Defense Procurement and Local Wage Spillovers: Compositional Evidence from the Post-2022 U.S. Emergency Supplementals*

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Abstract

We assess local labor market responses to the post-March 2022 U.S. emergency defense procurement supplementals using a matched 61-county panel (2021Q1–2025Q3) from BLS QCEW, linked to Census QWI for worker-flow evidence. Counties with high procurement intensity exhibit a modest rise in non-manufacturing average weekly wages: about 0.34% (or $3.40 per week) at the median treated-county intensity (TWFE $\beta = +0.0006$; Wild Cluster Bootstrap $p = 0.081$; permutation $p = 0.001$). The pattern in QWI worker-flow data is consistent with a compositional channel: new-hire earnings rise in non-manufacturing and fall in manufacturing, while stable-employee earnings change little. Because the QWI data are aggregated at the county-by-sector level, however, they cannot fully separate compositional entry from changes in occupation mix, hours, or establishment composition. The headline wage spillover survives a pre-COVID placebo, recovery-trajectory controls, Honest-DiD sensitivity analysis, intensity winsorization, sector falsification, and same-sample 2SLS estimation with two predetermined defense-industrial-base instruments. Because pre-trends are formally rejected in our 2021–2025 window, identification relies on these sensitivity checks rather than on unconditional parallel trends. We find no robust effect on manufacturing employment or wages.

Keywords: Defense procurement, local multipliers, wage spillovers, local projections, cluster-robust inference.
JEL Codes: E62, J23, J31, R12.

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1 Introduction

Localized fiscal shocks are a workhorse for estimating regional fiscal multipliers (Nakamura and Steinsson, 2014; Chodorow-Reich, 2019) and spatial-equilibrium wage dynamics (Moretti, 2010). The cross-sectional multipliers literature predicts employment expansion in directly shocked counties; the local-spillovers literature predicts upward pressure on non-traded wages through demand spillovers. We assess the wage-spillover prediction using the natural experiment created by the post-March 2022 U.S. Congressional emergency military supplementals.

These supplementals (defense allocations for Ukraine and Israel under Disaster Emergency Fund Codes 3, 6, `AAA`, `AAB`, `AAJ`) directed tens of billions of dollars to historically predetermined defense-industrial hubs, mitigating concerns about contemporaneous endogenous site selection. We construct a matched county-quarter panel of 61 counties (11 ex-ante-identified defense-procurement hubs and 50 economically similar adjacent or same-state controls; 1,132 county-quarter observations in the 2021Q1–2025Q3 estimation window) from the BLS Quarterly Census of Employment and Wages (QCEW).

The headline result is a modest rise in non-manufacturing average weekly wages in counties with high realized procurement intensity, present in the static pooled estimate and in the dynamic LP estimates at the contemporaneous and one-year horizons. Census Quarterly Workforce Indicators (QWI) decompose this spillover into changes affecting new hires versus full-quarter stable employees; the evidence is consistent with a workforce-entry composition channel rather than within-job wage growth, though aggregate county-by-sector data cannot fully identify the mechanism. The headline survives a battery of checks—a pre-COVID placebo on 2017–2019 data, controls for differential 2021 recovery trajectories, Honest-DiD sensitivity bounds (Rambachan and Roth, 2023), intensity winsorization, sector falsification on Construction (NAICS 23) and Education (NAICS 61), and a two-instrument predetermined-defense-industrial-base IV with a joint first-stage F of 30.4–31.5 for the headline outcome. We do not find a robust effect on manufacturing employment or wages.[1]

2 Data and Empirical Framework

We construct a quarterly county panel spanning 2021Q1–2025Q3 from BLS QCEW. The continuous treatment variable, Intensity_c, is a county-fixed, time-invariant measure: the cumulative dollar value (over the full sample window) of place-of-performance military contract obligations under the emergency supplemental DEFC codes, normalized by 2021Q1 baseline private-sector employment (in millions USD per 1,000 baseline employees) and constructed from USAspending. Eleven counties are identified ex ante as defense-procurement hubs using a > \$1.0M-per-1,000-employees threshold; eight realize strictly positive obligations during the sample (intensity range 0.42 to 89.4); the remaining three obtain zero realized obligations and thus contribute identically to control units in the continuous-treatment coefficient. We match each treated county to adjacent or same-state controls via Mahalanobis distance on 2021Q1 baseline total employment and manufacturing share within

