Beyond arrests: How ICE enforcement depressed local employment in 2025

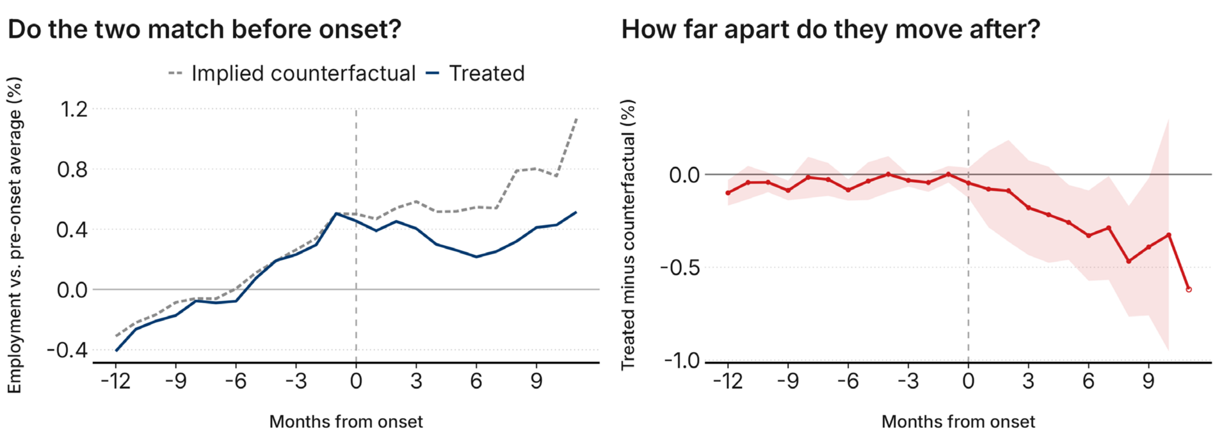
Appendices: Robustness and tables

Several additional analyses reinforce the interpretation that the employment shortfall reflects the local enforcement surge rather than a modeling artifact or an unrelated economic shock.

Appendix A: Testing the pre-surge trend

Our preferred estimation method relies on the assumption that the pre-trend differences between surge and control cities observed in the year prior to enforcement (shown in Figure 1) would have continued absent enforcement. Though a reversal of a year-long trend precisely at the time of enforcement is unlikely, we test this assumption with an estimate using synthetic controls, which builds each surge cohort's counterfactual from a weighted blend of comparison cities rather than from the trend extrapolation. Figure 4 shows that the synthetic control estimates produce the same substantive pattern though with somewhat smaller magnitude.

Figure 4: Results are robust to a synthetic control test



Though this approach arguably better captures the counterfactual post-surge employment path, it also likely gives a conservative estimate because the comparison cities given the greatest weight are those that share a pre-period trajectory similar to surge cities' trajectories. Since this may be correlated with ICE arrests, it is possible that heavily weighted comparison cities were themselves subject to ICE enforcement to a lesser extent than surge cities, but to a greater extent than the average of the 277 control cities, whose employment trajectories determine the counterfactual for our preferred specification. In the synthetic controls approach, if employment in these closely matched comparison cities was also dampened by ICE enforcement, using them to form the counterfactual artificially narrows the gap between them and surge cities, yielding a more conservative estimate of the employment shortfall.

Figure 5: Employment declines remain after accounting for each metro's pre-surge trend.

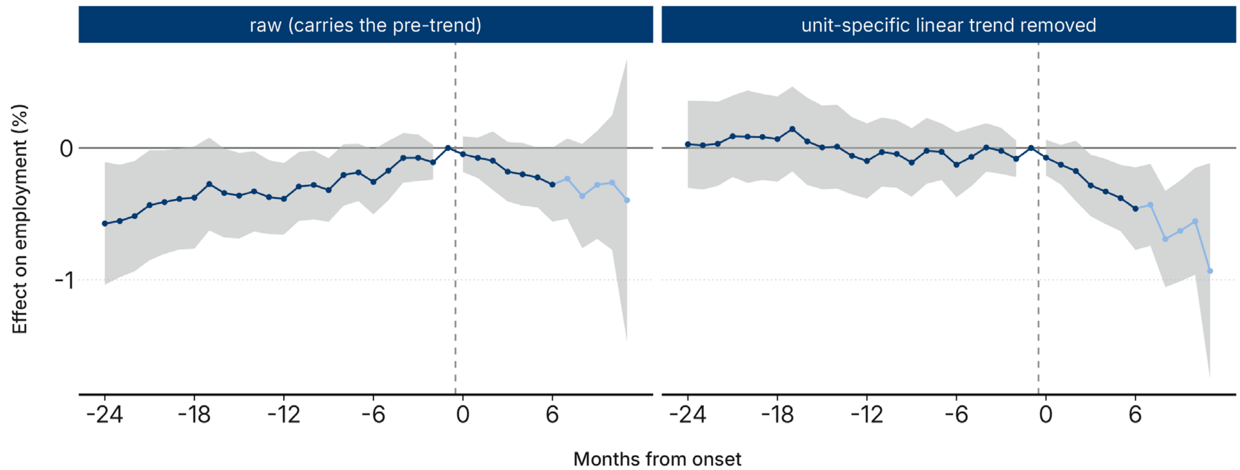


Figure 5 compares the CS estimates (shown in Figure 1 and in Figure 5's left panel) to estimates obtained after subtracting each city's pre-surge employment trend (right panel). By contrast, the headline estimates are made in comparison to an extrapolation of the *average* pre-surge difference in employment trajectories between surge and control cities. Along with the synthetic controls estimates, results indicate that the main result is not an artifact of pre-existing differences in employment trends between surge and comparison cities.

