Beyond arrests: How ICE enforcement depressed local employment in 2025

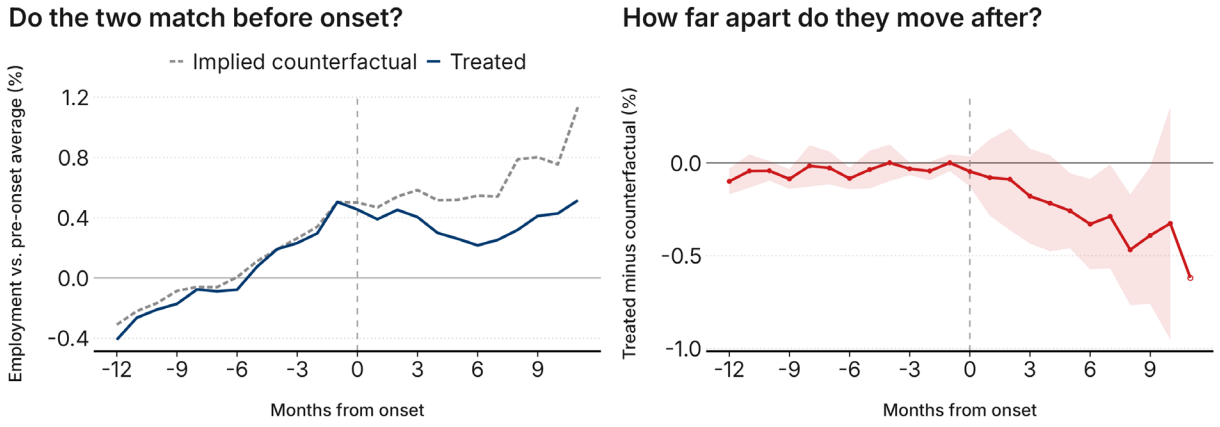
Appendices: Robustness and tables

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.

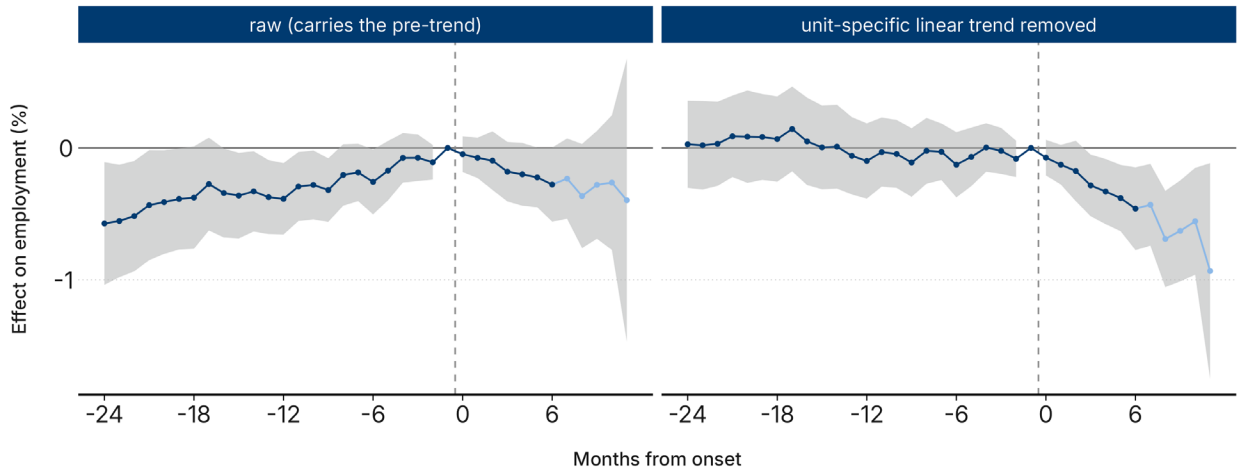


Figure 5 compares the CS estimates (shown in Figure 1 and in Figure 5's left panel) to estimates obtained after subtracting each city's pre-surge employment trend (right panel). By contrast, the headline estimates are made in comparison to an extrapolation of the *average* pre-surge difference in employment 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

