

The Local Labor-Market Effects of Interior Immigration Enforcement: Evidence from Recent Policy Swings

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Abstract

Interior immigration enforcement swung sharply between 2021 and 2025, yet its effects on local labor markets, and on whom, remain hard to measure. I assemble a new monthly panel of ICE arrests by metropolitan area from October 2015 through March 2026 and isolate a cleaned measure of interior enforcement, separating it from the administrative processing of the 2021-2022 border surge. I identify effects with a shift-share design that interacts each metro's predetermined , Obama-era enforcement exposure with two national shocks: the Biden de-escalation of January 2021 and the Trump second-inauguration escalation of January 2025. The shocks move enforcement on different margins, and replacing realized arrests with predetermined exposure reverses the sign of their naive correlation with employment: under the escalation, employment falls in immigrant-intensive sectors, while the de-escalation mirrors this with a lag. The estimates point to spillovers onto the documented and native-born workforce, suggesting that enforcement's consequences fall partly on the workers it is often meant to protect.

1 Introduction

“All illegal entry will immediately be halted, and we will begin the process of returning millions and millions of criminal aliens back to the places from which they came.”

— President Donald J. Trump, Inaugural Address, January 20, 2025

Few domestic policies have swung as sharply in as short a span as interior immigration enforcement. In just four years the federal government moved from the Biden administration’s 2021 de-prioritization of interior arrests (which narrowed enforcement largely to individuals deemed threats to public safety or national security) to the second Trump administration’s 2025 escalation, which made the mass removal of undocumented immigrants a central goal and sharply increased interior arrests. The stakes are large: roughly ten million undocumented immigrants work in the United States ([Passel and Krogstad, 2025](#)), concentrated in industries such as construction, agriculture, and food service, so swings of this magnitude can meaningfully alter the supply of undocumented labor in local economies, and with it the prospects of the documented and native-born workers who often work alongside them. How local labor markets respond when enforcement ramps up and down is therefore a first-order economic question, and one on which the evidence remains thin and contested.

The question I ask is how local labor markets respond when the intensity of interior enforcement rises or falls, and in particular whose outcomes move. Enforcement directly removes undocumented workers, but the more consequential effects may fall on the documented and native-born workers who remain, depending on whether they substitute for or complement the workers who are removed. Answering this is harder than it appears, for several reasons. First, enforcement is not assigned at random: ICE concentrates its resources where local conditions (a large undocumented population, a tight low-wage labor market) make realized arrests correlated with the outcomes of interest, so simple comparisons of high- and low-enforcement places confound the effect of enforcement with the conditions that attract it. Second, the population enforcement targets is among the hardest to observe in standard data, which undercount the undocumented and rarely record legal status. Third, the existing evidence is mixed, with estimates ranging from native workers gaining as competitors are removed to native workers losing as complementary labor disappears. Progress in answering these questions therefore requires both a credible source of variation in enforcement and high-frequency data that can trace its effects as they unfold.

My approach pairs a new data set with a design built for this endogeneity problem. On the data side, I assemble a new monthly panel of ICE arrests by metropolitan area, spanning October 2015 through March 2026 and covering roughly 1.5 million arrests, built from admin-

istrative records released under the Freedom of Information Act. A key step is measurement: I show that the raw 2021–2022 counts are heavily inflated by the administrative processing of the border surge (office check-ins of recently arrived migrants that the data record as “arrests”), and I isolate a cleaned series that reflects interior enforcement of settled residents. On the design side, I use a pre-determined-exposure shift-share strategy that interacts each metro’s enforcement exposure, fixed in the Obama-era years before any of the shocks I study, with the timing of two national policy shocks: the Biden administration’s de-escalation in January 2021 and the Trump-2 escalation in January 2025. Because the exposure measure is set years in advance and the timing of the shocks is national, the design breaks the dependence of treatment on contemporaneous local conditions that confounds naive comparisons. I match this variation to monthly employment from the Quarterly Census of Employment and Wages and unemployment from the Local Area Unemployment Statistics, and I first verify that the shocks move enforcement itself before turning to their labor-market effects.

The first stage confirms the shocks move enforcement: more-exposed metros see interior arrests rise after the escalation and fall after the de-escalation, and the two administrations work different margins: the escalation through community and non-criminal arrests, the de-escalation through the custodial, jail-based pipeline. Turning to labor markets, the credible design overturns the naive correlation. Regressing employment on realized arrests yields a positive, marginally significant coefficient for construction (the signature of reverse causality, as ICE concentrates where construction is booming) but replacing realized arrests with pre-determined exposure reverses it. Under the escalation, the clearest effect is in an immigrant-intensive sector: administrative and support services fall about 1.2 percent per standard deviation of exposure. The de-escalation produces the mirror image with a lag, as unemployment in more-exposed metros falls over the following year and then reverts when a court injunction halts the policy. The estimates are power-limited (confidence intervals often include zero), so I rest the interpretation on their internal consistency (the sign reversal, the concentration in immigrant-intensive employment, and the escalation–de-escalation mirror) more than on any single coefficient. Because the workers I observe are overwhelmingly documented and native-born, I read these effects as spillovers onto the measured workforce through labor-market complementarity, not as a mechanical count of removed undocumented labor.

The paper makes three contributions. First, it brings design-based, high-frequency evidence to the most recent enforcement swings. The canonical finding that interior enforcement reduces employment, including among U.S. citizens in complementary roles, comes from the Secure Communities era of the early 2010s (East et al., 2023), and recent work has begun to examine the Trump-2 escalation (Cox and East, 2026); I complement these with a

monthly, metropolitan-level panel spanning October 2015 through March 2026 and a pre-determined-exposure design that addresses the endogeneity of where enforcement occurs. Second, I study both directions of the policy cycle. Most work examines escalations alone; by pairing the Trump-2 escalation with the Biden de-escalation, I show that enforcement and local labor markets move in opposite directions across the two shocks, a coherence check no single episode can provide. Third, I contribute a measurement correction: I document that the 2021–2022 arrest counts are dominated by the administrative processing of the border surge rather than interior enforcement, and I isolate a cleaned interior-enforcement series, a distinction that matters most in the de-escalation window I study. Together, these let me speak to who bears the cost of enforcement: with the directly-removed workers largely unobserved, the measured effects fall on the documented and native-born workforce that remains, evidence more consistent with complementarity than with substitution ([Ottaviano and Peri, 2012](#); [Borjas, 2003](#)).

The remainder of the paper is organized as follows. Section 2 provides background on the recent enforcement regimes and on the role of immigrant labor in local economies. Section 3 describes the arrest and labor-market data, including the cleaned interior-enforcement measure and the pre-determined-exposure measure. Section 4 presents the empirical strategy. Section 5 reports the results (the naive treatment, the first stage, and the labor-market estimates) and Section 6 interprets them in light of the substitution-versus-complementarity debate. Section 7 concludes.

2 Background

2.1 Immigration enforcement and the modern policy regimes

Immigration enforcement in the United States is primarily carried out by Immigration and Customs Enforcement (ICE), a federal agency under the Department of Homeland Security (DHS). ICE is responsible for enforcing immigration laws and managing the detention and removal of individuals who are in the country without authorization. Interior enforcement refers to ICE’s activities within the interior of the country, as opposed to border enforcement which is primarily run by U.S. Customs and Border Protection (CBP). Interior enforcement is run by ICE’s Enforcement and Removal Operations (ERO) division, which operates through a network of field offices across the country. These field offices are responsible for carrying out enforcement actions in their respective geographic areas, which can include arrests, detentions, and removals of undocumented individuals ([East et al., 2023](#); [Cox and East, 2026](#)). The intensity and focus of ICE’s interior enforcement can vary over time, depending

on federal policy priorities and directives from the administration in power.

Arrests made by ICE can occur in various contexts including custodial/jail-based arrests, at-large/community arrests, and workplace raids. Custodial arrests typically involve individuals who are already in the custody of local law enforcement, such as those in jails or prisons; ICE identifies many of them through the Criminal Alien Program (CAP), which screens inmates at federal, state, and local facilities. Programs like Secure Communities and the Priority Enforcement Program further facilitate the identification of undocumented individuals in local custody by sharing arrestees' fingerprints with ICE (East et al., 2023; Alsan and Yang, 2024). The 287(g) program also allows local law enforcement agencies to enter into agreements with ICE to perform immigration enforcement functions, including identifying and arresting undocumented individuals. At-large arrests occur in the community, where ICE officers may apprehend individuals in public spaces or at their homes. Workplace raids involve ICE targeting businesses suspected of employing undocumented workers, leading to arrests of both employees and employers.

