Appendix A: Data and methods

Appendix A1: Data sources

ICE arrests:

The administrative arrest data for this analysis is sourced from the Deportation Data Project (DDP). The arrest data published by DDP contains a record of each ERO ICE arrest from October 2022 to early March 2026. Each record carries a “landmark” string, and both state and area of responsibility (AOR) of the arrest as well as the date and method of arrest.

Lightcast:

Monthly core-based statistical area (CBSA) x NAICS employment counts come from Lightcast’s proprietary metro employment series, which is built primarily from the BLS Quarterly Census of Employment and Wages (QCEW), an administrative dataset covering nearly all formal employment via quarterly tax filings. QCEW suppresses cells in the raw data (area x time) where disclosure could reveal an individual employer. Lightcast imputes many of these suppressed cells largely using aggregate geographies and broader industry codes. Because QCEW is built from tax records, it captures formal payrolled employment only. Job losses observed in our models are therefore tilted heavily toward formal incumbents (American-born and documented immigrants)

ACS/IPUMS:

We use the IPUMS public-use microdata sample of the yearly ACS to build a CBSA x NAICS2 immigrant share panel by pooling the 1-year files from 2021, 2022, and 2023.

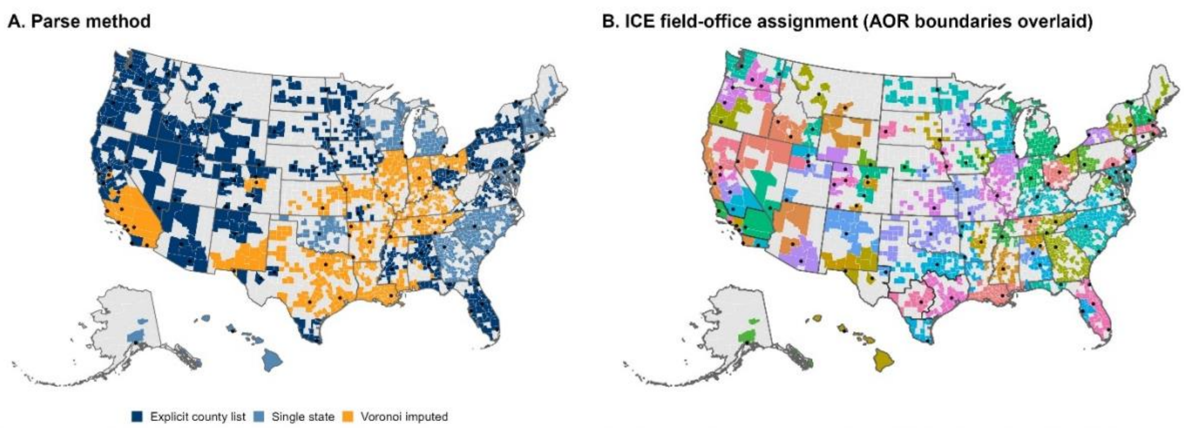
Appendix A2: Data cleaning

The raw records from the arrest data do not include a robust geography field more granular than the state level. However, we use the free-text “landmark” field in the dataset to identify the CBSA of likely arrest. The field contains a mix of states, cities, counties, addresses, facility names, facility codes, and operational codes.

Pre-cleaning crosswalk construction

A meaningful share of records has landmarks that explicitly name the ICE office (or sub-office) where the arrest was recorded, not the physical location of the arrest. Treating the office as the arrest location could overcount arrests in a handful of urban metros and undercount the surrounding territory. We address this by creating a crosswalk that maps each sub-office to the counties it covers, so records referencing an office location can be re-allocated to the counties with the appropriate jurisdiction.

We accomplish this using an ICE office shapefile, also published by DDP. In the file metadata, 73 sub-offices have explicit county lists; 16 refer to a single state; and 32 have vague or incomplete geographic information. The explicit lists are parsed directly into (sub-office x county) pairs. For the 32 sub-offices without a parseable area, each county is assigned to whichever same-AOR sub-office has the closest centroid, guaranteeing that every county sits in the territory of its nearest office. We arrive at a crosswalk matching 117 out of 121 suboffices to 2067 counties covering 878 CBSAs.



Black dots = 119 ICE field sub-office locations (Guam excluded). Panel B fills counties by their assigned ICE field (sub-)office (117 categories, legend omitted); heavier grey lines show AOR boundaries. Non-metro counties (grey) have no field-office assignment in the crosswalk.

A significant share (41%) of arrest records contains a three-letter code as the only meaningful geographic signal in the landmark field. To date, we have not seen any published taxonomy detailing the locations associated with these (approximately 300 unique) codes. The codes largely refer to cities, but also in some cases counties and county jails. We create a crosswalk using a combination of manual entry for likely matches (e.g., PHX -> Phoenix) and a DOJ immigration court handbook containing matches for around 50% of the codes.

Cleaning pipeline

After removing all likely duplicates, the cleaning pipeline runs eight passes over the arrest panel.

1. Matches the location code with the location-code crosswalk.
2. Extracts geographic signals from landmark strings (state codes, county patterns, facility keywords).
3. Pre-fills sparse AOR and state data (in the respective fields). For landmarks with no state field, we promote the dominant state of the recorded AOR so subsequent dictionary matching is constrained to the correct state.
4. Resolves city/county with state-constrained n-gram dictionary matching. With an additional fuzzy-match pass.
5. Resolves address-like leftovers and unresolved facility names via the OpenStreetMap API.

6. Records whose landmark refers to an ICE office are nulled and distributed to the counties within the office jurisdiction based on the immigrant population in those counties.
7. Joins city/county to CBSA via city centroids, and a county to CBSA crosswalk.
8. Records still missing a CBSA, but with a known state x AOR (or a state signal in the landmark field), are split across candidate CBSAs in that AOR x state in proportion to the existing arrest distribution blended with the county immigrant population in proportion to the share of arrests cleaned at the State x AOR level.

Arrests retained at each geocoding stage

Stage	Arrests	% of raw
Raw arrests	**697,378**	**100.0**
└ *of which: only location code as geo signal*	*291,819*	*41.8*
Resolved via location code and city/county dictionaries	541,692	77.7
+ address and facility geocoding	551,866	79.1
+ manual landmark recodes	551,874	79.1
+ ICE office allocation	580,494	83.2
+ state-level fallback allocation	678,546	97.3

At this point, around 3% of arrests in the full panel are unmatched to a CBSA due to a lack of any meaningful geographic signal. The missing arrests are heavily skewed toward earlier dates in the panel and among only a handful of AORs. We scale the matched counts to the AOR-month "ground truth" total with the assumption that the remaining arrests in an AOR-month would have been distributed similarly to the cleaned locations.

For our cleaned arrest panel, we perform an additional cleaning step to recover only the arrests visibly taking place in communities. We accomplish this by filtering the apprehension method field to only "at-large arrests which include the arrest methods coded as: located, noncustodial arrest, worksite enforcement, 287(g) program, other task force, law enforcement agency response, and other efforts. With the exception of 287(g) in some cases, this filter excludes arrests performed directly out of jails (CAP or other jail-based arrest methods).