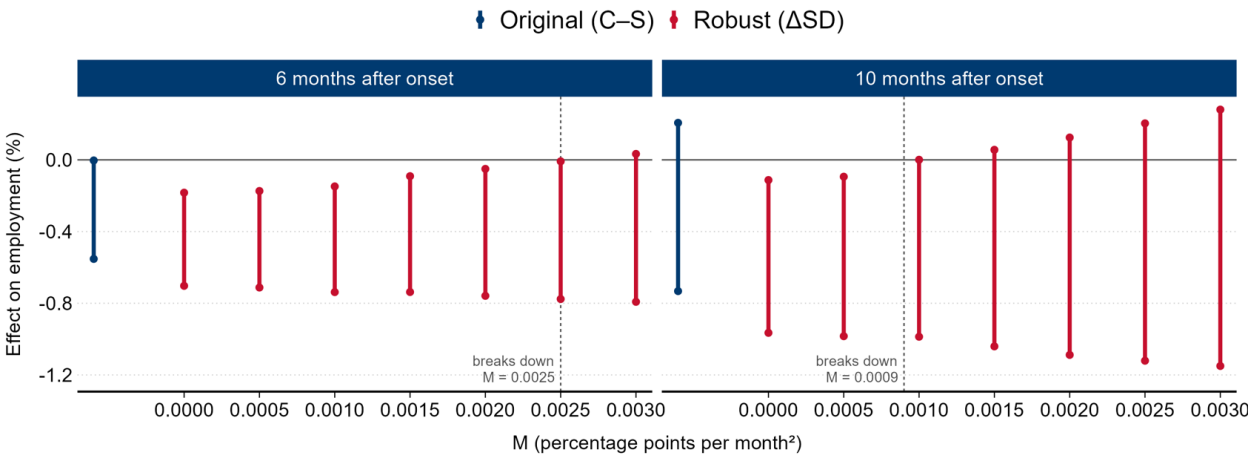


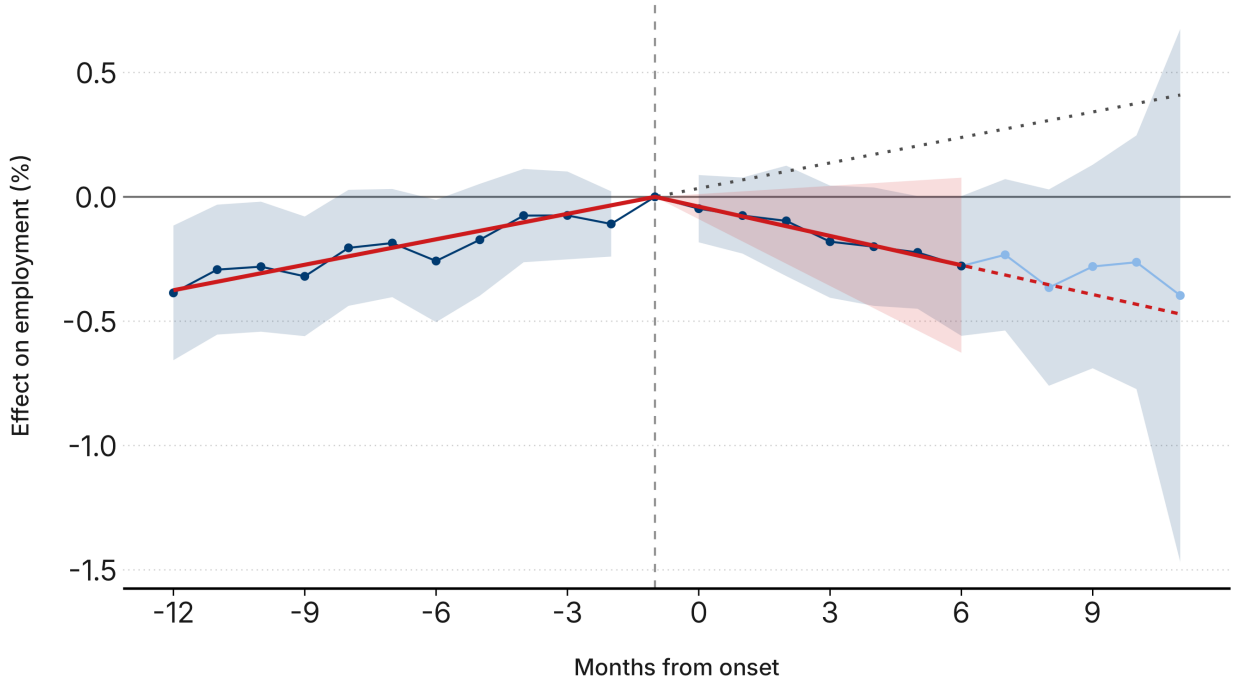
Figure 6 shows results of a test following Rambachan and Roth (2023) that curves the trend to ask how far it would need to bend before the results became insignificant. Estimates at six months remain significant even if the post-surge trend deviates from its linear

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.