Figure 6: How far the trend can bend before results are made insignificant

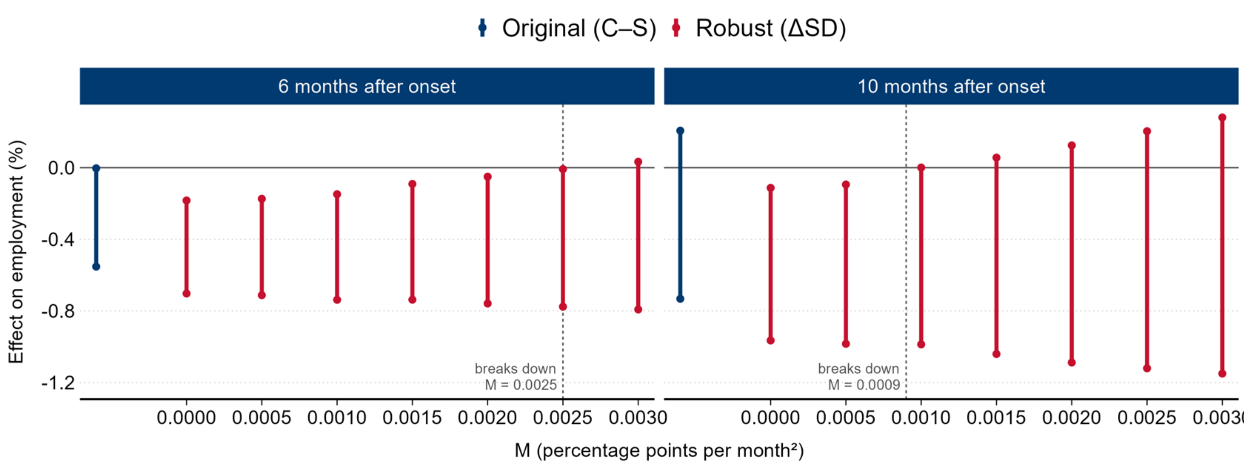
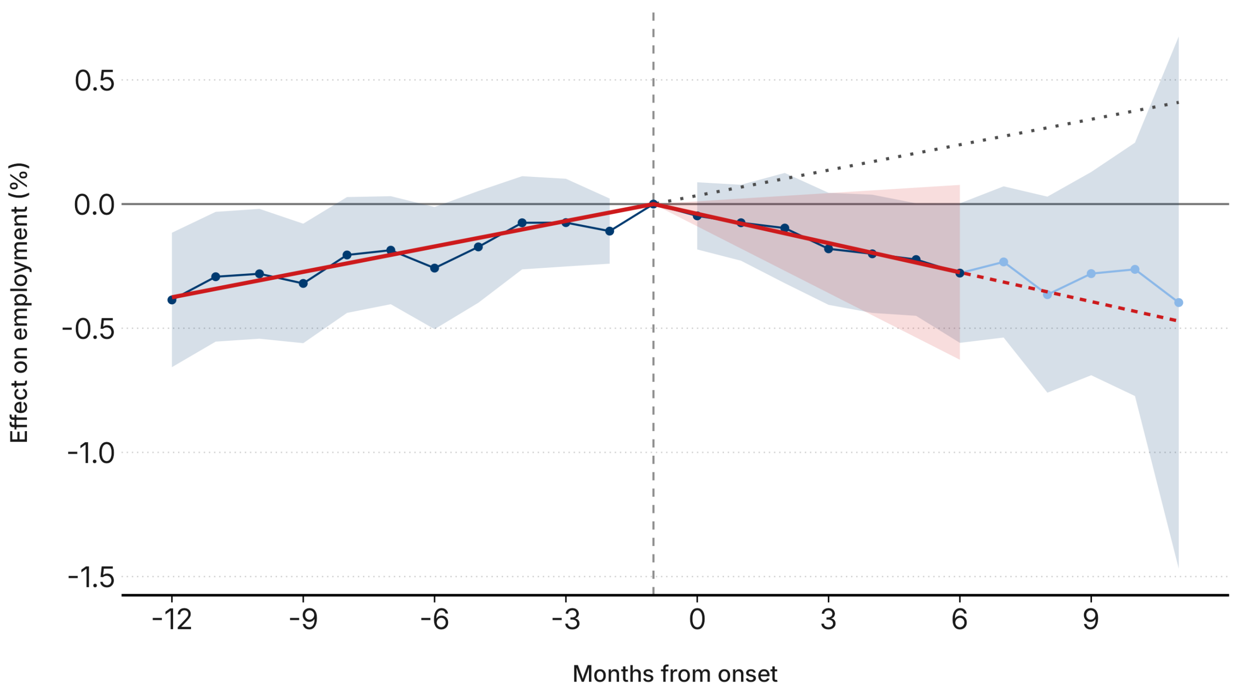


Figure 6 shows results of a test following Rambachan and Roth (2023) that curves the trend to ask how far it would need to bend before the results became insignificant. Estimates at six months remain significant even if the post-surge trend deviates from its linear extrapolation by up to M = 0.0025 percentage points per month between consecutive months.