[1] Wild-bootstrap-significant negative manufacturing-employment coefficients at h = 1–3 fail to reject under county-intensity permutation ($p_{\text{perm}} \geq 0.44$), fail the Honest-DiD sensitivity bound ($M^\star = 0$, since the naive 95% CI already contains zero), and shrink by about 50% when the two highest-intensity counties (Churchill NV, intensity 89.4; Ouachita AR, intensity 39.9) are dropped. We report point estimates in Table 2 for completeness but do not interpret them as a robust local-multiplier finding.

the same BEA Economic Area, expanding the donor pool to 50 controls for $G = 61$ clusters. The non-manufacturing aggregate is constructed at the county-quarter level as the QCEW total-private-minus-manufacturing residual.

To identify the mechanism we pull Census QWI for the same counties (2021Q1–2025Q3), providing quarterly county-by-NAICS-sector measures of full-quarter stable-employee earnings (EarnS), new-hire earnings (EarnHirNS), end-of-quarter hiring rates, beginning-of-quarter separation rates, and stable turnover. We use the manufacturing (NAICS 31–33) and total-private (NAICS 00) cuts. Disclosure suppression in the QWI county-by-sector cells affects 5.6% of earnings and worker-flow observations in our panel; suppressed cells are dropped from the corresponding regressions. Table 1 reports QCEW baseline balance.

Table 1: Pre-Treatment Baseline Balance (2021Q1)

Variable	Treated Mean	Treated SD	Control Mean	Control SD	SMD
Total Private Employment	206,270	252,448	134,531	175,213	0.347
Manufacturing Employment	17,311	20,999	15,481	16,732	0.099
Avg Mfg Weekly Wage ($)	1,475	499	1,372	490	0.209
Avg Non-Mfg Weekly Wage ($)	1,124	444	994	328	0.339
Manufacturing Share of Emp	0.118	0.075	0.135	0.117	−0.171

Note: SMD $= (\bar{X}_T - \bar{X}_C)/\sqrt{(s_T^2 + s_C^2)/2}$. Level differences are absorbed by county fixed effects in (1)–(2).

We estimate two complementary specifications. The static Two-Way Fixed Effects (TWFE) model captures the post-period average effect,

$$Y_{c,t} = \alpha_c + \gamma_t + \beta\left(\text{Intensity}_c \cdot \text{Post}_t\right) + \varepsilon_{c,t}, \tag{1}$$

where Intensity_c is the county-fixed dollar measure described above and $\text{Post}_t = \mathbf{1}\{t \geq 2022\text{Q2}\}$. We trace dynamic adjustment with Jordà (2005) Panel Local Projections (LP) with two lags:

$$Y_{c,t+h} = \alpha_c^h + \gamma_t^h + \beta^h\left(\text{Intensity}_c \cdot \text{Post}_t\right) + \sum_{p=1}^{2} \phi_p^h\, Y_{c,t-p} + u_{c,t+h}, \quad h \in \{0,1,2,3,4\}. \tag{2}$$

Inference is layered: (i) conventional county-clustered standard errors; (ii) restricted Wild Cluster Bootstrap (WCB) with Rademacher weights ($B = 1{,}000$); (iii) county-intensity permutation ($B = 1{,}000$). With $G = 61$ clusters the WCB approximates the null distribution well (MacKinnon and Webb, 2018); we treat it as the preferred inference procedure. We complement these with Romano–Wolf step-down adjusted p-values (Cameron and Miller, 2015) across LP horizons and with Honest-DiD breakdown values (Rambachan and Roth, 2023). The pre-treatment joint F-test rejects strict parallel trends in our 2021–2025 window for all six wage and employment outcomes (F between 6.3 and 45.6). For ln(WageNonMfg), the maximum absolute pre-period event-study coefficient is 0.00083 (in 2021Q3), comparable in magnitude to the median post-treatment coefficient (~ 0.00101) and roughly half the largest post-treatment coefficient (~ 0.00149). For the manufacturing outcomes the pre-period coefficients are both larger and more persistent (Figure 1, panels b–c). We do not claim that unconditional parallel trends holds; instead we rely on (a) formal Honest-DiD bounds, (b) a pre-COVID placebo, and (c) a robustness check that explicitly controls for each county's 2021 wage-recovery trajectory.