ICE arrests by apprehension method Bold = methods classified as at-large / worksite.		
Apprehension method	Arrests	% of total
Non-Custodial Arrest	**270,728**	**38.82%**
CAP Local Incarceration	158,716	22.76%
Custodial Arrest	102,373	14.68%
Located	**42,236**	**6.06%**
CAP Federal Incarceration	35,859	5.14%
287(g) Program	**25,857**	**3.71%**
Other efforts	**16,969**	**2.43%**
CAP State Incarceration	15,599	2.24%
ERO Reprocessed Arrest	**12,481**	**1.79%**
Probation and Parole	5,754	0.83%
Inspections	4,650	0.67%
Patrol Border	2,018	0.29%
Law Enforcement Agency Response Unit	**1,324**	**0.19%**
Other Agency (turned over to INS)	812	0.12%
Other (12 methods)	811	0.12%
Other Task Force	**756**	**0.11%**
Worksite Enforcement	**435**	**0.06%**

Source: Deportation Data Project.

Employment Data Cleaning (Lightcast)

We use the county-level total employment data for the aggregate CBSA estimates, and county-level data at the county x NAICS2 and county x NAICS3 levels for the respective sector-level analyses. We then aggregate to CBSA with a simple crosswalk. We pull data from January 2019 through September 2025. For our headline estimate, we only use employment data starting in January 2024, but include trends from January 2022 in the event studies.

We keep CBSAs with average monthly Lightcast employment greater than 50,000 in 2024 and at least 12 months of uninterrupted pre-treatment data. This is the eligible analysis sample for both treated and control sets.

Demographic Data Cleaning (ACS/IPUMS)

We build the NAICS2 x CBSA demographic panel by pulling PERWT, STATEFIP, PUMA, EMPSTAT, IND, BPL, AGE, and EDUC from the IPUMS USA API. We restrict the sample to only those employed with a valid industry code. We then map the ACS 4-digit industry codes to NAICS2.

We define two parallel immigrant indicators on each respondent: foreign-born (BPL >=100) and likely-undocumented—corresponding to all the foreign-born working age population with at most a high school education (BPL >=100 & AGE in [20,64] & EDUC <=6). PUMA-level respondent counts are allocated to counties via a population-weighted Census 2020 tract to PUMA relationship file, where county-PUMA weights are computed from the NHGIS tract populations. County-level employment counts are then aggregated to CBSA. Sector-level estimates are filtered to the treatment CBSAs, aggregated to the sector total and divided by total sector employment to produce sector-level immigrant shares. The CBSA shares used to aid the data cleaning process are computed similarly.

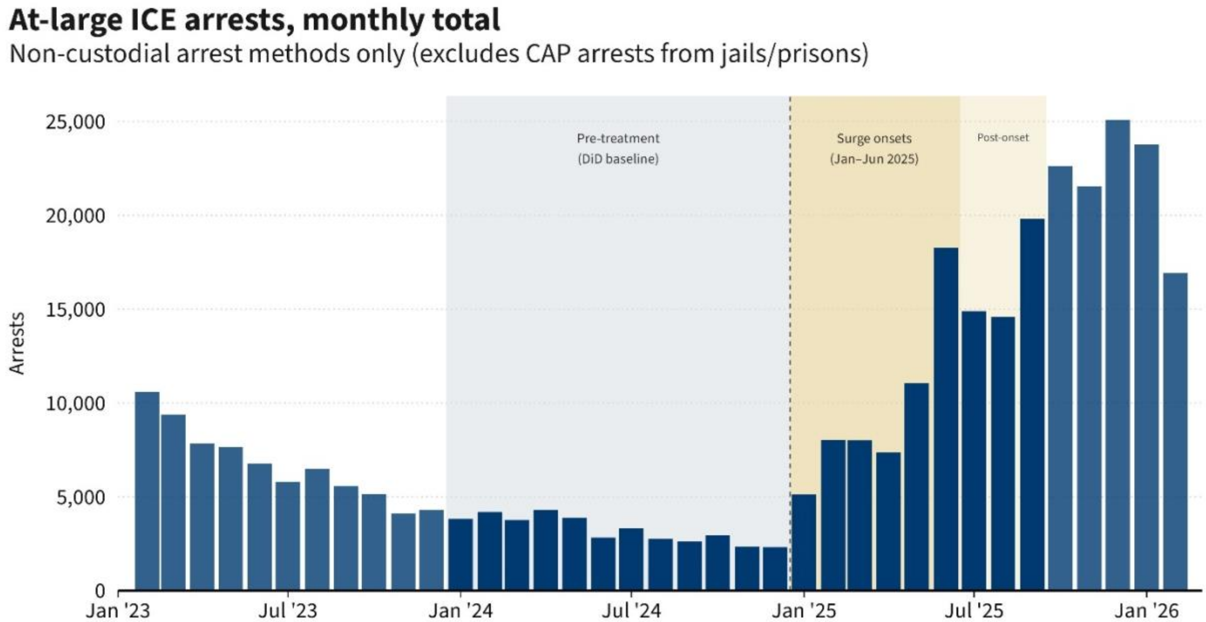
Appendix A3: Surge definitions

Surge dose and surge cities

For each eligible CBSA c, we define the surge dose as the cumulative excess of 2025 H1 arrests over the 2024 monthly baseline:

$$A_c = \sum_{\{m \in Jan-Jun\ 2025\}} (arrests_{c,m} - \overline{arrests}_{c,2024})$$

A CBSA is treated if $\mathrm{A_c} \geq \mathrm{Q}_{0.75}(\mathrm{A})$, the top quartile of the dose distribution within the eligible sample. Everyone else, including zero-arrest CBSAs, makes up the donor pool of never-treated controls.



Cohort assignment

Each treated CBSA is assigned a surge onset month g_c from the following algorithm:

$$g_c \ \operatorname{argmax}_{t \in Jan-Jun2025}(\overline{arr}_t - baseline_c)\sqrt{T - t + 1}$$

Where $\overline{arr}_t$ averages the arrests from month t through the deadline. Rather than picking the first month above some threshold (which is noisy on small counts), it picks the month from which the forward-average arrest level is highest, with a months-remaining penalty that discourages assigning a late onset to a CBSA whose increase was more gradual.

Panel description

We constrain the panel to begin in January 2024, giving at least a full year of pre-treatment months for parallel-trends estimation. The panel ends at the Lightcast employment frontier (currently September 2025). The arrest data extend until March 2026 but are clipped to match the employment window. The eligible universe of CBSAs yields a panel of 339 CBSAs of which 86 clear the top-quartile dose cutoff and 255 form the never-treated control. Treated CBSAs span six surge cohorts (Jan-Jun 2025), which determines the maximum observable event horizon per cohort. The earliest cohorts are observed through e = 8 months; the latest, only through e = 3.