Appendix B: Estimating a trend break and testing its significance

Visually, Figure 1 shows a clear break in the trajectory of surge and control cities' relative employment. Figure 7 shows the results of a formalization of that visual pattern into a statistical test. We fit a piecewise-linear model to the CS event-study coefficients with a trend break specified at the month ICE enforcement began.

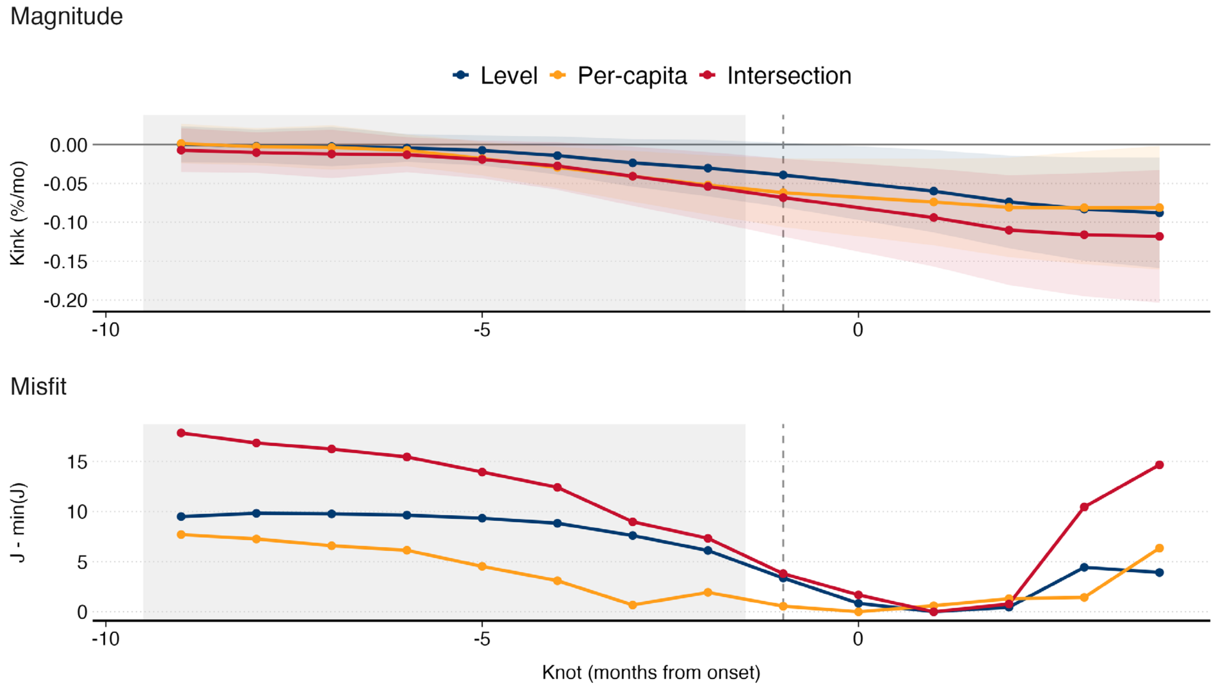
Figure 7: Trend break analysis shows significant downturn when enforcement begins.



The figure shows that before enforcement arrived, employment in surge cities was growing relative to control cities at a rate of about 0.41% annually (0.034% per month) the year before the onset of ICE enforcement. Afterward, surge cities lose employment relative to comparison cities at a rate of about 0.47% annually (0.039% per month) during the first six months after enforcement onset. The estimated change in slope is -0.073 percentage points per month (95% confidence interval: −0.12 to −0.02). Figure 7 also shows a joint confidence band (shaded red) around the fitted path showing range within which the entire post-surge path (as opposed to individual estimates) falls with 95% probability. This is the appropriate test of whether employment moved off its prior trajectory over the post-surge window.

The trend-break model in Figure 7 places the break at the month of each city's actual surge. Figure 8 shows results of a “knot scan” test, which repeats the trend-break analysis while varying the placement of the break across months to confirm that the sharpest change in trajectory falls at, or in the months immediately following, the surge's actual onset rather than at some other point elsewhere in the window.

Figure 8: Testing alternative trend break locations shows salience of months immediately following enforcement.



The top panel of Figure 8 shows how the degree to which the slopes of the two line segments differ varies with the placement of the break over each month in the event window. The bottom panel shows the results of measuring how well the trend-break model fits the CS estimates for each of the same trend break placements. If the break were coincidental, or if it reflected some unrelated shift in the data, the best-fitting kink would fall elsewhere. Instead, the trough shows the trend-break model best fits the data when placed shortly after enforcement.