3 Empirical Findings

Table 2 reports the static TWFE estimates (Panel A) and dynamic LP estimates with two lags (Panel B) for the three central outcomes. Figure 2 plots the LP impulse responses with 95% wild-bootstrap percentile-t confidence intervals; Figure 1 shows event-study coefficients with cluster-robust 95% intervals.

Table 2: TWFE and Jordà Panel Local Projection Estimates (Expanded Panel, $G = 61$)

Outcome (Y_{t+h})	(1) ln(WageNonMfg)	(2) ln(EmpMfg)	(3) ln(WageMfg)
Panel A: Two-Way Fixed Effects (Static)			
Intensity·Post (β)	0.0006	0.0012	−0.0003
	(0.0001)	(0.0018)	(0.0005)
[WCB/Perm p]	[**0.081**/**0.001**]	[0.578/0.686]	[0.574/0.766]
Observations	1,132	1,132	1,132
Panel B: Jordà Panel Local Projections (with 2 Lags)			
$h = 0$ (Contemp.)	0.0007	−0.0011	−0.0009
[WCB/Perm p]	[**0.075**/**0.003**]	[0.144/0.443]	[0.579/0.200]
N	1,010	1,010	1,010
$h = 1$	0.0006	−0.0021	−0.0009
[WCB/Perm p]	[0.194/**0.004**]	[0.043/0.467]	[0.131/0.046]
N	949	949	949
$h = 2$	0.0001	−0.0028	−0.0015
[WCB/Perm p]	[0.609/0.741]	[0.016/0.530]	[0.068/0.316]
N	888	888	888
$h = 3$	0.0001	−0.0027	−0.0007
[WCB/Perm p]	[0.559/0.581]	[0.040/0.544]	[0.506/0.639]
N	827	827	827
$h = 4$ (1 Year)	0.0006	−0.0019	−0.0012
[WCB/Perm p]	[**0.072**/**0.013**]	[0.079/0.509]	[0.319/0.012]
N	766	766	766

Parentheses (Panel A) report county-clustered standard errors. Brackets report empirical p-values from (i) restricted Wild Cluster Bootstrap with Rademacher weights, and (ii) county-intensity permutation, $B = 1{,}000$ each. With $G = 61$, the WCB is the preferred procedure (MacKinnon and Webb, 2018). **Bold** entries are significant at the 10% level under *both* the WCB and the permutation test. The non-manufacturing wage spillover in column (1) is the only column meeting this joint criterion. Estimates in columns (2)–(3) are reported for completeness but are not robust to permutation inference.

Non-manufacturing wage spillover. The static TWFE estimate is $\hat{\beta} = +0.0006$, significant under both WCB ($p = 0.081$) and permutation ($p = 0.001$). LP estimates show a bookend pattern: contemporaneous and one-year coefficients reject under WCB ($\hat{\beta}^{h=0} = +0.0007$, $p_{\text{WCB}} = 0.075$; $\hat{\beta}^{h=4} = +0.0006$, $p_{\text{WCB}} = 0.072$), while intermediate horizons $h = 2, 3$ are statistically indistinguishable from zero ($p_{\text{WCB}} > 0.55$). Under the sharper permutation test, the pattern is denser ($p_{\text{perm}} = 0.003$, 0.004, 0.013 at $h = 0, 1, 4$). We do not have a structural model for the dip at $h = 2$–3 and caution against over-interpreting the dynamic path in a small panel; the pooled TWFE estimate is the more reliable summary. Evaluated at the median nonzero-intensity treated county (5.6), the static coefficient implies a 0.34% rise in non-manufacturing weekly wages—about $3.40 per week relative to the baseline mean. This is a modest, demand-side spillover consistent with Moretti (2010).