Table 3.1 summarizes the treated and control sets along the dimensions that matter for the design: metro size, immigrant share, pre-period employment growth, and 2024 baseline arrests. Although pre-period employment growth is similar, the two groups differ on size and immigrant share. Importantly, our findings are robust to these differences. The relevant identifying assumption of the Callaway-Sant'Anna estimator described below is that employment trajectories of surge and non-surge cities do not differ prior to the surge. Results in the following sections confirm they do not. Still, we explicitly test whether differences in demographics of surge and non-surge cities affect our results. Appendix A5

shows that they do not. The treatment effect is still large even when sub-setting on numerous baseline traits including immigrant density.

Balance: treated vs. control metros Levels are unweighted; growth rates are aggregate (group totals).				
	Median		Mean (group aggregate)	
	Treated (N=86)	Control (N=255)	Treated (N=86)	Control (N=255)
Employment (2024 avg, thousands)	487	96	1,057.7	158.8
Employment growth '23–'24 (%)	1.16	1.11	1.10	1.09
Foreign-born workers (thousands)	85	10	263.8	20.4
Foreign-born share (%)	17	9	24.8	12.4
Monthly arrests (2024 baseline)	15	0	30.8	1.7
Monthly arrests (Jan–Jun 2025)	60	0	94.9	2.6
Increase from 2024 baseline (%)	270	88	208.5	70.1

Appendix A4: Identification and estimator

Our baseline estimator is a Callaway-Sant'Anna staggered difference-in-difference on log monthly CBSA employment (Lightcast) with the six January-June surge cohorts as the treated groups and the 255 metros that never crossed the surge cutoff as the control pool. We compute one group-time average treatment effect for each cohort-calendar month, then aggregate the group-time effects two ways, into a dynamic event-time path from up to 16 months before onset (in the case of a June 2025 surge) to up to 8 months after (in the case of a January 2025 surge), and a simple overall post-treatment ATT.

Identification rests on unconditional parallel trends: That absent the surge, treated and control metros would have moved together from January 2025 onward. Our confidence intervals are generated from a bootstrap method clustered at the CBSA level.

Headline ATT

Employment effect of the 2025 ICE enforcement surge					
Outcome	ATT (log emp)	SE	95% CI	Treated CBSAs	Control CBSAs
total_emp	−0.0074	0.0019	[-0.011, -0.004]	86	255

Dynamic and Cohort ATT

Dynamic ATT path (C-S event-time aggregate)

Event time	ATT (SE)	95% CI	Cohorts	Treated CBSAs
-11	0.000 (0.001)	[-0.002, 0.002]	6	86
-10	-0.002 (0.001)	[-0.004, -0.000]	6	86
-9	-0.003 (0.001)	[-0.005, -0.001]	6	86
-8	-0.001 (0.001)	[-0.003, 0.001]	6	86
-7	0.001 (0.002)	[-0.002, 0.004]	6	86
-6	0.000 (0.001)	[-0.002, 0.002]	6	86
-5	0.001 (0.001)	[-0.001, 0.003]	6	86
-4	0.002 (0.001)	[-0.000, 0.004]	6	86
-3	0.002 (0.001)	[0.000, 0.004]	6	86
-2	0.002 (0.001)	[0.001, 0.003]	6	86
-1	0.001 (0.001)	[-0.000, 0.003]	6	86
0	-0.001 (0.001)	[-0.003, 0.002]	6	86
1	-0.001 (0.002)	[-0.004, 0.002]	6	86
2	-0.003 (0.002)	[-0.007, 0.000]	6	86
3	-0.006 (0.002)	[-0.011, -0.002]	6	86
4	-0.011 (0.003)	[-0.017, -0.006]	5	72
5	-0.014 (0.004)	[-0.021, -0.007]	4	56
6	-0.015 (0.004)	[-0.022, -0.008]	3	51
7	-0.015 (0.003)	[-0.020, -0.010]	2	45
8	-0.015 (0.004)	[-0.023, -0.007]	1	16

Each row is the aggregate ATT at event-time e (months since local surge onset), with bootstrap SE in parentheses and 95% CI. Cohorts and Treated CBSAs count units contributing an ATT(g,t) cell at that horizon.

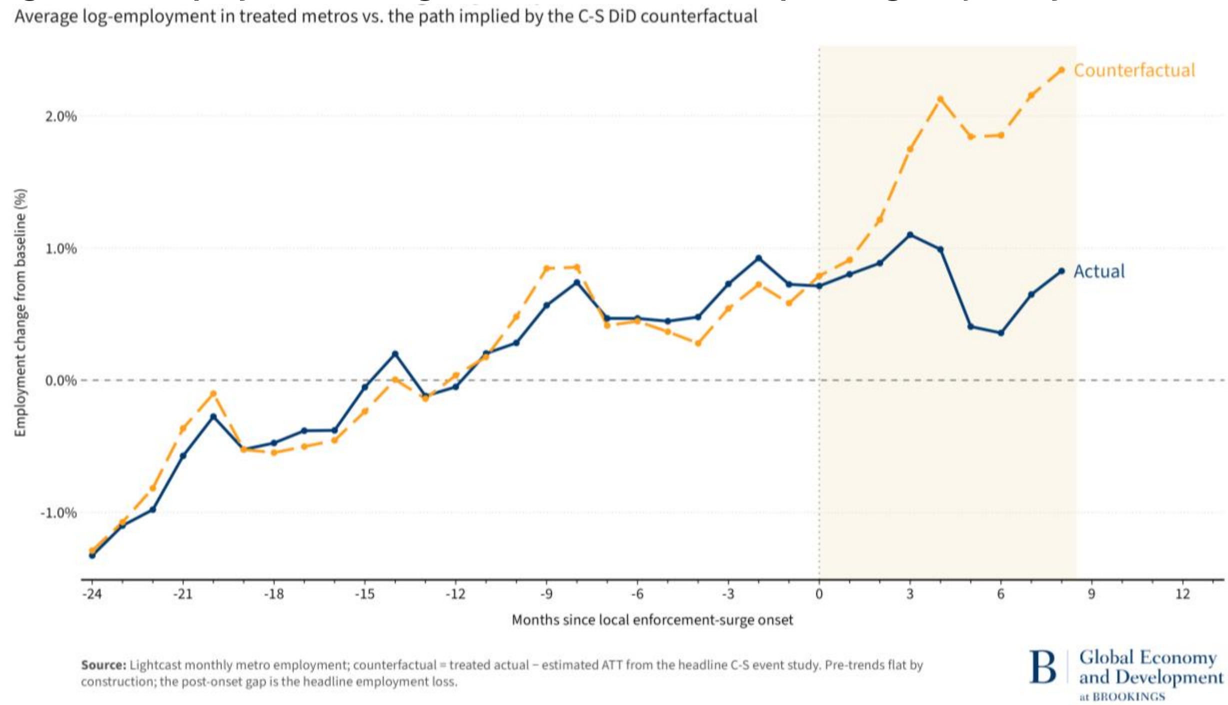
Balance by cohort (March–April pooled)

Cohort	N CBSAs	Median 2024 emp	Median FB share (%)	Median emp growth '23–'24 (%)	ATT at e=3	Cohort ATT
Jan 2025	16	388,100	17.12	1.22	-0.0038 (0.0033)	-0.0083 (0.0029)
Feb 2025	29	615,764	12.90	1.12	-0.0119 (0.0027)	-0.0104 (0.0022)
Mar+Apr 2025	11	483,934	22.76	1.44	-0.0139 (0.0122)	-0.0081 (0.0076)
May 2025	16	470,240	16.74	0.67	-0.0056 (0.0035)	-0.0030 (0.0026)
Jun 2025	14	431,834	28.10	1.28	0.0050 (0.0063)	0.0028 (0.0066)
Never-treated	255	96,280	9.07	1.11	—	—

ATT columns report cohort-specific log-employment effects with bootstrap SEs in parentheses. Cohort ATT averages the group-time ATTs across each cohort's observed post-period.