Figure 9: Random control treatment group assignment

1,000 reshuffles of which metros got an arrest surge

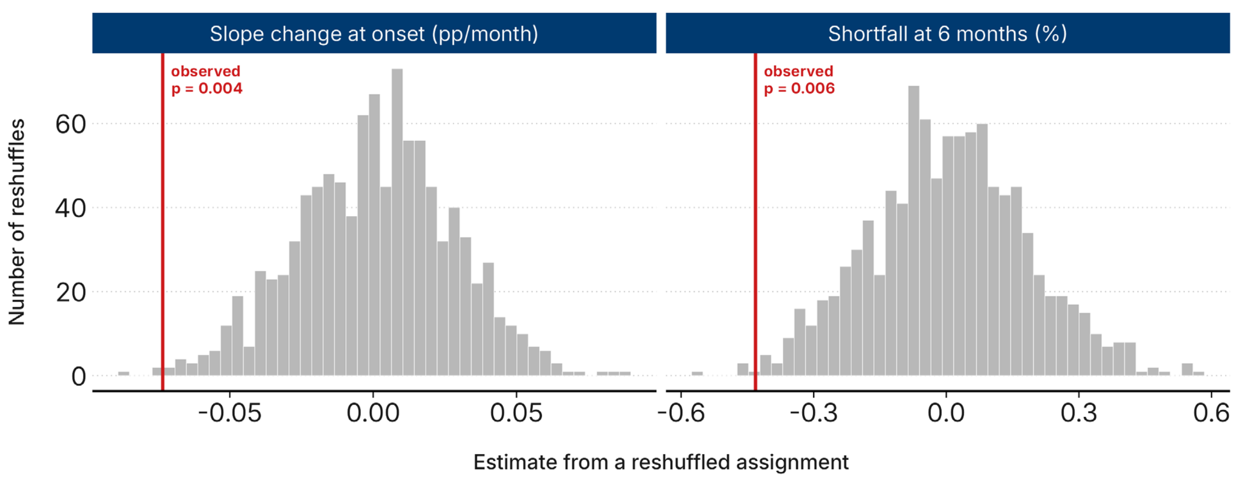


Figure 9 shows the results of a randomization-inference test that asks how often an effect as large as the one we observe would arise purely by chance. That is, could a break this large arise between two arbitrary groups of cities, entirely by chance? We construct 1000 random, "placebo" assignments of surge and control cities and then repeat the estimation (right panel) as well as the trend-break estimations (left panel). For both methods, the estimated effect (or trend break) based on actual, arrest-based surge and control group assignments is substantially more negative than one in 200 random placebo assignments.

Appendix C: Estimate of American-born employment shortfall

Recent literature on immigration enforcement gives independent justification to expect that a substantial share of the missing jobs would have been held by U.S. born workers. Extrapolating from Studies of Secure Communities (Zipper 20205) found that a significant share of total job loss fell on U.S. born workers with estimates falling between 44% over all sectors, 38% for construction, and 81% for childcare.

The QCEW data used in this analysis measure payroll employment and do not identify nativity. As such, the data do not allow estimates of how many of the missing jobs in our results would have been held by American-born workers. However, as shown in Table 1, applying Cox and East's (2026) results implies that roughly 900 jobs missing in the median surge city would have been held by American-born men.

Using CPS data, Cox and East estimate that state and sub-state-level ICE enforcement reduced the employment rate of American-born men by 0.65% (0.6pp on a 92.8% base) after the onset of enforcement in 2025. We estimate that there are around 140,168 U.S.-born men working in the typical surge city, derived by looking at the share of U.S. born men from ACS data in the surge metros in our panel applied to total payroll jobs in the median surge city.

Table 1: An estimate of lost jobs that would have been held by American-born workers

The median surge city, six months after its local enforcement surge. The three rows are estimated separately

	People working	Employment fell by	Jobs lost
All workers	342,000	0.43%[a]	1,480
U.S.-born men	140,200[b]	0.65%[c]	910
Likely undocumented men[d]	21,500[b]	3.02%[e]	650[f]

[a] This paper. Cumulative employment shortfall against the extrapolated pre-surge trend, six months after local surge onset.

[b] Group shares of employment in these metros, from ACS (ages 20-64, employed, excluding government and military), applied to the city's payroll employment: U.S.-born men 41.0%, likely undocumented men 6.3%.

[c] Cox and East (2026): a 0.6 percentage-point decline on a 92.8 percent base.

[d] Defined as foreign born men with at most high school education.

[e] Cox and East (2026): a 2.8 percentage-point decline on the same base. It measures only workers who stayed in the country and answered the survey.

[f] Adding the author's estimate of departures raises this estimate to 1,020 jobs.

Employment: Lightcast monthly metro employment, 2024 average, median of the 64 surge cities.

This estimate should be interpreted cautiously. It is a scale comparison across two studies with different samples, outcomes, and treatment definitions. Cox and East estimate an average effect over the first six months following enforcement, whereas our estimate measures the employment shortfall at month six. Cox and East count an individual as employed only if they report for work in the prior week, so a worker who keeps their job but stays home counts as not employed, whereas the payroll-based QCEW data would count the same worker as employed.