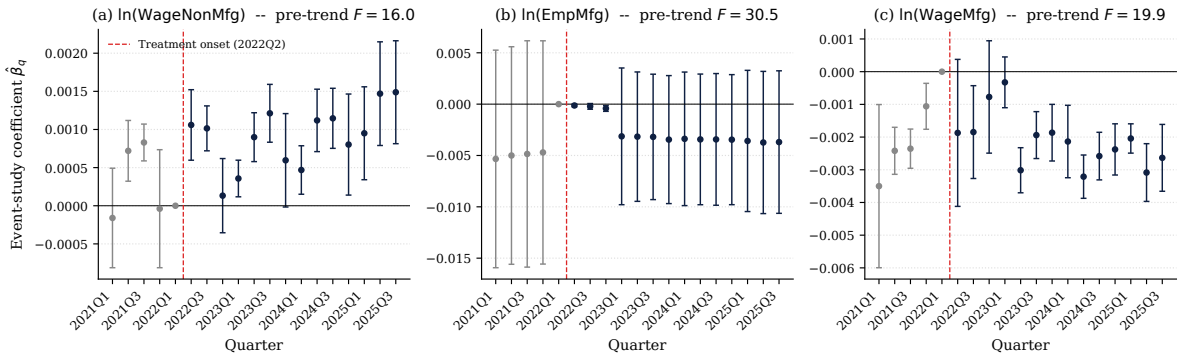


Figure 1: **Event-Study Coefficients with 95% Cluster-Robust Confidence Intervals (Expanded Panel, $G = 61$).** Each panel plots $\hat{\beta}_q$ from a saturated regression of the outcome on Intensity$_c$ interacted with quarter dummies, omitting 2022Q1 as the reference. Gray markers are pre-treatment; dark markers are post-treatment. Pre-treatment coefficients for ln(WageNonMfg) peak at $|\hat{\beta}_q| = 0.00083$ in 2021Q3 (vs. a median post-treatment $|\hat{\beta}_q| \approx 0.0010$); for the manufacturing outcomes the pre-period coefficients are larger and persistent, which weakens the parallel-trends defense for those outcomes (see Honest-DiD analysis in text).

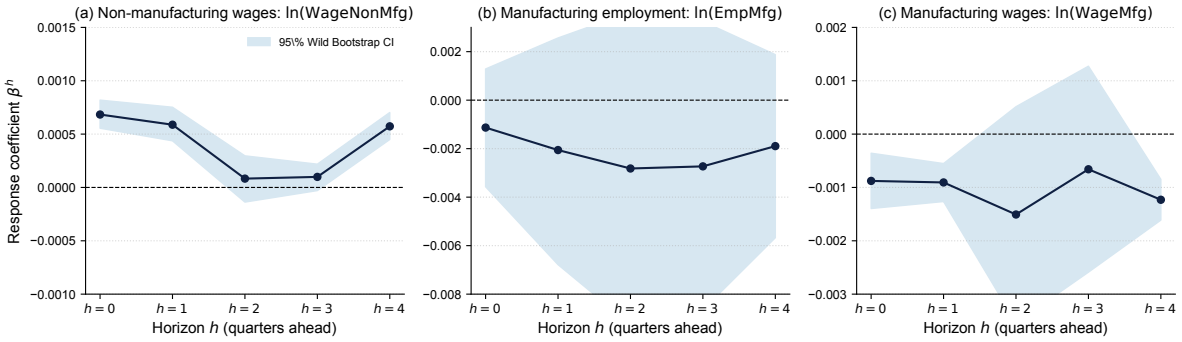


Figure 2: **Jordà Panel LP Impulse Responses with 95% Wild Cluster Bootstrap Confidence Intervals (Expanded Panel, $G = 61$).** Specifications include two lags of the dependent variable, county and quarter fixed effects, and cluster-robust standard errors at the county level. Bootstrap intervals are percentile-t with Rademacher weights ($B = 500$).