Arrests made by ICE can be classified into different categories based on the individual's criminal history and other factors. For example, ICE may prioritize the removal of individuals with criminal records or those who pose a threat to public safety, while deprioritizing the removal of individuals without criminal records. The agency's enforcement priorities are often influenced by national policy directives, such as memos issued by the Secretary of Homeland Security or court rulings that affect the implementation of enforcement policies.

Around the period of my study, several national policy shifts reshaped ICE's interior enforcement. The first is the Biden administration's de-escalation, which began at the January 2021 inauguration: within days, the Department of Homeland Security issued interim enforcement priorities and a 100-day pause on many removals,¹ and over the course of 2021 ICE shifted away from at-large arrests of individuals without criminal records. This deprioritization was formally codified that fall in the Mayorkas memo, "Guidelines for the Enforcement of Civil Immigration Law" (issued September 2021, effective November 2021), which directed ICE to focus on individuals posing threats to national security, public safety, and border security.² The de-escalation was partially interrupted in June 2022, when a federal court in *Texas v. United States* enjoined the Mayorkas guidelines after a challenge by Texas and Louisiana; the Supreme Court reinstated them in June 2023, holding that the states lacked standing. The final shift is the inauguration of the second Trump ad-

¹U.S. Department of Homeland Security (2021), "Review of and Interim Revision to Civil Immigration Enforcement and Removal Policies and Priorities," memorandum from Acting Secretary David Pekoske, January 20, 2021, implemented by ICE interim guidance on February 18, 2021.

²U.S. Department of Homeland Security (2021), "Guidelines for the Enforcement of Civil Immigration Law," memorandum from Secretary Alejandro Mayorkas, September 30, 2021.

ministration (Trump-2) in January 2025, which marked a return to aggressive enforcement, emphasizing mass removal of undocumented individuals and a large increase in resources for interior operations (Cox and East, 2026). My analysis centers on the two shocks with clean pre-periods (the January 2021 de-escalation and the January 2025 escalation) and treats the 2022 injunction and 2023 reinstatement as intervening events. Future work could examine the first Trump administration (Trump-1), but my arrest data begin only in October 2015, leaving no clean pre-period separate from the window I use to construct exposure; I therefore focus on the shifts from 2021 onward.

2.2 Immigrants in the local labor market

As of 2023, there were an estimated 14 million undocumented immigrants in the United States, of whom about 10 million were in the workforce (Passel and Krogstad, 2025). Immigrants, particularly undocumented workers, are concentrated in certain industries, including construction, agriculture, and hospitality/food services (USDA ERS, 2024; Passel and Krogstad, 2025). These industries often rely on low-wage labor and have a higher proportion of undocumented workers compared to other sectors. The concentration of undocumented workers in these industries makes local labor markets particularly sensitive to changes in immigration enforcement policies. Additionally, exposure to enforcement can vary across metropolitan areas based on the size of the undocumented population, the presence of industries that employ undocumented workers, and the local enforcement footprint of ICE field offices. Many undocumented immigrants also live in mixed-status households, where some family members are citizens or legal residents, which can further complicate the labor market dynamics in response to enforcement actions and broadens who is affected by these policies.

Labor market outcomes for undocumented workers are often difficult to measure due to their legal status and the informal nature of many of their jobs. Undocumented workers may face barriers to accessing formal employment, leading them to work in industries or occupations that are less regulated and more susceptible to enforcement actions. This can result in a higher degree of labor market volatility for undocumented workers, as they may be more likely to experience job loss or displacement due to enforcement actions. Additionally, undocumented workers may have limited access to social safety nets and labor protections, which can exacerbate the negative effects of enforcement on their employment prospects. This can lead immigrants, undocumented or documented, to experience chilling effects on their labor market participation: fear that commuting or working could increase their likelihood, or their family's likelihood, of detention or deportation (Alsan and Yang,

2024; Watson, 2014).

Moreover, the presence of undocumented workers in the labor market can have spillover effects on the employment and wages of native-born and documented immigrants, as they may compete for similar jobs or complement each other’s skills in certain industries. This literature has produced mixed findings (Dustmann et al., 2016), with some studies suggesting that the presence of undocumented workers can lower the wages and employment opportunities of competing native-born workers (Borjas, 2003), while others find that immigrant workers can complement the skills of native-born workers and contribute to overall economic growth (Ottaviano and Peri, 2012; Peri and Sparber, 2009; Card, 2009). Understanding the effects of different immigration enforcement regimes on local labor markets is therefore crucial for informing policy decisions and assessing the broader economic implications of these policies.

3 Data

3.1 Immigration enforcement

I measure interior immigration enforcement using individual-level records of arrests made by U.S. Immigration and Customs Enforcement (ICE). The records are government data provided by ICE in response to a FOIA request, and are processed by the Deportation Data Project. I combine two releases. The first is a modern file covering October 2022 through March 2026. The second is a historical extension obtained under FOIA case 2022-ICFO-22955 (the García Hernández release), which I use for fiscal years 2016 through 2022. Together they span October 2015 through March 2026 (126 months) and cover four distinct enforcement regimes: the end of the Obama administration, the first Trump administration, the Biden administration, and the first fourteen months of the second Trump administration. In total the sample contains approximately 1.47 million arrests.

Each record is an administrative arrest with a date, basic demographic information (e.g., sex, birth year, country of citizenship), a criminality classification, and the ICE field office responsible for the arrest. I aggregate these records to the metropolitan-area (MSA) by month-year level, which is the unit of analysis throughout the paper.

One feature of the data shapes how I assign arrests to geographical places. ICE records do not reliably report where an arrest physically occurred; instead, each record identifies the duty station of the arresting officer. I therefore assign each arrest to the metropolitan area that contains the responsible ICE field office (its “duty site”), so my geographic unit reflects the catchment area of an ICE office rather than a precise arrest location. Because

ICE field offices typically operate within their surrounding metropolitan area, the duty site is a reasonable proxy for the location of enforcement. The main concern is that some offices serve large catchment areas spanning more than one metro, which introduces measurement error in the geographic assignment; to the extent this error is classical, it attenuates my estimates toward zero. I restrict the sample to the **167 MSAs** that contain an ICE duty site.

The two releases record this office under different variable names, and I harmonize them to a single geography. In the modern release I use the arresting officer’s current duty station; in the historical release I use the office tied to where the apprehension physically occurred (its *physical site*). I deliberately do not use the historical release’s free-text apprehension city and county fields: they are blank for roughly a third of historical records, and the blank rate differs systematically by arrest type, so using them would distort the composition of arrests across metros.³ Both releases are then mapped to metropolitan areas through the same office-to-MSA crosswalk, so the geographic definition is consistent across the full sample period.

Aggregating to the MSA-by-month-year level and keeping the 167 metros with an ICE office yields a balanced panel of **167 metropolitan areas observed monthly from October 2015 through March 2026**. This covers 126 months, or **21,042 metro-month-year observations**, containing 1,466,714 arrests.

However, not every record in these data reflects interior enforcement of the settled population I study. ICE registers an administrative “arrest” whenever it issues charging documents, including at routine office check-ins, so during the 2021–2022 surge in Southwest border crossings the counts swelled with the processing of recently arrived migrants released onto the non-detained docket rather than apprehensions of established residents (Kim, 2023; East et al., 2026). This processing runs through two administrative programs (non-detained docket control and Alternatives to Detention) and is overwhelmingly composed of recent-arrival nationalities: in 2021, fewer than one percent of these non-criminal arrests are of Mexican nationals, against roughly ninety percent from the countries driving the contemporaneous border surge.⁴ To isolate interior enforcement, I drop arrests made under these docket-management programs throughout; the resulting series is the *cleaned interior-enforcement* measure used in all that follows. The adjustment removes about nineteen percent of arrests

³The physical-site field is populated for 97.5% of historical records; for the remaining rows I fall back to the *responsible site* (the office with administrative responsibility for the case), which is fully populated on that subset. I prefer the physical site because the responsible site reflects case ownership, often a centralized regional office, rather than the location of enforcement. The crosswalk assigns a metropolitan area to 94.4% of historical arrests and 99.6% of modern arrests.

⁴Interior enforcement of the settled population, by contrast, is Mexican-dominated; in the Trump-2 period Mexican nationals are the single largest group among non-criminal arrests.

overall and is concentrated in the 2021–2022 de-escalation window. Appendix B documents the contamination in detail, and Appendix A shows that the escalation results are robust to using the uncleaned counts.