Appendix D: Determining the job shortfall relative to arrests

We estimate cumulative excess arrests in the average treated metro using the same unweighted Callaway-Sant'Anna specification with arrests per worker as the outcome, summing the average treatment effects over event times zero to six.

Enforcement intensity has one extreme outlier: Laredo, TX, a small border metro with a workforce of roughly 107,000, experienced about 56 excess arrests per 1,000 workers over this window, more than 11 times the rate of the next highest metro (Alexandria, LA, at 4.8). Under unweighted aggregation, a single observation of this magnitude would dominate the panel-wide average. We therefore winsorize arrest intensity at the second-largest order statistic: Laredo's arrests per worker are set equal to the next-highest metro's rate, and its monthly arrest counts are rescaled proportionally. The resulting estimate is nearly identical to the estimate excluding Laredo altogether.

Appendix E: Estimates by month and under alternative specifications

Table 2 tracks how the employment shortfall evolves month by month after a local enforcement surge. The first and second columns show estimates from the CS event study relative to a linear extrapolation of the pre-surge trend. The first, main specification uses a conventional OLS projection, while the second uses Rambachan and Roth's (2023) framework to produce more precise estimates by incorporating the estimated covariance across months. Columns three and four show results derived by imposing a linear trend break at the surge month (Appendix B) to show the employment shortfall implied by that change in relative employment trajectories, with column three showing the post-surge trend estimated over the balanced six-month window, while column four shows results derived from the full panel.

Table 2

Employment effect by months since local enforcement surge

Intersection design, X-13 seasonal adjustment, 64 treated metros

			Net of pre-trend: OLS contrast		Net of pre-trend: FLCI (M = 0)		Kink x h, balanced		Kink x h, unbalanced	
Month	Surge cities	C-S	Est.	95% CI	Est.	95% CI	Est.	95% CI	Est.	95% CI
0	64	−0.048	−0.031	[-0.233, +0.170]	−0.027	[-0.150, +0.097]	−0.073	[-0.124, -0.023]	−0.067	[-0.114, -0.019]
1	64	−0.075	−0.087	[-0.292, +0.118]	−0.077	[-0.211, +0.058]	−0.147	[-0.247, -0.046]	−0.133	[-0.228, -0.038]
2	64	−0.097	−0.137	[-0.387, +0.113]	−0.146	[-0.346, +0.054]	−0.220	[-0.371, -0.069]	−0.200	[-0.342, -0.057]
3	64	−0.180	−0.249	[-0.504, +0.006]	−0.268	[-0.472, -0.064]	−0.294	[-0.495, -0.093]	−0.266	[-0.457, -0.076]
4	64	−0.200	−0.297	[-0.560, -0.035]	−0.341	[-0.553, -0.129]	−0.367	[-0.618, -0.116]	−0.333	[-0.571, -0.095]
5	64	−0.223	−0.349	[-0.653, -0.044]	−0.352	[-0.581, -0.124]	−0.440	[-0.742, -0.139]	−0.400	[-0.685, -0.114]
6	64	−0.278	−0.432	[-0.762, -0.102]	−0.443	[-0.703, -0.183]	−0.514	[-0.866, -0.162]	−0.466	[-0.799, -0.133]
7	55	−0.233	−0.415	[-0.747, -0.083]	−0.392	[-0.668, -0.116]	−0.587	[-0.989, -0.185]	−0.533	[-0.913, -0.152]
8	41	−0.365	−0.575	[-0.972, -0.179]	−0.488	[-0.831, -0.144]	−0.661	[-1.113, -0.208]	−0.599	[-1.027, -0.171]
9	39	−0.280	−0.519	[-0.950, -0.087]	−0.418	[-0.793, -0.043]	−0.734	[-1.237, -0.231]	−0.666	[-1.141, -0.190]
10	31	−0.263	−0.530	[-1.018, -0.041]	−0.539	[-0.965, -0.113]	−0.807	[-1.360, -0.254]	−0.733	[-1.256, -0.209]
11	11	−0.397	−0.692	[-1.645, +0.261]	−0.676	[-1.491, +0.138]	−0.881	[-1.484, -0.278]	−0.799	[-1.370, -0.228]

Note: Estimates are percentage differences in payroll employment relative to the counterfactual. "C-S" gives the cohort-aggregated, staggered, dynamic differences-in-differences estimate assuming no pre-surge trend. The next two columns report the gap between that estimate and the employment path implied by the fitted pre-surge trend, using conventional OLS inference and Rambachan–Roth fixed-length confidence intervals under M=0, respectively. The final two columns report the shortfall implied by the change in slope after surge onset using the balanced six-month panel and unbalanced panel, respectively.

Table 3 shows the stability of the main result over several specifications reflecting choices made in the analysis: Surge city definition, seasonal adjustment, pre-surge trend adjustment, and control-pool composition. Defining surge cities by absolute arrests alone, per worker arrests alone, or their intersection all give results in the same direction with similar in magnitude.