Figure A4 offers an intuitive view of the same underlying result in Figure 3. Rather than showing the statistical gap directly, it plots employment trajectories across time. The blue line shows how employment in surge cities evolved. The orange dashed line shows where surge cities would likely have gone absent enforcement, constructed from the actual trajectory of non-surge cities over the same period. The widening space between the two lines is the employment loss enforcement caused.

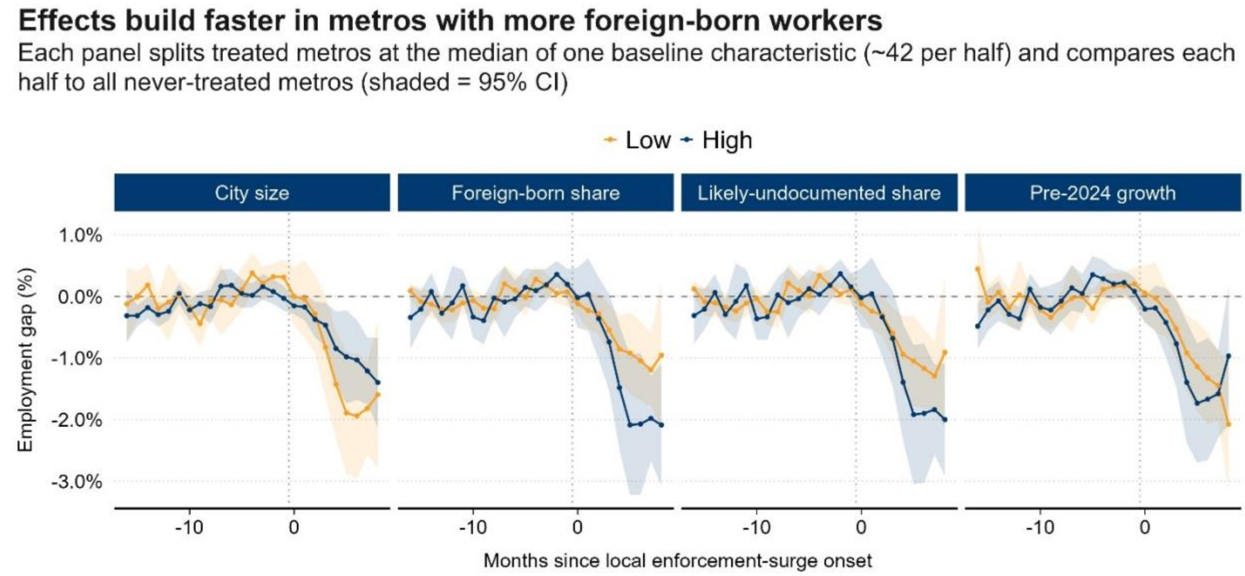
Figure A4. Employment in surge cities fell behind their pre-surge trajectory



Appendix A5: Heterogeneity

We split treated metros at the median on four pre-treatment characteristics and run the headline DiD on each half against the full donor pool. The patterns are consistent with the mechanism story.

- **City size**. Small, treated metros showed larger overall employment losses and losses more pronounced for construction.
- **Foreign-born share**. Metros with higher foreign-born employment experienced deeper employment declines.
- **Pre-2024 employment growth**. Both halves show similar negative effects in the post-period.



Appendix A6: Robustness

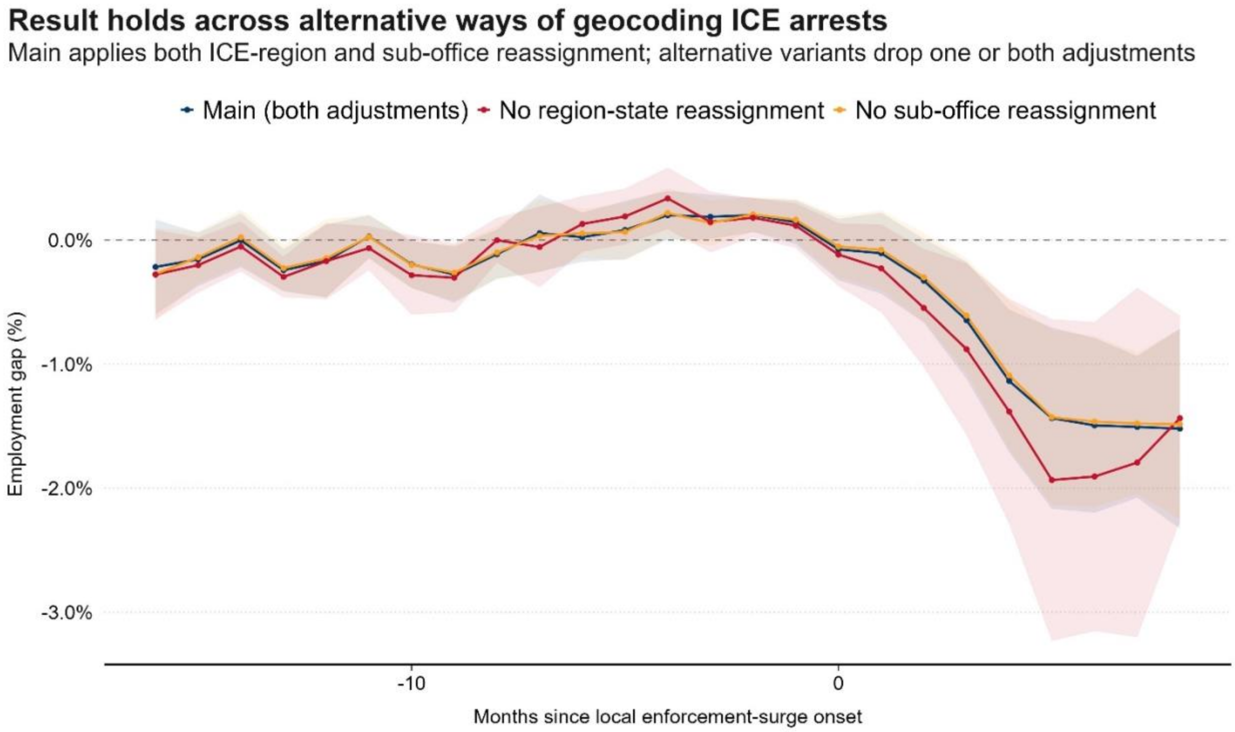
Estimate with less cleaned data

Our arrest panel relies on a multi-stage geographic cleaning pipeline to allocate arrest records to the CBSA where the arrest most likely occurred. When a record has no usable landmark at all, we allocate it across candidate CBSAs in the recorded AOR x state in proportion to the existing distribution of cleanly allocated arrests. Together with the office redistribution, this step allocates roughly 20% of the arrest records. It assumes that the missing arrests would have been distributed similarly to cleaned arrests.