My key independent variable is not the number of arrests an MSA *actually* experiences. Because ICE chooses where to concentrate enforcement in response to local conditions, realized arrests are endogenous to the very labor markets I study (I return to this in Section 4). Instead, I build a pre-determined measure of each MSA’s exposure to enforcement: the average monthly number of ICE arrests in the metro over **October 2015–December 2016**, scaled per 1,000 workers (2016 employment). This window is the full span of arrest data before the first Trump administration, so the measure reflects an Obama-era enforcement footprint that is fixed years before any of the policy shocks I study and cannot respond to them.

3.2 Labor-market outcomes

My primary outcome is local employment, which I measure using the Quarterly Census of Employment and Wages (QCEW). The QCEW is an administrative dataset derived from unemployment insurance (UI) tax records, and it contains a near-census of jobs covered by UI⁵. While wages are available at the quarterly level, the QCEW’s employment data are available monthly, and I use the latter to match the frequency of my arrest data⁶. Monthly QCEW employment is available at the county level; I aggregate to the MSA level⁷ and by industry, so I can measure both total employment (my main outcome) and industry-specific outcomes (e.g., construction, accommodation and food services, and administrative and support services, the last comprising building and grounds work such as landscaping and janitorial services and temporary staffing) that are more likely to employ undocumented workers. The QCEW is present for about 98% of the MSA-month-year observations in my arrest panel. A limitation in the data is that the QCEW covers only formal, UI-covered jobs. It does not capture informal/off-the-books work, most self-employment, and some

⁵The QCEW covers about 95%–98% of civilian wage and salary employment.

⁶QCEW is released quarterly, but each filing reports a separate employment count for every month in the quarter. It reports the number of workers on the payroll for the pay period that includes the 12th of the month, which is a standard reference date shared across federal employment statistics. I use these monthly employment counts; total wages are reported only at the quarterly level and are not used here.

⁷QCEW assigns each MSA its constituent counties under the Office of Management and Budget (OMB) delineation in effect at the time, and it applies new delineations prospectively without revising earlier years. The published MSA-level employment series therefore contains a one-time discontinuity in the reference year a new delineation takes effect. To hold geography constant, I instead construct each MSA’s employment by aggregating its constituent counties’ QCEW employment using a single fixed (2023) county-to-MSA crosswalk applied to every year. Since county boundaries are stable across delineations, this reproduces the MSA employment concept on a constant footprint.

farm work, which are occupations many undocumented workers hold. Therefore, the QCEW mostly observes the formal workforce (citizens and documented immigrants), which is a caveat I return to in the Discussion.

A secondary outcome I use is the unemployment rate, which I measure using the Local Area Unemployment Statistics (LAUS) from the Bureau of Labor Statistics (BLS). It publishes monthly unemployment rates directly at the MSA level, therefore I match each MSA’s series to my panel by its Census CBSA code. Unlike the employment QCEW data, BLS produces the estimates at the MSA level and revises the historical series onto current geography, so it is not subject to the definitional breaks that affect employment levels. The LAUS unemployment rate is a survey-based estimate derived from the Current Population Survey (CPS) and other sources, and it reflects the share of the labor force that is unemployed and actively seeking work. Like the QCEW, the LAUS has limitations in capturing the undocumented workforce, since undocumented workers are less likely to be captured in the CPS and may be undercounted in the labor force. Therefore, the LAUS unemployment rate is also an imperfect measure of labor market conditions for the undocumented population, and I return to this caveat in the Discussion.

Together, the arrest data and labor market outcomes yield a balanced panel of 167 MSAs observed monthly from October 2015 through March 2026, containing 21,042 metro-month-year observations. The arrest data are available for every observation; the QCEW employment data are present for about 98% of them, and the LAUS unemployment rate is available for nearly all (99%). I merge the datasets by MSA and month-year to create the final analysis panel.

4 Empirical Strategy

A naive approach to estimating the effect of ICE enforcement on local labor markets would be to regress outcomes directly on the number of arrests an MSA experiences.

$$y_{it} = \beta \text{ArrestRate}_{it} + \alpha_i + \tau_t + \varepsilon_{it}, \tag{1}$$

Equation (1) is this specification, where ArrestRate_{it} is realized ICE arrests per 1,000 (2016) employment in MSA i and month-year t .⁸ α_i are MSA fixed effects, τ_t are month-year fixed effects, and standard errors are clustered by MSA.

⁸ ArrestRate_{it} is the number of ICE arrests in MSA i during month-year t , divided by MSA i ’s average monthly employment in 2016 and scaled per 1,000 workers. Fixing the denominator at 2016 employment keeps it from responding to the policy shocks, so that variation in ArrestRate_{it} over time comes only from realized arrests.

However, this is not a credible strategy because ICE’s enforcement is not randomly assigned: ICE chooses where to concentrate its resources in response to local conditions, which may be correlated with labor market outcomes. For example, if ICE targets areas with large undocumented populations or high crime rates, these areas may also have different employment trends for reasons unrelated to enforcement. This reverse causality problem means that a regression of outcomes on realized arrests would yield biased estimates of the effect of enforcement.

To overcome the endogeneity problem, I use a shift-share (Bartik-style) design that replaces realized arrests with the interaction of a pre-determined measure of each MSA’s exposure to enforcement and the timing of national policy shocks (Borusyak et al., 2022). The key idea is that while ICE chooses where to concentrate enforcement, the timing of major shifts in national enforcement policy is exogenous to any individual MSA. By interacting a pre-determined measure of exposure with these national shocks, I can isolate variation in enforcement intensity that is plausibly exogenous to local labor market conditions.

The pre-determined exposure measure I use is the average monthly number of ICE arrests in the metro over October 2015–December 2016, scaled per 1,000 workers (2016 employment), as described in Section 3. This measure captures the relative intensity of enforcement across metros before any of the policy shocks I study occur. The national policy shocks I study are the Biden de-escalation (January 2021), which began with the administration’s first-day interim enforcement priorities and was codified by the Mayorkas memo that fall, and the Trump-2 escalation (January 2025); the intervening *Texas v. United States* injunction (2022) and Supreme Court reinstatement (2023) bracket the de-escalation regime.⁹ These shocks represent distinct shifts in enforcement policy that affect all metros simultaneously, providing exogenous variation in enforcement intensity when interacted with the pre-determined exposure.

The event study specification allows me to follow the dynamics of the effect of enforcement around each policy shock, by interacting the pre-determined exposure with event-time dummies that indicate the number of months relative to the shock. This is given by the

⁹One national escalation shock falls outside the set I study: the first Trump administration, which took office in January 2017. Although it represents the same kind of enforcement escalation as Trump-2, my arrest data begin only in October 2015, so the only pre-period available for a Trump-1 event study is the same October 2015–December 2016 window I use to construct the exposure measure. Because there is no clean pre-period separate from the exposure measure itself, I cannot test for parallel trends around Trump-1, and I therefore center the analysis on the policy regimes from 2021 onward, where exposure is fixed years before the shock. Studying Trump-1 would instead require a pre-determined measure of exposure that does not depend on the timing of arrests, such as a demographic measure of the likely-undocumented population from the American Community Survey. I leave this to future work.

following equation:

$$y_{it} = \sum_{k \neq -1} \beta_k (\text{Exposure}_i \times \mathbf{1}\{t - s = k\}) + \alpha_i + \tau_t + \varepsilon_{it}, \quad (2)$$

where s is the calendar month of the shock, $k = t - s$ is event time, and Exposure_i is the standardized pre-determined exposure from Section 3: the average monthly number of ICE arrests in the metro over **October 2015–December 2016**, scaled per 1,000 workers (2016 employment). Therefore each β_k is the effect per +1 standard deviation of exposure. The reference period is $k = -1$ and is omitted.

Collapsing the event study dynamics into a single difference-in-differences (DiD) specification yields the following equation:

$$y_{it} = \beta (\text{Exposure}_i \times \text{Post}_t) + \alpha_i + \tau_t + \varepsilon_{it}, \quad (3)$$

where $\text{Post}_t = \mathbf{1}\{t \geq s\}$ and β is the differential change in the outcome, after the shock relative to before, associated with a one-standard-deviation-higher pre-determined exposure. The outcome of interest, y_{it} , is the labor market outcome (e.g., log employment or unemployment rate) in MSA i and month-year t , α_i are MSA fixed effects, τ_t are month-year fixed effects, and ε_{it} is the error term.