Estimates using X-13 are more precise than those using calendar-month fixed effects, because the latter cannot capture seasonal patterns that grow or shrink year over year and so leave more seasonal noise in the estimates. Seasonal factors are estimated between January 2019 and December 2024 and applied to the full series with X-13's automatic outlier detection enabled to prevent pandemic employment from distorting the pattern. Ending the calibration window before any 2025 enforcement surges occur ensures the seasonal adjustments do not absorb any employment decline caused by enforcement. And applying the pre-estimated seasonal factors to the full 2025 series then ensures that subsequent effect estimates can capture effects that may themselves vary with time of year (as opposed to estimating a specification that includes month fixed effect terms).

Estimates are broadly similar in magnitude whether surge cities are defined by absolute arrests, per worker arrests, or the intersection of the two, as are estimates from alternative control groups that drop single-rule surge metros, drop controls bordering a surge metro, or restrict the control pool to lower-arrest cities. As a final test, the placebo result shows that shifting surge onsets back 12 months produces a null result, which ought to be the case if the main result reflects effects of an enforcement surge rather than a spurious feature of the data or research design.

Table 3

	Surge cities	Comparison cities	Slope change at onset (pp/month)	Shortfall at 6 months		Shortfall at 10 months	
				OLS contrast	FLCI (M = 0)	OLS contrast	FLCI (M = 0)
Preferred specification							
Intersection, X-13	64	277	-0.073 [-0.124, -0.023]*	-0.43 [-0.76, -0.10]*	-0.44 [-0.70, -0.18]*	-0.53 [-1.02, -0.04]*	-0.54 [-0.97, -0.11]*
Alternative surge-city definitions (X-13)							
Absolute excess arrests	86	255	-0.042 [-0.084, -0.000]*	-0.30 [-0.58, -0.03]*	-0.29 [-0.52, -0.07]*	-0.47 [-0.87, -0.06]*	-0.45 [-0.80, -0.09]*
Per-worker excess arrests	86	255	-0.062 [-0.107, -0.018]*	-0.41 [-0.71, -0.11]*	-0.41 [-0.66, -0.17]*	-0.46 [-0.91, -0.00]*	-0.43 [-0.84, -0.01]*
Absolute only (not rate)	22	255	0.050 [0.010, 0.090]*	0.07 [-0.24, 0.39]	0.20 [-0.02, 0.41]	-0.27 [-0.70, 0.17]	-0.05 [-0.38, 0.29]
Rate only (not absolute)	22	255	-0.029 [-0.111, 0.054]	-0.30 [-0.86, 0.25]	-0.09 [-0.47, 0.29]	-0.31 [-1.16, 0.55]	0.46 [-0.26, 1.18]
Alternative pre-trend handling (intersection, X-13)							
City-specific trends	64	277	-0.073 [-0.124, -0.023]*	-0.43 [-0.76, -0.10]*	-0.44 [-0.70, -0.18]*	-0.54 [-1.00, -0.07]*	-0.50 [-0.89, -0.11]*
Synthetic control	64	277	—	-0.33 [-0.57, -0.09]*	—	-0.33 [-0.95, 0.30]	—
Counterfactual through onset (X-13)	64	277	-0.090 [-0.150, -0.029]*	-0.41 [-0.68, -0.15]*	—	-0.51 [-0.92, -0.09]*	—
Counterfactual through the fitted break, +2 (X-13)	64	277	-0.142 [-0.233, -0.050]*	-0.34 [-0.55, -0.13]*	—	-0.41 [-0.79, -0.03]*	—
Alternative pre-trend window (intersection, X-13)							
18 months	64	277	-0.066 [-0.112, -0.019]*	-0.30 [-0.64, 0.03]	-0.35 [-0.59, -0.11]*	-0.36 [-0.86, 0.14]	-0.35 [-0.75, 0.04]
24 months	64	277	-0.064 [-0.109, -0.019]*	-0.32 [-0.65, 0.01]	-0.45 [-0.65, -0.24]*	-0.38 [-0.86, 0.10]	-0.52 [-0.86, -0.18]*
Control-pool composition / spillover (intersection, X-13)							
Drop single-rule surge metros	64	233	-0.075 [-0.126, -0.024]*	-0.45 [-0.79, -0.11]*	-0.45 [-0.71, -0.19]*	-0.55 [-1.04, -0.05]*	-0.53 [-0.96, -0.10]*
Drop controls bordering a surge metro	64	199	-0.079 [-0.130, -0.028]*	-0.45 [-0.79, -0.12]*	-0.46 [-0.73, -0.20]*	-0.56 [-1.06, -0.06]*	-0.56 [-1.00, -0.13]*
Drop late-surging controls	64	262	-0.073 [-0.124, -0.023]*	-0.44 [-0.77, -0.11]*	-0.45 [-0.71, -0.19]*	-0.54 [-1.03, -0.05]*	-0.55 [-0.97, -0.12]*
Controls below median arrests	64	170	-0.084 [-0.136, -0.032]*	-0.49 [-0.83, -0.14]*	-0.46 [-0.73, -0.19]*	-0.63 [-1.14, -0.12]*	-0.59 [-1.03, -0.16]*
Controls below 25th pct arrests	64	85	-0.087 [-0.144, -0.030]*	-0.49 [-0.85, -0.12]*	-0.51 [-0.81, -0.20]*	-0.56 [-1.12, -0.01]*	-0.58 [-1.05, -0.10]*
Placebo							
Onsets shifted back 12 months	64	277	0.013 [-0.020, 0.047]	-0.18 [-0.50, 0.14]	-0.05 [-0.27, 0.17]	-0.10 [-0.55, 0.36]	-0.01 [-0.39, 0.37]
Alternative seasonal adjustment (month fixed effects, pre-2025)							
Intersection	64	277	-0.086 [-0.156, -0.015]*	-0.32 [-0.77, 0.14]	-0.23 [-0.56, 0.10]	-0.45 [-1.02, 0.12]	-0.38 [-0.84, 0.09]
Absolute excess arrests	86	255	-0.053 [-0.110, 0.005]	-0.20 [-0.57, 0.17]	-0.13 [-0.40, 0.14]	-0.40 [-0.87, 0.06]	-0.30 [-0.69, 0.09]
Per-worker excess arrests	86	255	-0.063 [-0.125, -0.001]*	-0.29 [-0.69, 0.11]	-0.25 [-0.55, 0.04]	-0.35 [-0.85, 0.15]	—
18-month pre-trend window	64	277	-0.079 [-0.142, -0.015]*	-0.31 [-0.69, 0.08]	-0.26 [-0.51, -0.01]*	-0.44 [-0.90, 0.03]	-0.48 [-0.84, -0.12]*
24-month pre-trend window	64	277	-0.076 [-0.138, -0.014]*	-0.30 [-0.70, 0.09]	-0.23 [-0.45, -0.02]*	-0.43 [-0.90, 0.04]	-0.42 [-0.73, -0.12]*
Counterfactual through onset	64	277	-0.107 [-0.191, -0.023]*	-0.41 [-0.75, -0.06]*	—	-0.57 [-0.99, -0.14]*	—
Counterfactual through the fitted break, +2	64	277	-0.172 [-0.298, -0.046]*	-0.39 [-0.65, -0.13]*	—	-0.54 [-0.90, -0.19]*	—

