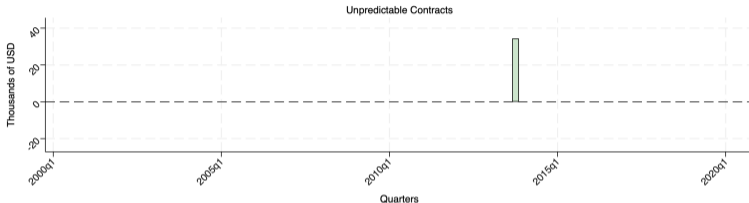
[BACK](#)

[Large Service Provider](#)
[Small Goods Supplier](#)
[Large Goods Supplier](#)

Fixed random seed + random draw from representative firms (number of quarters with predictable and non-predictable contracts within interquartile range).

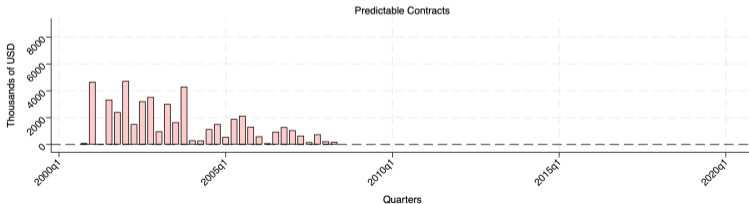
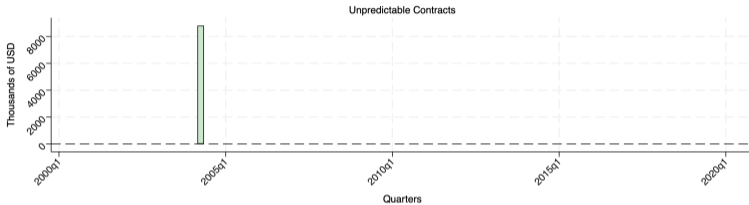
EXAMPLE - LARGE SERVICE PROVIDER

[BACK](#)

EXAMPLE - SMALL GOODS SUPPLIER

[BACK](#)

EXAMPLE - LARGE GOODS SUPPLIER

[BACK](#)

SAMPLE RESTRICTIONS BACK

- Before matching Restrictions:

- Exclude firms with multiple locations within state (i.e. single establishment).
Innocuous since most EIN have one establishment within a state.
- Establishments with average # of employees < 1.

→ **13.662 matches.**

- After Matching Restrictions:

- Exclude firms with gaps in the time series: 10,651 matches.
- Trim sample. Exclude firms with:

- $T_i < 1 + \underbrace{\text{Number of lags}}_{=4} + \underbrace{\text{IRF Horizon}}_{=8} = 13.$

- First contract awarded in the first four periods (preserve all data for all lags).
- First contract awarded before eight quarters from the end of the time-series (preserve data at all all horizons of IRF)

→ Sample: **5,349 matches.**

EXTERNAL VALIDITY BACK

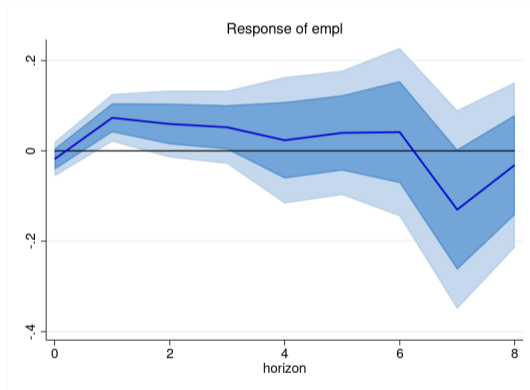
$$Y_{i,\ell,s} = \lambda_{\ell} + \alpha_s + \beta \cdot D_{i,\ell,s}^{\text{In the Sample}} + u_{i,\ell,s}$$

Outcome Firm Size	Employment		Total Wages		Average Wage	
	Small	Large	Small	Large	Small	Large
(In the Sample)	32.19*** (0.36)	-47.55 (118.99)	468,375.6*** (6,938.2)	-1,263,486 (1,711,001)	645.43 (1,586.65)	-484.41 (345.36)
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
N	7,814,549	371,935	7,814,549	371,935	7,814,549	371,935

Notes: Standard errors are reported in parentheses below the OLS estimates. *** denotes 99% significance level. Small firms have an average number of employees below 150. Data are sample averages for years 2010 through 2015. Firms in the sample represent the universe of US private firms with addresses in one of the 43 signatory states we have access to.

LP: EMPLOYMENT

- Regression run in levels
- Estimate dominated by (muted) response of largest firms



EFFECTS OF \$1M SHOCK ON NUMBER OF EMPLOYEES

CONTROLLING FOR PRODUCTIVITY SHOCKS & WAGES

Time-Varying Productivity:

- Include lags of wage-per-worker in the specification to capture short-run productivity changes.
- Results remain robust: no large bias from transitory firm-specific productivity.

Response of Wages:

- *No significant effect* on average wage ($\bar{w}_{i,t}$) for small or large firms.
- Employment rises \implies total wage bill ($W_{i,t} = \bar{w}_{i,t} \times E_{i,t}$) also \uparrow .
- Mechanisms: possibly learning-by-doing or sticky wages.

(Full details and additional tables in Appendix E.2 & E.3.)