To better understand the interpretation of β , it is helpful to be clear about the construction of the exposure measure. Each MSA has a single, time-invariant exposure value: its 2015–2016 average ICE arrest rate per 1,000 workers, standardized so that zero corresponds to the average MSA and one corresponds to an MSA one standard deviation more exposed than average. A one-standard-deviation-higher-exposed MSA is therefore one that was more heavily targeted by ICE enforcement before any of the policy shocks I am studying. The coefficient β therefore measures how the outcomes of more-exposed and less-exposed MSAs diverge once a policy shock takes effect. For example, consider two MSAs that were on similar paths before the shock but differed by one standard deviation in their pre-determined exposure. The coefficient β is the additional change in the outcome that the more-exposed MSA experiences relative to the less-exposed MSA after the shock and relative to their own pre-shock trajectories. For log-employment outcomes, $\beta \times 100$ can be interpreted as the percentage change in employment associated with a one-standard-deviation-higher pre-determined exposure to enforcement after the shock.

Additionally, MSA fixed effects, α_i , control for any time-invariant differences across MSAs, such as baseline economic conditions or demographic characteristics, while month-year fixed effects τ_t control for any common shocks or trends that affect all MSAs in a given

month-year, such as national economic conditions or seasonal patterns. The key identifying variation comes from the interaction of the pre-determined exposure with the timing of the national policy shock, which allows me to isolate the effect of enforcement intensity on labor market outcomes while controlling for other confounding factors.

The identification relies on the assumption that, in the absence of the policy shocks, the outcomes of more-exposed and less-exposed MSAs would have followed parallel trends. The event-study in equation (2) provides evidence on this assumption by showing the dynamics of the treatment effect before and after the shock. If the pre-trends (the β_k for $k < 0$) are flat and not statistically different from zero, this supports the parallel trends assumption and strengthens the credibility of the DiD estimates in equation (3).

The credibility of the design does not rely on the pre-determined exposure being exogenous in the sense of being randomly assigned. The exposure measure reflects where ICE historically concentrated enforcement and is therefore a revealed enforcement footprint, not a randomly assigned treatment. Instead, my design follows the logic of shift-share designs (Card, 2001; Goldsmith-Pinkham et al., 2020), in which a common national “shift” interacts with pre-existing local “shares.” Following Borusyak et al. (2022), identification rests on the exogeneity of the national shocks; these are federal decisions that change enforcement everywhere at once and are plausibly exogenous to any single MSA’s labor market conditions. Therefore, the credibility of the estimates relies on the timing of the shocks being exogenous and the absence of differential pre-trends.

Lastly, I cluster standard errors at the MSA level to allow the errors to be correlated within MSAs over time. As a planned refinement, I also consider using exposure-robust standard errors following Adão et al. (2019), which account for the fact that the key variation comes from the interaction of the pre-determined exposure with the national shocks.

5 Results

5.1 The naive treatment and why it fails

The two-way fixed effects coefficient estimates of Eq. (1) are reported in Table 1. The outcomes are the log of total employment, the unemployment rate in percentage points, and the log of employment in three industries that are more likely to employ undocumented workers: construction, accommodation and food services, and administrative and support services (Passel and Cohn, 2016; Passel and Krogstad, 2025). The treatment variable is the contemporaneous realized interior-enforcement arrest rate, measured as cleaned arrests per 1,000 of 2016 employment. Results show that the coefficient on construction employment is

positive and approaches significance, which is consistent with an upward reverse-causality bias. This pattern reflects the idea that ICE concentrates enforcement in areas with strong labor demand, particularly in construction. The design I implement in the following sections addresses this endogeneity concern and provides more credible estimates of the causal effect of enforcement on local labor markets.

Table 1: Naive realized-arrest treatment (two-way fixed effects), cleaned interior-enforcement arrest rate.

Outcome	Coef.	SE	t
ln employment (total)	0.0014	0.0033	0.41
Unemployment rate (pp)	-0.0422	0.0395	-1.07
ln employment (construction)	0.0169	0.0088	1.92
ln employment (accom./food)	0.0029	0.0034	0.87
ln employment (admin./support)	-0.0148	0.0108	-1.37

Notes: coefficient β from Eq. (1); treatment is the contemporaneous realized *interior-enforcement* arrest rate (cleaned arrests per 1,000 of 2016 employment; see Section 3 and App. B); MSA and month-year fixed effects; standard errors clustered by MSA. The construction coefficient is the only one approaching significance; its positive sign is consistent with an upward reverse-causality bias (ICE concentrates where labor demand is strong) rather than a causal effect. The design in Section 4 severs this channel and reverses the estimate.

5.2 First stage: do the policy shocks move enforcement?

Before turning to labor markets, I establish a first stage: do the policy shocks actually move enforcement? Figure 1 shows they do, in opposite directions that match their opposite policy content. Per standard deviation of pre-determined exposure, realized interior arrests rise by 0.14 per 1,000 after the Trump-2 escalation and fall by 0.05 after the Biden de-escalation. The escalation pre-trend is flat; the de-escalation pre-period is shorter and overlaps the COVID recovery, a caveat I return to below.

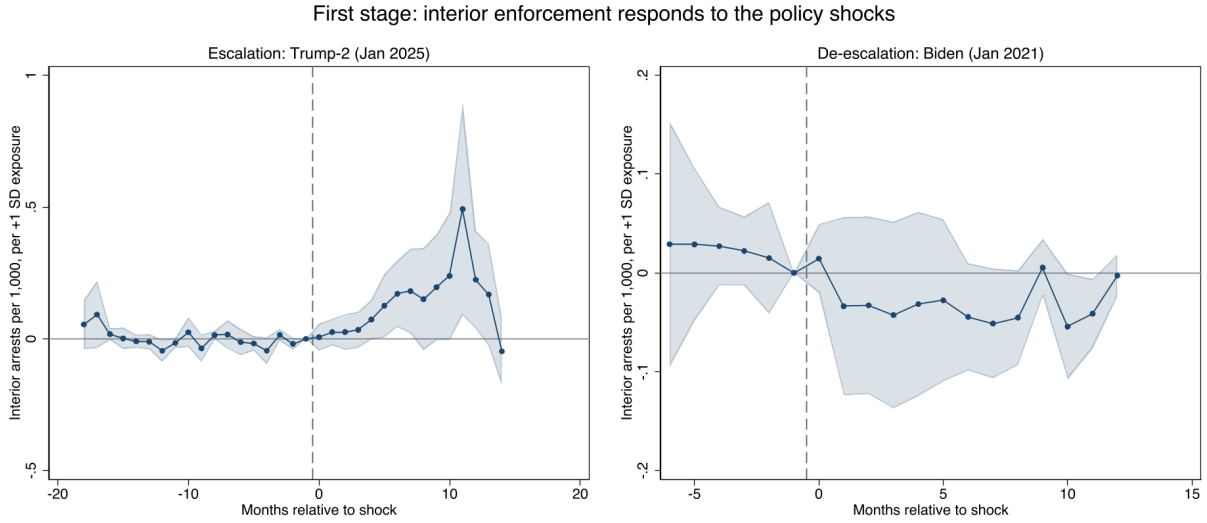


Figure 1: First-stage mirror: realized interior-enforcement arrests per 1,000 (2016 employment) per +1 standard deviation of pre-determined exposure. *Left*: the Trump-2 escalation (January 2025), arrests rise in more-exposed metros. *Right*: the Biden de-escalation (January 2021), arrests fall. Reference month $k = -1$; shaded bands are 95% confidence intervals (note the different vertical scales).

The two shocks also work *different margins* of enforcement (Table 2). The escalation operates through community/at-large and non-criminal arrests, with the custodial margin flat; the de-escalation is almost entirely custodial: of its -0.05 total decline, -0.047 is jail-based (Secure Communities/CAP) enforcement. What rose under Trump-2 is not what fell under Biden.

Table 2: First stage: enforcement response by margin (exposure shift-share DiD).

Arrests per 1,000	Escalation (Trump-2)		De-escalation (Biden)	
	β	t	β	t
Total interior	0.136	2.05	-0.050	-9.74
Community / at-large	0.113	2.38	-0.004	-2.04
Non-criminal	0.114	2.19	0.003	1.23
Custodial (LEA)	0.022	0.61	-0.047	-8.03

Notes: coefficient β per +1 SD of pre-determined exposure from Eq. (3), outcome = realized cleaned interior arrests per 1,000 of 2016 employment; MSA and month-year fixed effects; SEs clustered by MSA. Escalation = Trump-2 (January 2025, window -18 to +14); de-escalation = Biden inauguration / Day-1 interim priorities (January 2021, window -6 to +12). The escalation works the community/at-large and non-criminal margins (custodial flat); the de-escalation works the custodial margin. The de-escalation custodial estimate is strong here but is not separately identified from a secular decline in jail-based enforcement; see App. A.