Note: The main specification defines surge cities as those in the top quartile of 2024-2025 growth in both at-large ICE arrests and at-large arrests per worker using X-13 seasonally adjusted QCEW employment (Intersection, X-13) with effects estimated using the Callaway–Sant'Anna staggered difference-in-differences procedure as a percentage deviation from a conventional linear extrapolation of a pre-surge trend (OLS contrast).

Metro area	State	Onset month	Employment (Lightcast 2024)	Immigrant workers (ACS)	Baseline arrests (2024)	Total arrests, onset months 0-6	Excess (level)	Excess /1k workers	Employment effect at 6 months (jobs, -0.432%) Note: Here we apply our estimated average effect to each city's baseline employment. Our methods do not pinpoint the job losses in each individual city.
Albany-Schenectady-Troy	NY	May 2025	449,554	49,857	131	511	435	0.97	-1,941
Alexandria	LA	Feb 2025	59,848	2,645	13	296	289	4.82	-258
Amarillo	TX	May 2025	123,026	16,284	71	163	122	0.99	-531
Atlanta-Sandy Springs-Roswell	GA	Feb 2025	2,874,559	676,652	838	2,537	2,048	0.71	-12,410
Austin-Round Rock-San Marcos	TX	Feb 2025	1,264,679	276,292	152	791	702	0.56	-5,460
Bakersfield-Delano	CA	Jun 2025	347,664	87,647	73	527	484	1.39	-1,501
Baltimore-Columbia-Towson	MD	May 2025	1,354,989	233,753	734	1,395	967	0.71	-5,850
Boston-Cambridge-Newton	MA-NH	May 2025	2,708,466	673,776	818	3,725	3,248	1.20	-11,693
Brownsville-Harlingen	TX	Feb 2025	156,620	49,588	233	691	555	3.54	-676
Buffalo-Cheektowaga	NY	Feb 2025	534,206	51,551	50	516	487	0.91	-2,306
Cape Coral-Fort Myers	FL	Apr 2025	299,283	89,777	114	568	501	1.67	-1,292
Charleston	WV	Mar 2025	104,290	2,218	15	221	212	2.03	-450
Charleston-North Charleston	SC	Feb 2025	386,862	35,959	210	450	327	0.84	-1,670
Charlotte-Concord-Gastonia	NC-SC	Mar 2025	1,339,735	220,533	360	911	701	0.52	-5,784
Columbus	GA-AL	May 2025	115,684	10,563	2	190	189	1.64	-499
Dalton	GA	Jan 2025	66,081	11,574	155	215	125	1.88	-285
Denver-Aurora-Centennial	CO	Jan 2025	1,607,996	249,893	971	1,316	749	0.47	-6,942
Dover	DE	Apr 2025	71,245	12,299	6	261	258	3.62	-308
El Paso	TX	Jun 2025	335,190	83,955	152	1,165	1,076	3.21	-1,447
Fresno	CA	Mar 2025	483,934	131,205	160	572	479	0.99	-2,089
Gainesville	GA	Mar 2025	101,986	23,403	96	258	202	1.99	-440
Harrisburg-Carlisle	PA	Mar 2025	335,867	40,052	28	156	139	0.42	-1,450
Hartford-West Hartford-East Hartford	CT	Feb 2025	596,369	136,048	188	355	246	0.41	-2,575
Houston-Pasadena-The Woodlands	TX	May 2025	3,254,323	1,081,522	648	6,626	6,247	1.92	-14,050
Jackson	MS	Jan 2025	273,332	9,658	29	142	125	0.46	-1,180
Killeen-Temple	TX	Feb 2025	149,785	32,527	106	150	88	0.59	-647
Knoxville	TN	Jan 2025	420,319	31,278	185	269	161	0.38	-1,815
Laredo	TX	Jun 2025	107,433	37,420	128	6,123	6,048	56.30	-464
Little Rock-North Little Rock-Conway	AR	Jan 2025	355,882	27,050	105	428	366	1.03	-1,536