Mechanism: composition versus within-job wage growth. QWI worker-flow data decompose this aggregate wage response. In manufacturing, new-hire average monthly earnings (EarnHirNS) fall significantly with intensity (LP $h = 0$: $\hat{\beta} = -0.0022$, $p_{\text{perm}} = 0.035$), while stable-employee earnings (EarnS) show a small decline at $h = 2$ ($\hat{\beta} = -0.0007$, $p_{\text{perm}} = 0.038$) and no significant response at other horizons. In total private, new-hire earnings rise at intermediate horizons ($h = 1$: $\hat{\beta} = +0.0012$, $p_{\text{perm}} = 0.015$; $h = 2,3$: $p_{\text{perm}} = 0.029, 0.025$), while stable-employee earnings show no significant change ($p_{\text{perm}} \geq 0.40$). The pattern—movement in new-hire earnings, near-stability in stable-employee earnings—is consistent with the wage spillover operating through changes in workforce-entry composition rather than within-job wage growth for incumbents. Because the QWI data are aggregated at the county-by-sector level, we cannot rule out concurrent shifts in occupation mix, hours, or establishment composition; the near-stability of stable-employee earnings nevertheless argues against widespread raises to incumbents as the dominant mechanism, though it cannot exclude the possibility that some incumbents received raises while others were replaced by lower-paid workers within the same stable-employee pool.

Identification credibility. We address the rejected pre-trend F-test for the headline outcome through four complementary checks.

(i) Event-study visual. The pre-treatment event-study coefficients for ln(WageNonMfg) in Figure 1 are comparable in absolute magnitude to the post-treatment coefficients (maximum $|\hat{\beta}_q^{\text{pre}}| = 0.00083$ in 2021Q3 vs. median post-treatment $|\hat{\beta}_q| \approx 0.0010$). The $F = 16.0$ rejection thus reflects pre-period deviations that are non-trivial in level but estimated with sufficient precision to be statistically separated from zero. We address them quantitatively via Honest-DiD bounds rather than dismiss them.

(ii) Pre-COVID placebo. We re-estimate (2) on 2017Q1–2019Q4 data with a fake treatment date of 2018Q2 (709 obs.). The WCB delivers no significant placebo effects on ln(WageNonMfg) at $h = 0, 1, 4$ ($p_{\text{WCB}} \in [0.18, 0.58]$). Pre-trend F-tests pass in 2017–2019, suggesting that the trend rejections in our 2021–2025 window reflect differential post-COVID recovery dynamics specific to that window rather than a generic feature of these counties.

(iii) Recovery-trajectory control. To address the recovery-dynamics interpretation directly, we augment (1) and (2) with each county's 2021Q1–2021Q4 average quarterly log non-manufacturing wage change interacted with Post_t. Point estimates change only at the fourth decimal: TWFE $\hat{\beta} = +0.00063$, LP $\hat{\beta}^{h=0} = +0.00068$, LP $\hat{\beta}^{h=4} = +0.00057$. The LP horizons remain WCB-significant at the 10% level ($p_{\text{WCB}} = 0.077$ at $h = 0$, 0.072 at $h = 4$); the pooled TWFE WCB p-value crosses just above the 10% threshold ($p_{\text{WCB}} = 0.106$). Permutation p-values remain sharp at 0.001, <0.001, and 0.005 respectively. We read this as evidence that differential 2021 recovery trajectories explain at most a small share of the spillover; under the conservative WCB the dynamic-horizon estimates retain marginal significance, while under permutation they are clearly distinguishable from zero.[2]

(iv) Honest-DiD sensitivity. Following Rambachan and Roth (2023), the breakdown value $M^\star = (|\hat{\beta}| - 1.96\hat{\sigma}) / \max_q |\hat{\beta}_q^{\text{pre}}|$ is the largest unobserved post-treatment differential trend—measured in

[2] We also estimated (2) with county-specific linear time trends. With only $T = 19$ quarters and 61 counties, that specification adds 61 trend parameters on top of 61 county and 19 quarter fixed effects, and produces a near-singular design matrix: coefficient and standard-error estimates explode, and the clustered variance becomes non-positive-definite. We therefore do not report it.