This distinction matters for what follows. The escalation did not only raise the *level* of enforcement; it shifted its *composition* toward community and non-criminal arrests (Figure 2), the at-large enforcement most likely to reach the working-age, employed population, and hence the channel through which any labor-market response would operate. The escalation first stage is clean; the de-escalation first stage, though strongly estimated, rests on that shorter, COVID-era pre-period and is treated as suggestive (App. A).



Figure 2: Composition of the escalation: the non-criminal share (left) and community/at-large share (right) of cleaned interior arrests, per +1 standard deviation of exposure, around Trump-2. The escalation tilts enforcement toward the labor-relevant margin. Reference month $k = -1$; 95% confidence intervals.

5.3 The shift-share results: labor markets

I now turn to labor-market outcomes, beginning with the Trump-2 escalation. Figure 3 reports the exposure event studies for log employment (top) and the unemployment rate (bottom); each coefficient is the effect of a one-standard-deviation increase in pre-determined exposure relative to the month before the shock. In both panels the pre-trend coefficients are flat and close to zero, consistent with the parallel-trends assumption underlying the design. After the inauguration, employment in more-exposed metros drifts downward and the unemployment rate rises, peaking around seven months out before partially settling. The confidence intervals include zero throughout the post-period, so the estimates trace a coherent contraction (lower employment and higher unemployment) but are imprecise; the binding constraint here is statistical power rather than bias.

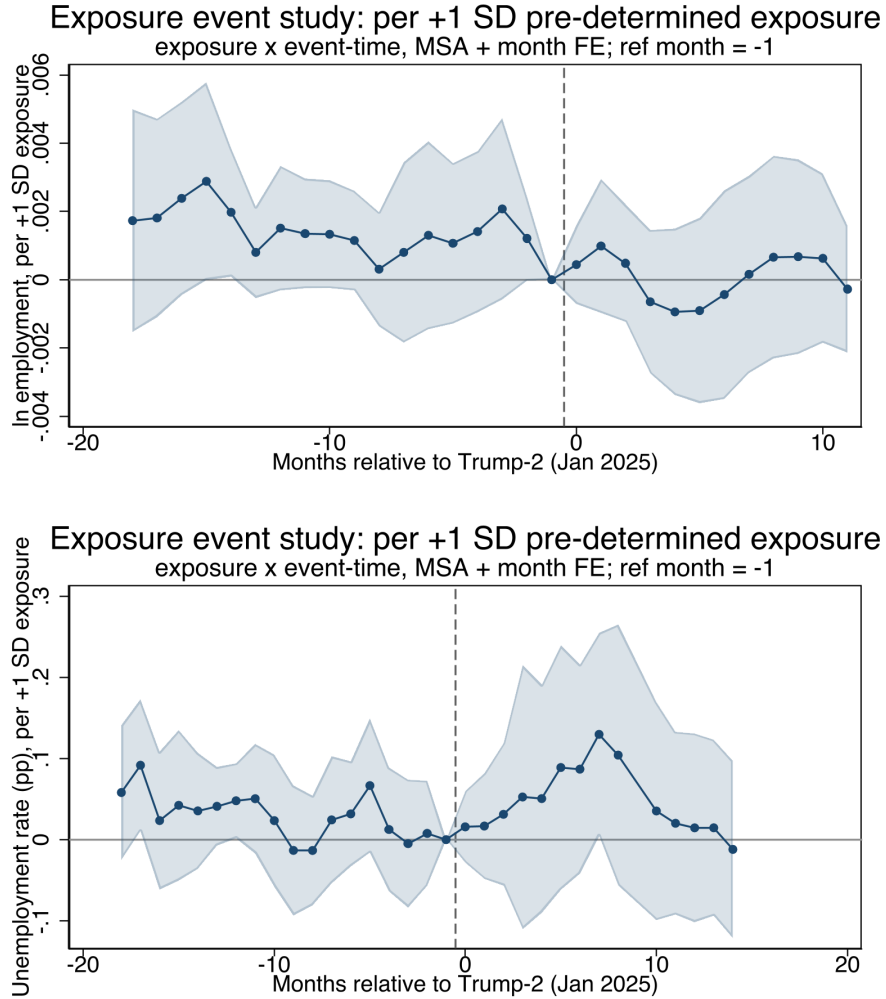


Figure 3: Exposure event studies around Trump-2 (January 2025). Top: log employment; bottom: unemployment rate. Each coefficient is the effect per +1 standard deviation of pre-determined (Obama-era) exposure, relative to the reference month $k = -1$; shaded bands are 95% confidence intervals.

Table 3 summarizes the post-shock response in a single difference-in-differences coefficient per outcome, for both the escalation and the de-escalation. The escalation results sharpen the contrast with the naive specification: the construction coefficient reverses from +0.017 in Table 1 to -0.005 , the sign change that the reverse-causality argument predicts once the endogenous treatment is replaced. The clearest signal is in administrative and support services, one of the most immigrant-intensive sectors (landscaping, janitorial, and temporary staffing), where employment falls by 1.2% per standard deviation of exposure ($t = -2.45$); total employment falls by 0.13% ($t = -1.73$). Accommodation and food services is an exception: a small but statistically significant positive estimate ($t = 2.13$). The de-escalation

column, by contrast, is a precise null across all outcomes, but this reflects the timing of the labor-market response rather than its absence, as the next figure makes clear.

Table 3: Exposure shift-share difference-in-differences: escalation versus de-escalation (labor outcomes).

Outcome	Trump-2 (escalation)		Biden (de-escalation)	
	β	t	β	t
ln employment (total)	-0.0013	-1.73	0.0006	0.33
Unemployment rate (pp)	0.0172	0.36	0.0307	0.57
ln employment (construction)	-0.0048	-0.18	0.0018	0.43
ln employment (accom./food)	0.0029	2.13	0.0009	0.27
ln employment (admin./support)	-0.0121	-2.45	-0.0007	-0.12

Notes: coefficient β per +1 SD of pre-determined exposure from Eq. (3); MSA and month-year fixed effects; standard errors clustered by MSA. Escalation = Trump-2 (January 2025); de-escalation = Biden (January 2021, window -6 to +12). The construction coefficient reverses from +0.017 in the naive Table 1 to -0.005 here: the reversal a positive reverse-causality bias predicts, and the central evidence that the naive estimate is confounded. The de-escalation column is a precise null because the labor response is

lagged (Fig. 4) and averages out over the window.

The de-escalation response appears with a lag, which the single coefficient in Table 3 obscures. Figure 4 plots the unemployment event study around the January-2021 interim priorities, extended through the following two years. Unemployment in more-exposed metros is roughly flat in the months immediately after the shock but then declines steadily, reaching about -0.2 percentage points per standard deviation of exposure by month fifteen, before reverting toward zero at the Texas injunction (the dashed line at +17), which blocked the de-escalation. This is the mirror image of the escalation (unemployment falls under the de-escalation just as it rose under Trump-2), and its timing explains the null in Table 3: a response that is absent early and builds over a year averages to roughly zero in a single post-versus-pre comparison. I read this de-escalation result as suggestive rather than conclusive, since its pre-period overlaps the COVID recovery; I defer a formal treatment of that contamination to robustness.

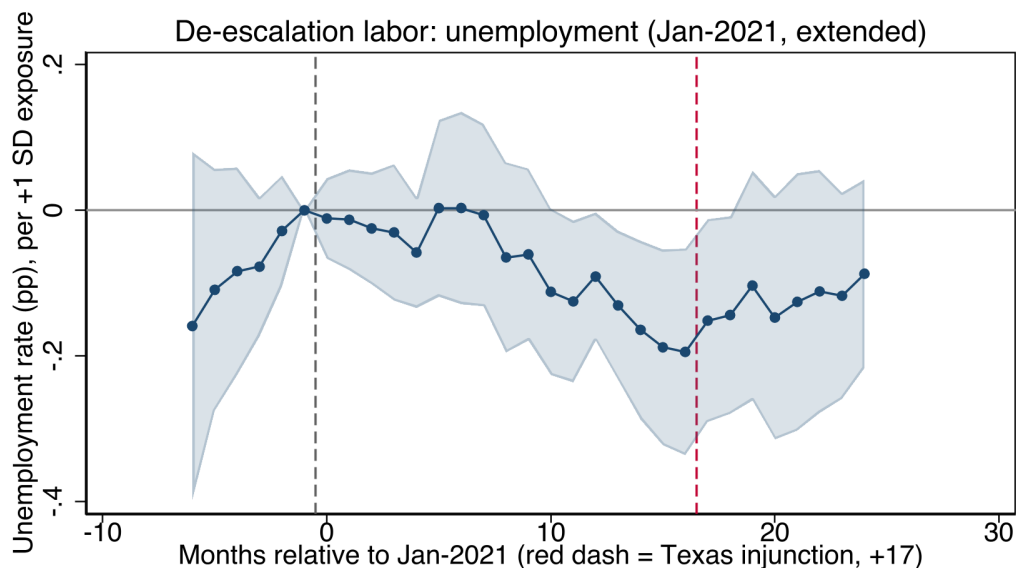


Figure 4: The de-escalation labor response, extended. Unemployment rate per +1 standard deviation of pre-determined exposure around the Biden de-escalation (January 2021). Unemployment in more-exposed metros drifts down through 2022, reaching near -0.2 pp around +15, and then *reverts* at the Texas injunction (red dashed line, +17), which blocked the de-escalation. The single DiD coefficient (Table 3) is null because the response is lagged. Reference month $k = -1$; 95% confidence interval. The pre-period overlaps the COVID recovery, so clean identification is deferred to robustness.