Los Angeles-Long Beach-Anaheim	CA	Jun 2025	6,186,987	2,303,544	1,788	5,057	4,014	0.65	-26,711
Lubbock	TX	Feb 2025	165,668	15,817	139	370	289	1.75	-715
Manchester-Nashua	NH	Jan 2025	201,342	33,390	18	108	97	0.48	-869
McAllen-Edinburg-Mission	TX	Jan 2025	291,901	122,560	177	381	277	0.95	-1,260
Miami-Fort Lauderdale-West Palm Beach	FL	Jan 2025	2,788,979	1,589,735	1,522	2,139	1,251	0.45	-12,041
Midland	TX	Jun 2025	120,666	16,658	54	172	141	1.17	-521
Mobile	AL	Feb 2025	175,327	9,289	46	240	213	1.21	-757
Myrtle Beach-Conway-North Myrtle Beach	SC	Feb 2025	144,489	16,411	159	273	180	1.25	-624
Naples-Marco Island	FL	Mar 2025	171,813	60,626	648	605	227	1.32	-742
Nashville-Davidson--Murfreesboro--Frankli	TN	May 2025	1,095,192	114,199	270	796	639	0.58	-4,728
New Orleans-Metairie	LA	Mar 2025	443,647	47,669	311	627	446	1.00	-1,915
Oklahoma City	OK	Jan 2025	668,259	84,809	492	974	687	1.03	-2,885
Omaha	NE-IA	May 2025	490,925	54,384	98	364	308	0.63	-2,119
Orlando-Kissimmee-Sanford	FL	Feb 2025	1,396,330	458,503	215	1,702	1,577	1.13	-6,028
Oxnard-Thousand Oaks-Ventura	CA	Jun 2025	336,272	103,313	146	934	849	2.52	-1,452
Philadelphia-Camden-Wilmington	PA-NJ-DE-MD	Feb 2025	2,868,691	521,762	490	1,636	1,350	0.47	-12,385
Port St. Lucie	FL	Jan 2025	166,844	52,193	814	1,232	758	4.54	-720
Providence-Warwick	RI-MA	Feb 2025	708,914	141,072	170	372	273	0.38	-3,061
Reno	NV	May 2025	273,607	52,431	77	186	141	0.52	-1,181
Richmond	VA	Feb 2025	658,880	85,519	87	1,122	1,071	1.63	-2,845
Riverside-San Bernardino-Ontario	CA	Jun 2025	1,686,340	528,473	424	1,523	1,276	0.76	-7,280

Metro area	State	Onset month	Employment (Lightcast 2024)	Immigrant workers (ACS)	Baseline arrests (2024)	Total arrests, onset months 0-6	Excess (level)	Excess /1k workers	Employment effect at 6 months (jobs, -0.432%) Note: Here we apply our estimated average effect to each city's baseline employment. Our methods do not pinpoint the job losses in each individual city.
Rochester	NY	Mar 2025	495,442	50,732	22	259	246	0.50	-2,139
San Angelo	TX	Feb 2025	50,969	5,445	4	155	153	3.00	-220
San Antonio-New Braunfels	TX	Jun 2025	1,116,379	218,848	1,453	4,521	3,674	3.29	-4,820
San Diego-Chula Vista-Carlsbad	CA	May 2025	1,536,541	394,935	371	4,681	4,465	2.91	-6,634
Santa Maria-Santa Barbara	CA	May 2025	216,323	60,464	21	197	185	0.85	-934
Savannah	GA	May 2025	196,222	23,817	31	392	374	1.91	-847
St. George	UT	Jan 2025	86,509	9,490	12	144	137	1.58	-373
Tallahassee	FL	May 2025	189,751	16,158	235	776	639	3.37	-819
Tucson	AZ	Feb 2025	386,312	78,484	120	216	145	0.38	-1,668
Washington-Arlington-Alexandria	DC-VA-MD-WV	Feb 2025	3,191,465	1,058,487	660	3,406	3,021	0.95	-13,779
Wichita	KS	Feb 2025	305,044	26,855	49	145	116	0.38	-1,317
Worcester	MA	May 2025	351,477	113,868	47	234	207	0.59	-1,517
Yakima	WA	Jun 2025	116,547	30,113	209	660	538	4.62	-503
York-Hanover	PA	Feb 2025	182,614	18,278	7	125	120	0.66	-788