To test whether the allocation steps of geo-cleaning change our main results, we re-estimate the headline DiD on the panel before performing the AOR-State allocation and find that the result is still significant despite wider confidence bands.

Effect under alternative arrest-location assignment

Variant	Treated CBSAs	Control CBSAs	ATT (log emp)	SE	95% CI
Main assignment	86	255	−0.0074	0.0021	[-0.011, -0.003]
Without state-level fallback	86	255	−0.0094	0.0032	[-0.016, -0.003]



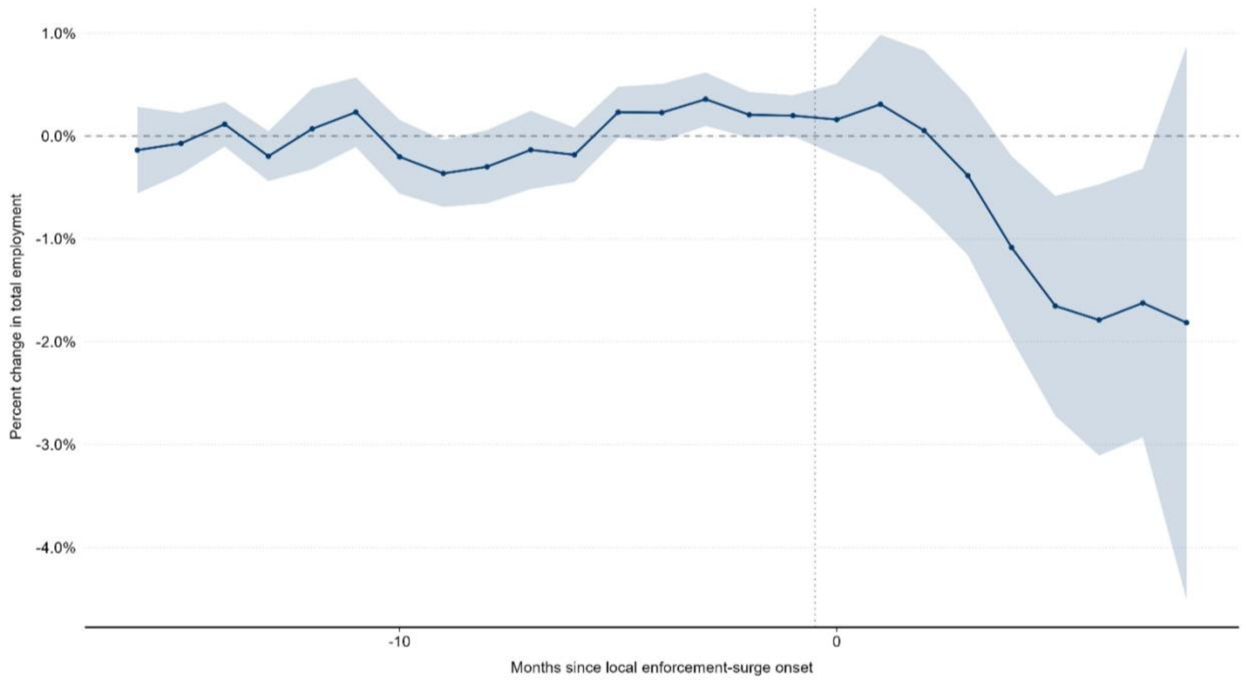
Arrests/workforce as outcome

We also estimate the DiD using the arrest surge per total workforce in a CBSA. This treatment definition presents a largely different group of CBSAs in the top quartile.

Effect under an alternative treatment definition

Specification	ATT (log emp)	SE	95% CI	Treated CBSAs	Control CBSAs
Headline (arrest surge)	−0.0074	0.0019	[-0.0111, -0.0036]	86	255
Surge per worker	−0.0058	0.0032	[-0.0120, 0.0004]	86	255

Treated CBSAs reclassified by surge + total workforce (top 25%); same C-S DiD spec as headline



Alternate clustering schemes

Standard errors under alternative clustering levels					
Cluster level	N clusters	ATT (log emp)	SE	95% CI	SE × headline
Construction					
CBSA	341	−0.0217	0.0045	[-0.031, -0.013]	1.00
State	49	−0.0221	0.0086	[-0.039, -0.005]	1.89
AOR	26	−0.0221	0.0076	[-0.037, -0.007]	1.68
Total employment					
CBSA	341	−0.0074	0.0021	[-0.011, -0.003]	1.00
State	49	−0.0074	0.0024	[-0.012, -0.003]	1.13
AOR	26	−0.0074	0.0024	[-0.012, -0.003]	1.13