Taken together, the labor-market estimates are power-limited but internally coherent. I do not rest the interpretation on the sign of any single coefficient: theory is ambiguous about whether immigrant and native labor are complements or substitutes, so the direction of the effect is itself an empirical question rather than a maintained assumption. What does provide confidence that the estimates capture a true effect is their consistency along three dimensions: the construction coefficient flips sign once the endogenous treatment is replaced, the largest and most precise effect falls in an immigrant-intensive sector (administrative and support services), and the escalation and de-escalation move outcomes in opposite directions. I defer the interpretation of what these effects imply for the complementarity-versus-substitution debate, and for who bears the cost, to the Discussion.

5.4 Robustness and what is coming

Using the 2016 employment denominator and the -6 to $+12$ month windows around the shocks are choices that I made for consistency and comparability across shocks. I plan to

check that alternative choices of denominator (e.g., using 2015 or 2017 employment) and window lengths do not materially change the results. Additionally, I plan to compare using the QCEW employment data with LAUS unemployment data. Finally, I plan to construct an alternative pre-determined exposure measure based on the American Community Survey (ACS) estimates of the likely-undocumented population in each MSA. This measure would be regime-free and would allow me to study the first Trump administration (Trump-1) as well, which is currently outside the scope of the analysis due to the current exposure measure overlapping with the Trump-1 pre-treatment period.

6 Discussion

A natural concern complicates the interpretation of these estimates: if enforcement removes undocumented workers, but my outcomes are measured on a workforce that barely counts them, what do the estimates capture? Far from undermining the interpretation, working this through sharpens it. The labor force is defined by activity, not legal status, so an undocumented immigrant who works or searches for work belongs to it by definition. But the series I use do not see this population well: the QCEW is built from unemployment-insurance records that the undocumented cannot file, and the LAUS rests on the Current Population Survey, which never asks legal status and systematically undercounts undocumented residents. The population most directly affected by enforcement is therefore largely invisible on the outcome side.

It follows that the estimates are not a mechanical accounting of removed labor supply. Even setting measurement aside, removing the directly-affected individuals would move the measured rates only slightly, and in an ambiguous direction: removing an unemployed person lowers the unemployment rate, while removing an employed person raises it. Because undocumented immigrants have high employment rates (they migrate to work and lack access to the safety net), the marginal removed worker is more likely employed, implying at most a small upward push on the measured rate, and one that is largely unobserved in any case. The accounting channel is thus a weak and sign-ambiguous mover of what I estimate.

The economically meaningful variation is therefore not in the removed individuals but in what their removal does to the workers who remain, natives and documented immigrants, who are well measured in my data. Whether enforcement helps or harms them is the classic substitution-versus-complementarity question. Under the substitution view, immigrants and natives compete for the same jobs, so removing immigrants should help competing natives and lower their unemployment (Borjas, 2003). Under the complementarity view, the two groups are imperfect substitutes (immigrants specializing in manual tasks and natives in

communication- and language-intensive ones), so immigrant labor raises the productivity of, and demand for, complementary native labor (Ottaviano and Peri, 2012; Peri and Sparber, 2009); removing it then harms those workers and raises their unemployment. My estimates (lower employment and higher unemployment in more-exposed metros under the escalation, and the reverse under the de-escalation) are more consistent with complementarity than with substitution.

This reading aligns with the evidence on actual removals. Clemens et al. (2018) find that excluding nearly half a million Mexican *bracero* farmworkers did not raise U.S. farm wages or employment, as growers mechanized and switched crops rather than hiring natives. Closer to the present setting, East et al. (2023) find that Secure Communities *reduced* the employment of U.S. citizens in complementary roles. The broader literature reaches mixed conclusions, in part for methodological reasons (Dustmann et al., 2016), but the removal-based evidence most comparable to mine points toward complementarity.

On this reading, the results describe a spillover onto the measured workforce, including U.S. citizens, operating through labor-market complementarity and local-demand contraction, rather than the mechanical removal of undocumented labor. The escalation and de-escalation trace the same mechanism with opposite signs. The chief limitation of this interpretation is that the at-risk population enters my design only on the left-hand side, where it is poorly measured; a natural next step is to bring it onto the right-hand side through an American Community Survey–based measure of likely-undocumented exposure, which would both sharpen the design and allow for an extension to the first Trump administration.

7 Conclusion

This paper asks how local labor markets respond when interior immigration enforcement ramps up and down. To answer it I assemble a new monthly panel of ICE arrests by metropolitan area from October 2015 through March 2026 and study the sharp swings in enforcement between 2021 and 2025 (a de-escalation under the Biden administration and an escalation under the second Trump administration) with a shift-share design that interacts each metro’s pre-determined, Obama-era enforcement exposure with the timing of the national policy shocks. A first contribution is measurement: I show that the 2021–22 arrest counts are heavily contaminated by administrative processing of the border surge, and I isolate a cleaned interior-enforcement measure that separates enforcement of settled residents from the management of recent arrivals.

Three findings emerge. First, the policy shocks genuinely move enforcement: more-exposed metros see interior arrests rise after the Trump-2 escalation and fall after the

Biden de-escalation, and the two administrations operate on different margins: the escalation through community and non-criminal arrests, the de-escalation through the custodial, jail-based pipeline. Second, the credible design overturns the naive correlation: a contemporaneous regression of employment on realized arrests is confounded by reverse causality, and replacing it with pre-determined exposure reverses the sign. Third, the labor-market response, though power-limited, is internally coherent: employment contracts in the most immigrant-intensive sectors under the escalation, the de-escalation mirrors it with a lag, and both are best read as spillovers onto the measured, native and documented workforce through labor-market complementarity rather than the mechanical removal of undocumented workers.

These patterns matter because interior enforcement is large and swung sharply across the regimes I study, and because the workers who appear in the data, including U.S. citizens, are not the workers enforcement targets. To the extent that immigrant and native labor are complements, the costs of enforcement fall partly on the very workforce it is often argued to protect. That possibility is directly relevant to the current debate over large-scale interior enforcement.

Several limitations temper these conclusions and point to next steps. The estimates are power-limited, and the de-escalation results in particular rest on a short, COVID-era pre-period that I treat as suggestive pending formal robustness. More fundamentally, the population most exposed to enforcement is poorly measured in my outcomes; the natural remedy is to bring it onto the right-hand side of the design through an American Community Survey-based measure of likely-undocumented exposure, which would sharpen identification and extend the analysis to the first Trump administration. As additional months of employment and unemployment data accrue, the post-Trump-2 estimates will also tighten. I leave these extensions, along with effects on wages, to future work.

Appendix A Robustness: the uncleaned measure and alternative de-escalation dating

This appendix reports two checks. First, the headline arrest results are not an artifact of the interior-enforcement cleaning: estimated on the raw, uncleaned arrest measure, the naive treatment (Table 4) and the Trump-2 first stage (Fig. 5) are near-identical to their cleaned counterparts in the main text. Second, I report the November-2021 Mayorkas memo as an alternative de-escalation date (Figs. 6–8); dating the de-escalation at the formal memo rather than the January-2021 interim priorities does not change the qualitative picture.

Table 4: Naive realized-arrest treatment on the *raw* (uncleaned) arrest rate.