multiples of the worst pre-period coefficient—consistent with rejecting the null.[3] For ln(WageNonMfg), $M^\star = 0.63$ at $h = 0$, 0.55 at $h = 1$, and 0.50 at $h = 4$. Because the worst pre-period coefficient is 0.00083, $M^\star = 0.50$ implies that an unobserved post-treatment differential trend of 0.00042 per quarter—roughly half the magnitude of the largest pre-period deviation—would render the $h = 4$ LP estimate indistinguishable from zero. This is a tight, not generous, bound: the result is fragile to large unobserved trends but survives modest ones in the range observed in our pre-period. For ln(EmpMfg) at $h = 2$, $M^\star = 0$: the naive 95% CI already contains zero. Only the non-manufacturing wage spillover survives the Honest-DiD criterion. Figure 3 plots the full $M \mapsto$ CI curve.

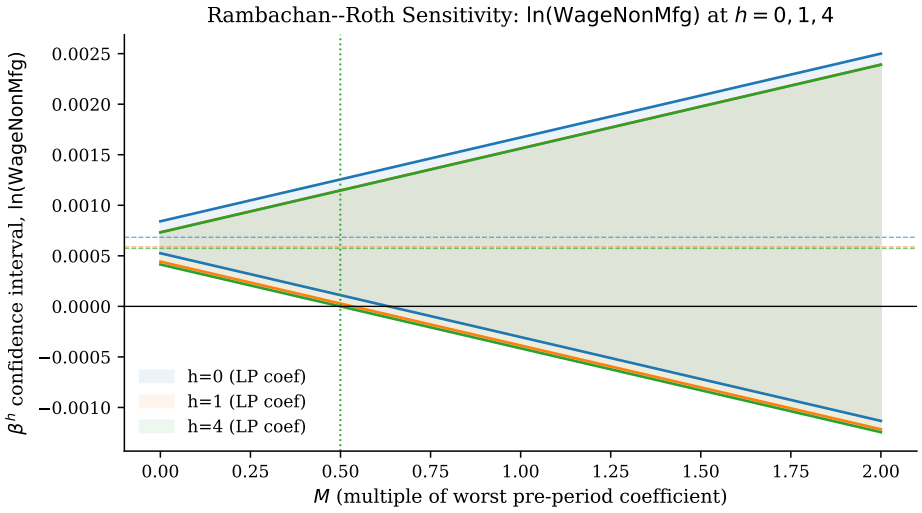


Figure 3: **Rambachan–Roth Sensitivity Curves for** ln(**WageNonMfg**)**.** Shaded regions display the LP coefficient's robust confidence interval as a function of M, the maximum allowed differential post-treatment trend in multiples of the worst pre-period coefficient (0.00083). Horizontal dashed lines mark the LP point estimates; the vertical dotted line marks $M^\star = 0.50$ for $h = 4$, beyond which the confidence interval includes zero.

Predetermined defense-industrial-base IV (consistency check). The shock has two institutional channels—private contracts to defense manufacturers (Ouachita AR, York PA, Sullivan TN) and obligations to DOD installations (Churchill NV / NAWS Fallon, Harford MD / Aberdeen Proving Ground, Fairfax VA / Pentagon–Fort Belvoir, Middlesex MA / Hanscom AFB)—motivating two complementary predetermined instruments: (i) the county-level defense-manufacturing employment share from EFSY-imputed CBP averaged over 1989–1991 (Eckert *et al.*, 2021) for thirteen 4-digit defense-manufacturing SIC codes, and (ii) the 1969 BEA REIS federal civilian + military employment share (Bureau of Economic Analysis, 2023). On the 23-county matched sample that merges with BEA, the joint first-stage F for the ln(WageNonMfg) horizons reported in Table 3 ranges from 30.4 to 31.5, well above the Stock–Yogo threshold ($F = 10$); first stages for the manufacturing outcomes (omitted from the table) are weaker, with F as low as 12.6. OLS coefficients on the 23-county sub-panel almost coincide with the 61-county estimates ($\hat{\beta}^{h=1}_{\text{nonmfg-wage,23}} = +0.00062$ vs. $+0.00059$), so the 2SLS-vs-OLS contrast in Table 3