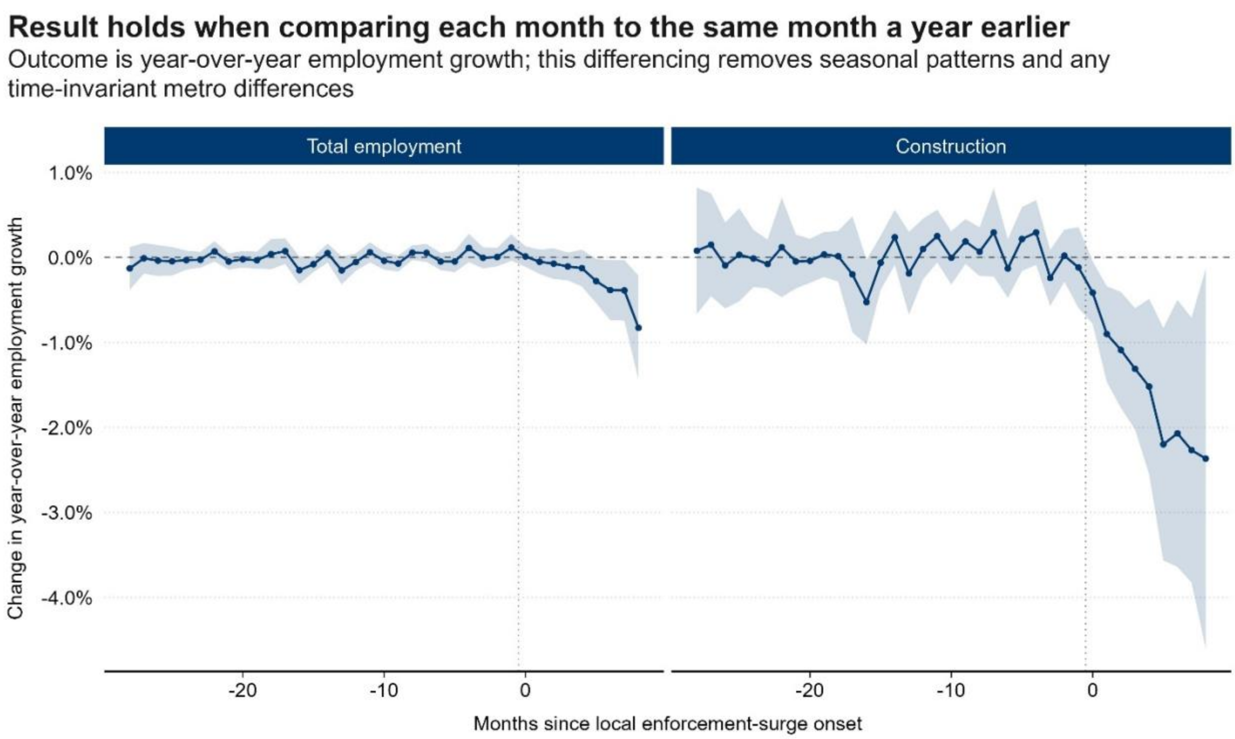
Alternate onset date selection

Headline ATT across surge-onset definitions				
Onset rule	ATT (SE)	95% CI	Treated CBSAs	Control CBSAs
Argmax (headline)	-0.0074 (0.0021)	[-0.0115, -0.0033]	86	255
Forward slope (+1 month)	-0.0052 (0.0019)	[-0.0088, -0.0015]	86	255
Threshold over baseline	-0.0067 (0.0019)	[-0.0103, -0.0030]	86	255

Each row re-runs the C-S DiD on log total employment with a different rule for dating each CBSA's surge onset. Argmax is the headline rule. Forward-slope onsets are shifted +1 month because the OLS slope is maximized at the month before the inflection.

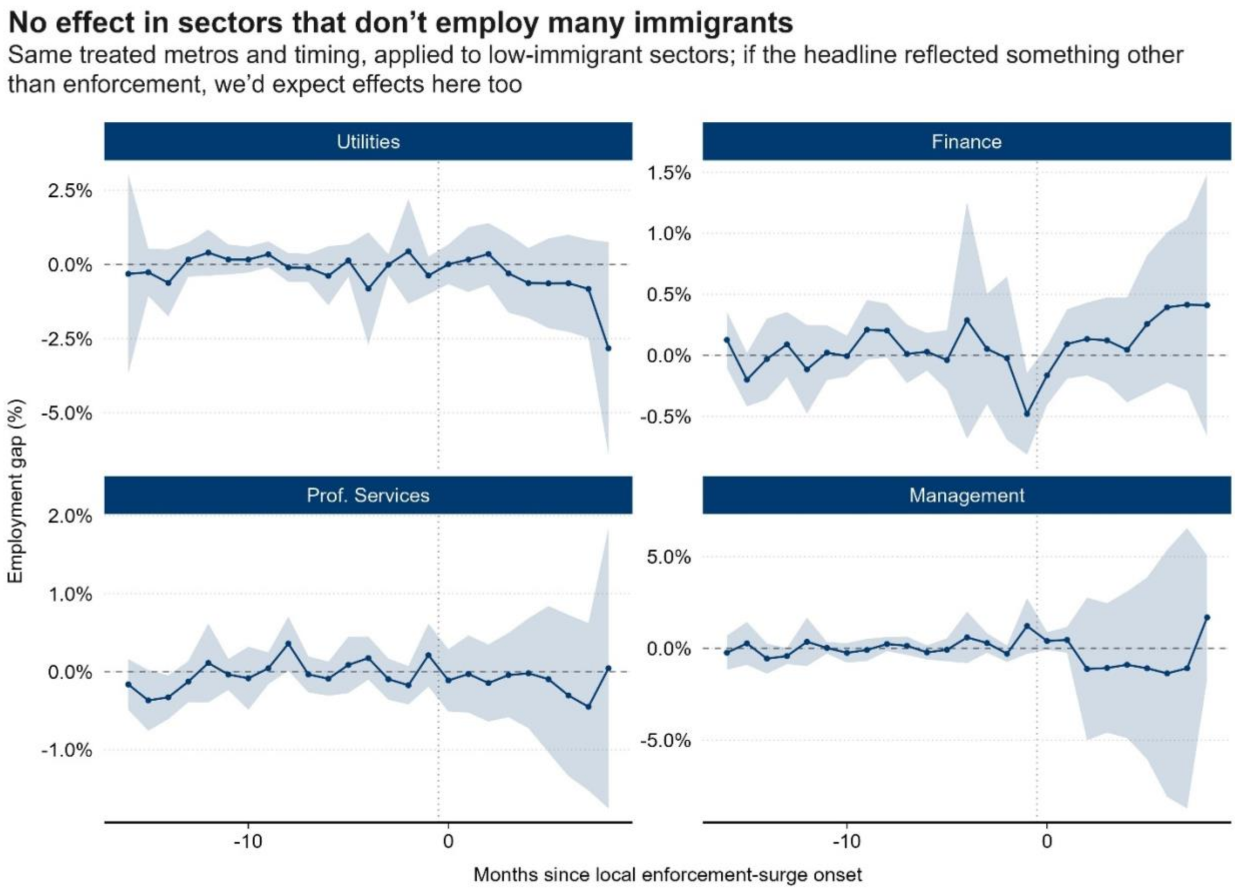
YoY-outcome

We re-estimate the headline DiD using year-over-year log employment change as the outcome, $\log(jobs_t) - \log(jobs_{t-12})$. Two mechanical properties of YoY differencing make it a useful complementary specification. First, the CBSA-specific month-of-year seasonality component drops, and the 12 month within-CBSA difference removes any time-invariant level component. The event study shows parallel pre-trends in the YoY employment outcome and a clean break at the event time. The DiD on this outcome identifies off parallel growth-rate trends rather than parallel level trends so while the magnitudes are not directly comparable, the direction, significance, and shape of the dynamic ATT event study corroborate our headline findings.



Outcome placebo

For this test, we look at sectoral outcomes where we do not expect to see any effect from immigration enforcement. We look at four 2-digit industries that are both non-immigrant-intensive and largely comprised of high-skill or capital-intensive labor. We do not see any significant effect in the post period for each sector.