Outcome	Coef.	SE	<i>t</i>
ln employment (total)	0.0012	0.0025	0.48
Unemployment rate (pp)	-0.0409	0.0304	-1.34
ln employment (construction)	0.0165	0.0071	2.32
ln employment (accom./food)	0.0037	0.0028	1.33
ln employment (admin./support)	-0.0127	0.0092	-1.38

Notes: this table reproduces Table 1 using the raw total arrest rate (all ICE administrative arrests per 1,000 of 2016 employment, without the interior-enforcement cleaning of App. B) as the treatment. The estimates are near-identical to the cleaned version, so the naive result is not an artifact of the cleaning.

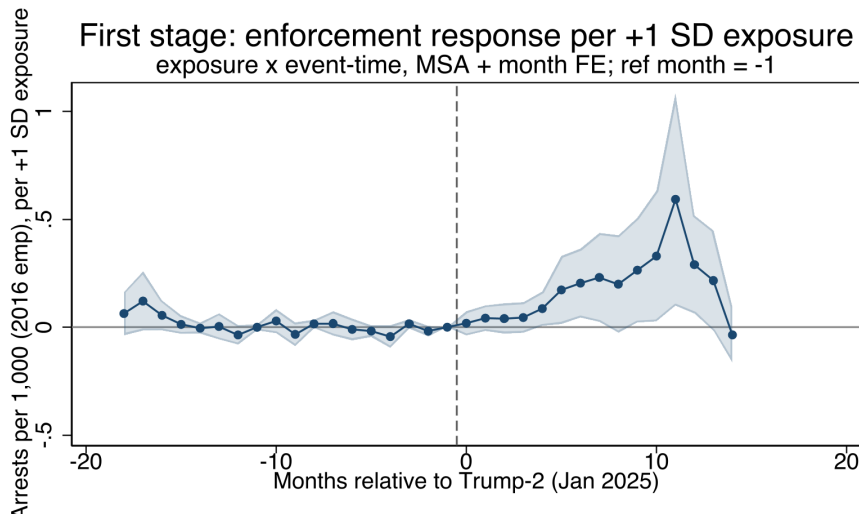


Figure 5: Trump-2 first stage on the *raw* (uncleaned) arrest measure: realized total arrests per 1,000 per +1 SD exposure. Near-identical to the cleaned version (Fig. 1, left): the cleaning does not drive the escalation first stage (it is power-neutral).

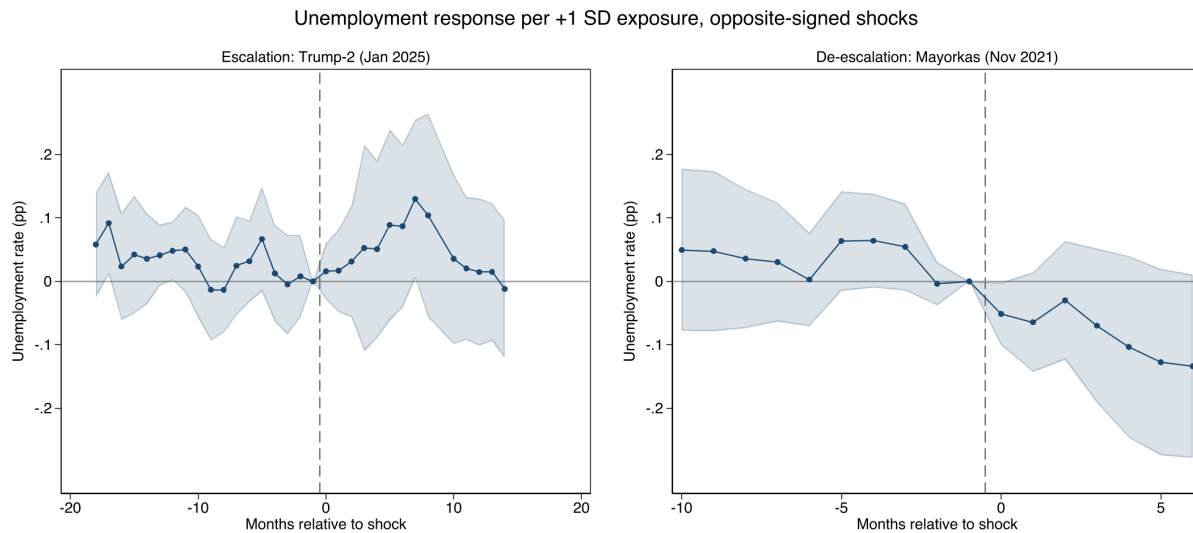


Figure 6: Alternative de-escalation dating (unemployment). The unemployment mirror when the de-escalation is dated at the *formal* Mayorkas memo (November 2021) rather than the January-2021 interim priorities used in the main text. *Left*: Trump-2 escalation; *right*: Mayorkas de-escalation. At this date the single DiD is significant ($-0.12\text{pp}/\text{SD}$, $t = -2.15$); see the discussion of the lagged response in the main text. Per +1 SD exposure, reference $k = -1$, 95% CIs.

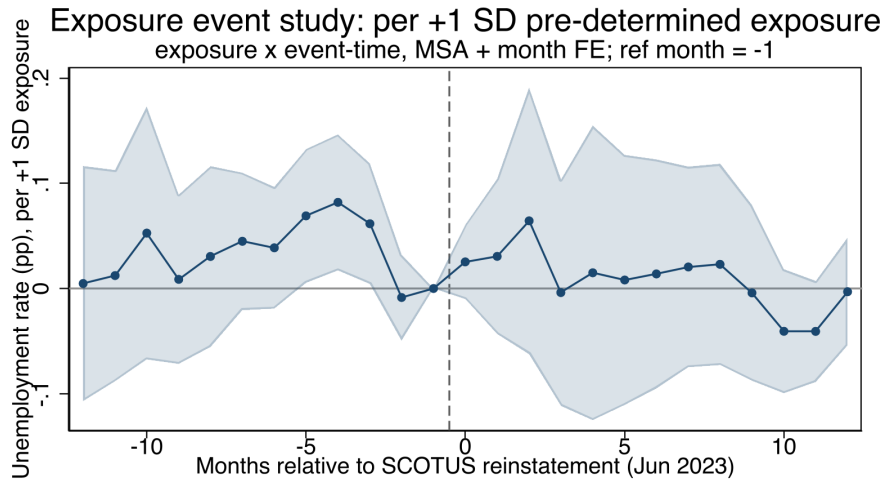
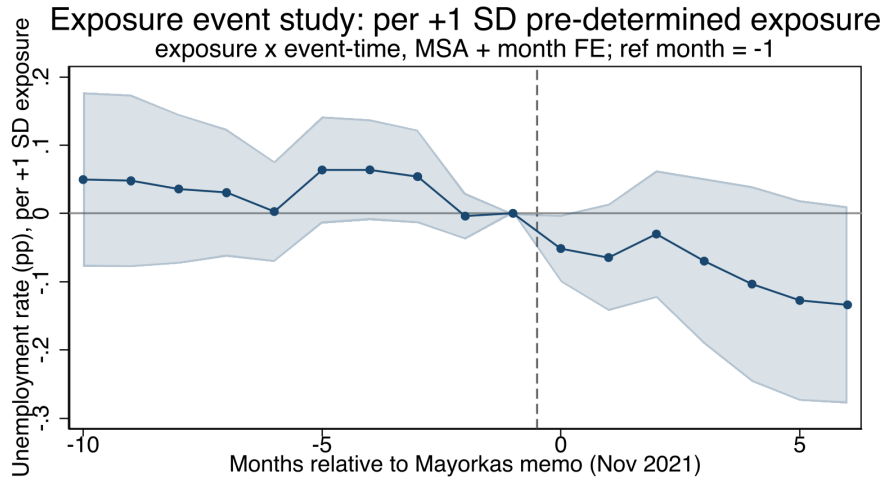


Figure 7: Other regimes (unemployment), exposure design. Top: the Mayorkas memo (November 2021). Bottom: the SCOTUS reinstatement (June 2023). Per +1 SD exposure, reference $k = -1$, 95% CIs.