[3] The breakdown formula uses the conventional cluster-robust standard error $\hat{\sigma}$ from the LP regression, as in the original Rambachan–Roth implementation. WCB-based confidence sets are not directly substitutable into the closed-form expression and would require a bespoke inversion of the WCB null distribution at each value of M; we therefore retain the standard implementation.

reflects the IV adjustment rather than sample shifts.

Table 3: Same-Sample OLS vs. 2SLS Local Projections for ln(WageNonMfg) (Matched 23-County Subsample)

	OLS		2SLS		First-stage
Y_{t+h}	$\hat{\beta}$	p	$\hat{\beta}$	p (p_{perm})	joint F
$h=0$	+0.00067	<0.001	+0.00065	0.057 (0.089)	30.4
$h=1$	+0.00062	<0.001	+0.00072	**0.029** (0.248)	30.6
$h=4$	+0.00060	<0.001	+0.00070	0.113 (0.257)	31.5

Instruments: 1989–1991 EFSY-imputed CBP defense-SIC employment share and 1969 BEA REIS federal civilian + military employment share. Reported p-values are conventional OLS / 2SLS, with permutation p in parentheses for 2SLS. **Bold** entries reject under conventional p. Same-sample IV estimates for ln(EmpMfg) and ln(WageMfg) are larger in magnitude than OLS but remain insignificant under permutation ($p_{\text{perm}} \geq 0.26$); we therefore do not interpret them as evidence of an underlying effect and omit them from the table.

The 2SLS estimates for the non-manufacturing wage spillover *match the OLS estimates in magnitude and direction*, providing a clean consistency check: replacing realized place-of-performance intensity with its predetermined-defense-industrial-base projection leaves the spillover unchanged.

Additional robustness. Winsorizing Intensity$_c$ at the 95th percentile of nonzero-treated counties preserves the non-manufacturing wage spillover ($\hat{\beta}^{h=0} = +0.0008$, $p_{\text{WCB}} = 0.057$, $p_{\text{perm}} = 0.026$; $\hat{\beta}^{h=4} = +0.0007$, $p_{\text{WCB}} = 0.062$, $p_{\text{perm}} = 0.038$), confirming the result is not driven by Churchill County's extreme value. Sector falsification on NAICS 23 (Construction) and NAICS 61 (Education) wage outcomes yields wild-bootstrap-insignificant effects at every horizon ($p_{\text{WCB}} \geq 0.18$), supporting the interpretation that the spillover is defense-procurement-specific rather than generic local demand. Romano–Wolf step-down p-values (Cameron and Miller, 2015) leave the headline ln(WageNonMfg) result significant at the 10% level after adjustment across the five LP horizons. Across donut-hole, leave-one-out, and four-lag perturbations, the qualitative pattern is preserved.

4 Conclusion

Using a matched 61-county panel combining BLS QCEW outcomes with Census QWI worker-flow indicators, we document a modest rise in non-manufacturing average weekly wages—about 0.34% at the median treated-county intensity—in counties receiving large post-March 2022 emergency defense procurement obligations. The QWI worker-flow pattern is consistent with the spillover operating through compositional changes in workforce entry rather than within-job wage growth; the county-by-sector aggregation precludes a fully identified mechanism. The result survives a pre-COVID placebo, recovery-trajectory controls, Honest-DiD sensitivity bounds, intensity winsorization, sector falsification, and a two-instrument predetermined-defense-industrial-base IV. We find no robust evidence that manufacturing employment or wages respond to the shock. Treated counties' baseline manufacturing employment shares range from 4% to 26% (median 11%); whether comparable wage spillovers obtain in less specialized local economies is left to settings with more variation in the local industrial base. The main empirical contribution is that the wage spillover appears to operate through hiring composition, an adjustment margin not isolated by aggregate cross-sectional multiplier estimates.

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