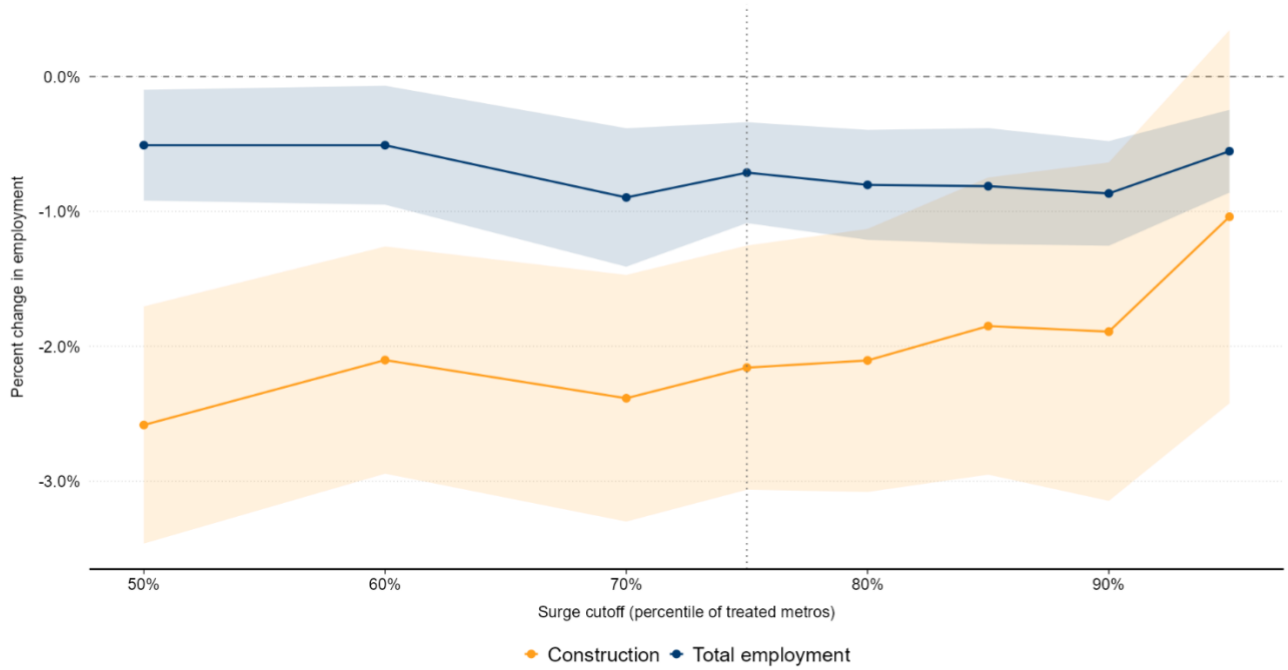


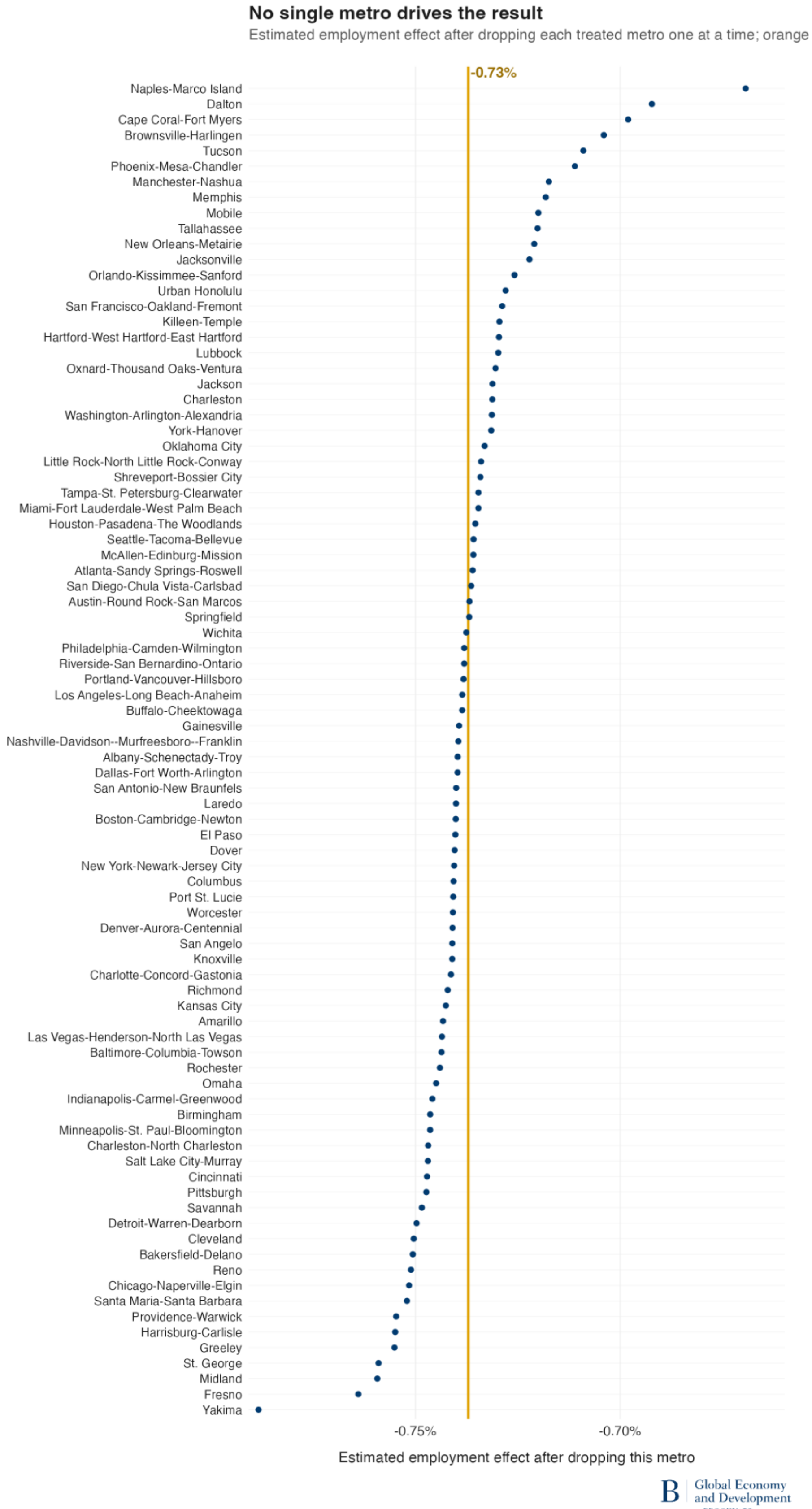
Other tests

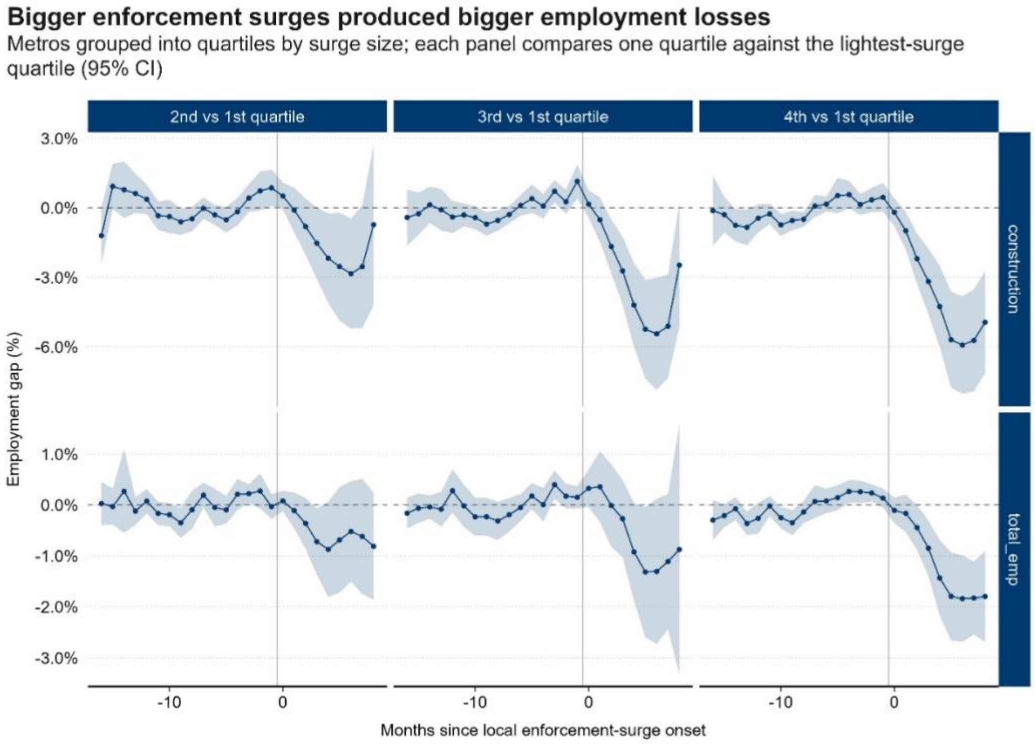
We also perform three other tests on our estimator that provide additional evidence that the outcome is robust. We show: (i) that the cutoff sensitivity across p50-p90 thresholds does not move the estimate significantly (shown below); (ii) that a quartile dose response shows monotonically decreasing estimates when the least enforced CBSAs are compared against more ICE-intensive CBSAs; (iii) that a leave-one-out by CBSA analysis does not move the estimate more than 10% for any missing CBSA.

The result holds across reasonable surge-cutoff choices

Estimated employment effect (95% CI) at each cutoff; dotted line = headline 75th-percentile cutoff




No single metro drives the result
Estimated employment effect after dropping each treated metro one at a time; orange
-0.73%
Naples-Marco Island
Dalton
Cape Coral-Fort Myers
Brownsville-Harlingen
Tucson
Phoenix-Mesa-Chandler
Manchester-Nashua
Memphis
Mobile
Tallahassee
New Orleans-Metairie
Jacksonville
Orlando-Kissimmee-Sanford
Urban Honolulu
San Francisco-Oakland-Fremont
Killeen-Temple
Hartford-West Hartford-East Hartford
Lubbock
Oxnard-Thousand Oaks-Ventura
Jackson
Charleston
Washington-Arlington-Alexandria
York-Hanover
Oklahoma City
Little Rock-North Little Rock-Conway
Shreveport-Bossier City
Tampa-St. Petersburg-Clearwater
Miami-Fort Lauderdale-West Palm Beach
Houston-Pasadena-The Woodlands
Seattle-Tacoma-Bellevue
McAllen-Edinburg-Mission
Atlanta-Sandy Springs-Roswell
San Diego-Chula Vista-Carlsbad
Austin-Round Rock-San Marcos
Springfield
Wichita
Philadelphia-Camden-Wilmington
Riverside-San Bernardino-Ontario
Portland-Vancouver-Hillsboro
Los Angeles-Long Beach-Anaheim
Buffalo-Cheektowaga
Gainesville
Nashville-Davidson--Murfreesboro--Franklin
Albany-Schenectady-Troy
Dallas-Fort Worth-Arlington
San Antonio-New Braunfels
Laredo
Boston-Cambridge-Newton
El Paso
Dover
New York-Newark-Jersey City
Columbus
Port St. Lucie
Worcester
Denver-Aurora-Centennial
San Angelo
Knoxville
Charlotte-Concord-Gastonia
Richmond
Kansas City
Amarillo
Las Vegas-Henderson-North Las Vegas
Baltimore-Columbia-Towson
Rochester
Omaha
Indianapolis-Carmel-Greenwood
Birmingham
Minneapolis-St. Paul-Bloomington
Charleston-North Charleston
Salt Lake City-Murray
Cincinnati
Pittsburgh
Savannah
Detroit-Warren-Dearborn
Cleveland
Bakersfield-Delano
Reno