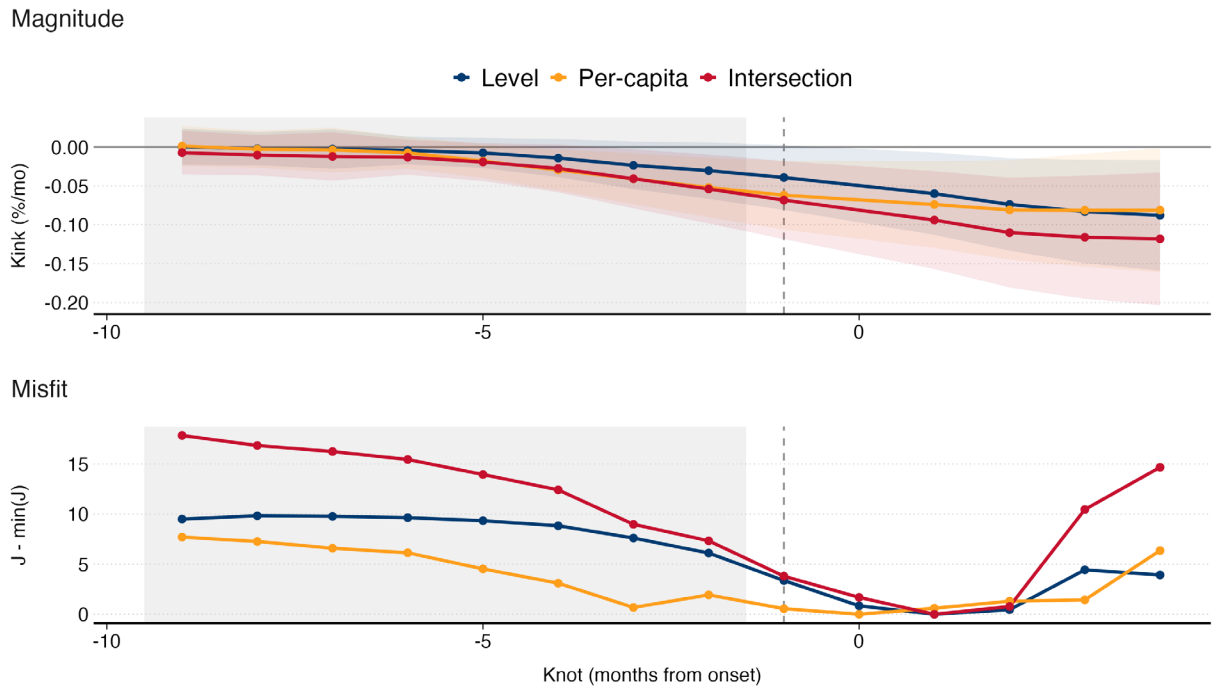
Figure 7: Trend break analysis shows significant downturn when enforcement begins.



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

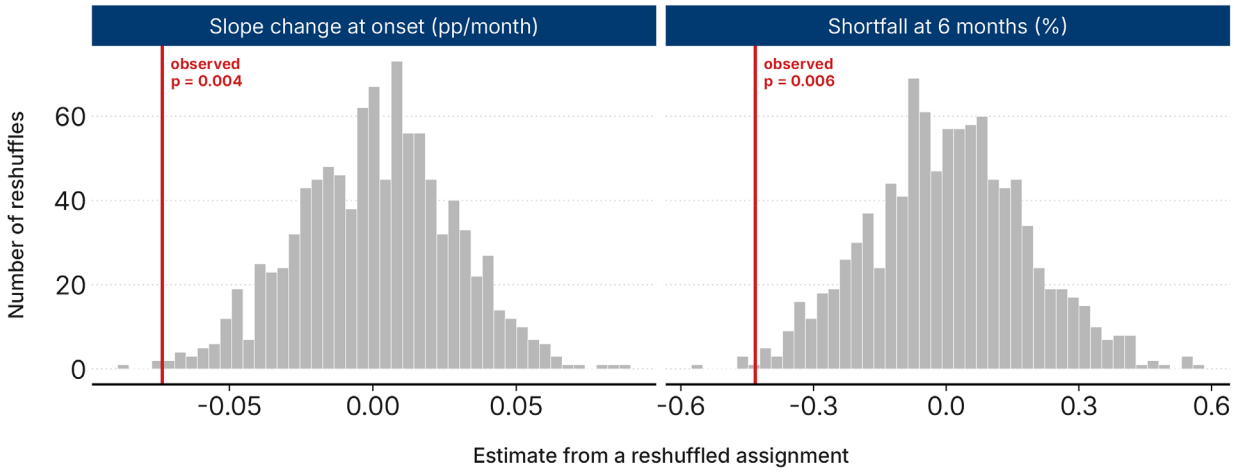


Figure 9 shows the results of a randomization-inference test that asks how often an effect as large as the one we observe would arise purely by chance. That is, could a break this large arise between two arbitrary groups of cities, entirely by chance? We construct 1000 random, "placebo" assignments of 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 $\bar{M}$=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).