Chicago-Naperville-Elgin
Santa Maria-Santa Barbara
Providence-Warwick
Harrisburg-Carlisle
Greeley
St. George
Midland
Fresno
Yakima
-0.75%
-0.70%
Estimated employment effect after dropping this metro
Global Economy and Development at BROOKINGS



Appendix A7: Arrest to job ratios

We report two summaries of the jobs-per-arrest relationship:

M_{post} : The window-matched stock-on-stock multiplier. The numerator is the simple ATT on employment across the post-horizon, scaled by baseline jobs to recover an estimate of average missing incumbent jobs by CBSA after an increase in ICE enforcement. The denominator is the sum of the event time ATT of arrests for the treated CBSAs, which estimates the cumulative excess arrests over the same window.

$$M_{\text{post}} = \frac{\left(1 - \exp \widehat{ATT}^{\text{simple}}_{\text{emp}}\right) \cdot \bar{J}^{*}}{\sum_{e \in \mathcal{E}} \widehat{ATT}^{\text{arr}}_{\text{dyn}}(e)}$$

Where E = {0,1,...,8}

$M_{plateau}(6)$: The same stock-on-stock multiplier idea but evaluated at six months post-onset and restricted to the 51 CBSAs observed through that horizon. In this instance, we only use the dynamic ATT on employment at e = 6 in the numerator and the sum of the dynamic ATT of arrests in the denominator from e = 0 to e = 6

$$M_{\text{plateau}}(e^*) \;=\; \frac{\left(1-\exp \widehat{ATT}^{\text{dyn}}_{\text{emp}}(e^*)\right)\cdot \bar{J}^*}{\sum_{e=0}^{e^*} \widehat{ATT}^{\text{arr}}_{\text{dyn}}(e)}.$$

Both estimates apply to the treated metros only over the time window of the analysis. They are not extrapolatable to untreated metros and do not constitute a national multiplier.

We derive the confidence intervals for each point estimate with a bootstrap approach, resampling the panel with replacement (n = 500).

Excess arrests in treated metros, by months since onset

		Surge above 2024 baseline		DiD estimate	
Event time (e)	Treated CBSAs observed	Surge: per month	Surge: cumulative	ATT: per month	ATT: cumulative
0	86	10,169	10,169	8,202	8,202
1	86	8,835	19,004	6,841	15,043
2	86	7,685	26,690	5,694	20,737
3	86	8,452	35,142	6,416	27,153
4	72	9,083	44,225	8,076	35,229
5	56	5,698	49,923	5,130	40,359
6	51	4,647	54,571	4,190	44,549
7	45	5,213	59,783	4,840	49,389
8	16	2,283	62,066	2,372	51,761

Jobs lost per excess arrest in treated metros

Metric	Treated CBSAs	Jobs lost per arrest	Lower 95%	Upper 95%
Average post-period	86	9.05	4.25	16.14
Plateau at month 6	51	29.93	16.73	52.28