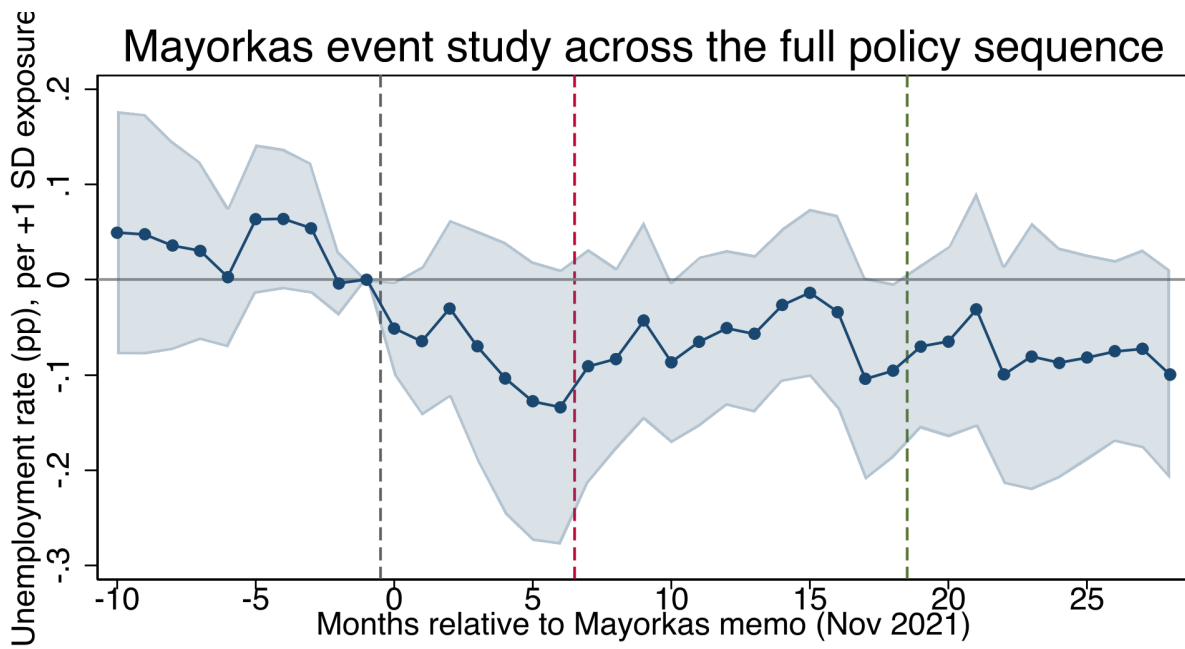


Figure 8: The Mayorkas event study extended across the full policy sequence (unemployment, per +1 SD exposure): the November-2021 de-escalation, the Texas injunction (red dashed), and the SCOTUS reinstatement (green dashed). Coefficients beyond the injunction are confounded by subsequent policy turns and should be read descriptively.

Appendix B Border-processing contamination and the interior-enforcement measure

This appendix documents the border-processing contamination that motivates the cleaned interior-enforcement measure. ICE records a non-detained “arrest” whenever it issues charging documents, including at routine office check-ins, so the 2021–22 arrest counts are swollen by administrative processing of the Western-Hemisphere border surge (the non-detained docket and Alternatives to Detention programs) rather than interior enforcement of settled residents. Three pieces of evidence support this. First, the national swing toward non-criminal and community arrests coincides with the surge (Fig. 9). Second, the docket and Alternatives-to-Detention share of arrests is large over 2021–22 (Fig. 10). Third, the raw de-escalation “first stage” (Fig. 11) reflects this border processing rather than interior enforcement: only 0.5% of the 2021 non-criminal surge are Mexican nationals, against roughly 90% from recent-arrival nationalities. Following Kim (2023) and East et al. (2026), I drop the docket-control and Alternatives-to-Detention programs to isolate interior enforcement.

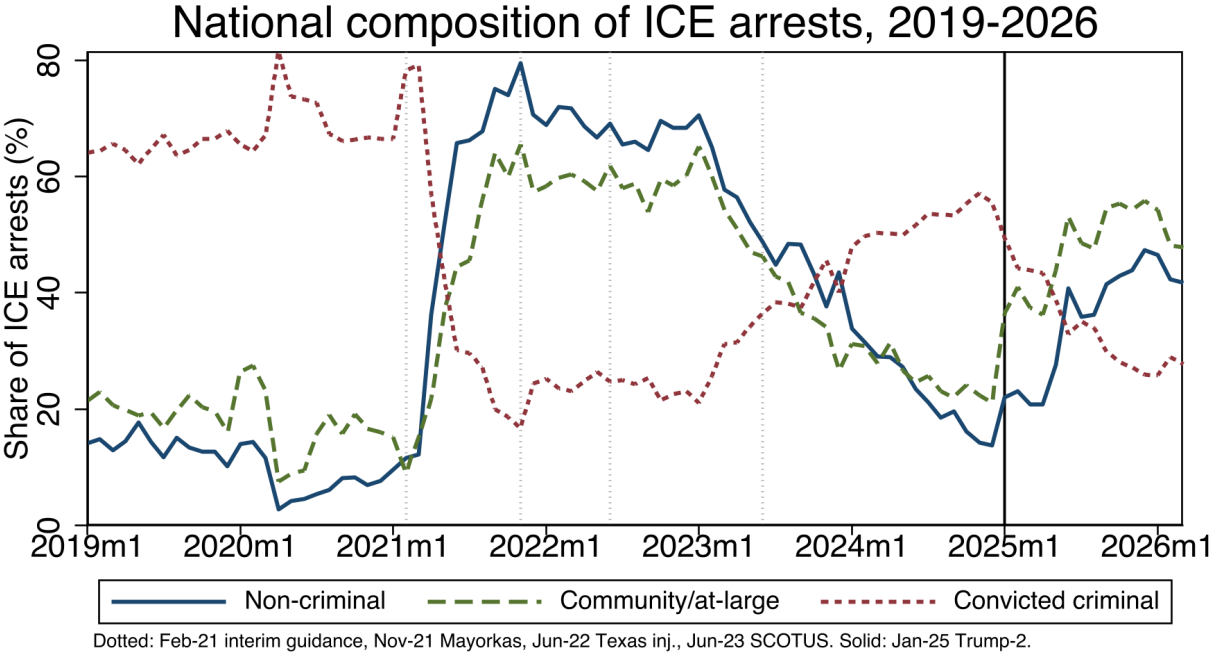


Figure 9: National composition of ICE arrests, 2019–2026 (criminality and community shares). The 2021 swing toward non-criminal/community arrests coincides with the border surge, not interior de-escalation.

Border-surge docket processing contaminates the arrest coun

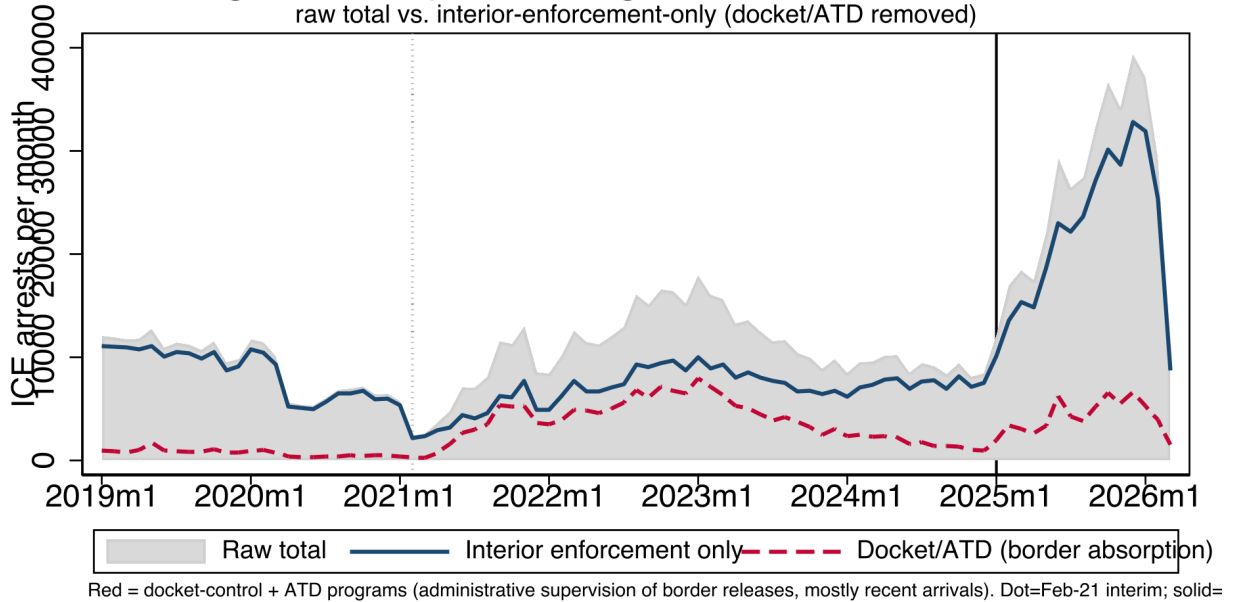


Figure 10: Raw total arrests versus the cleaned interior-enforcement series. The gap (shaded) is docket-control + Alternatives-to-Detention processing, administrative supervision of (mostly border-release) non-detained cases.

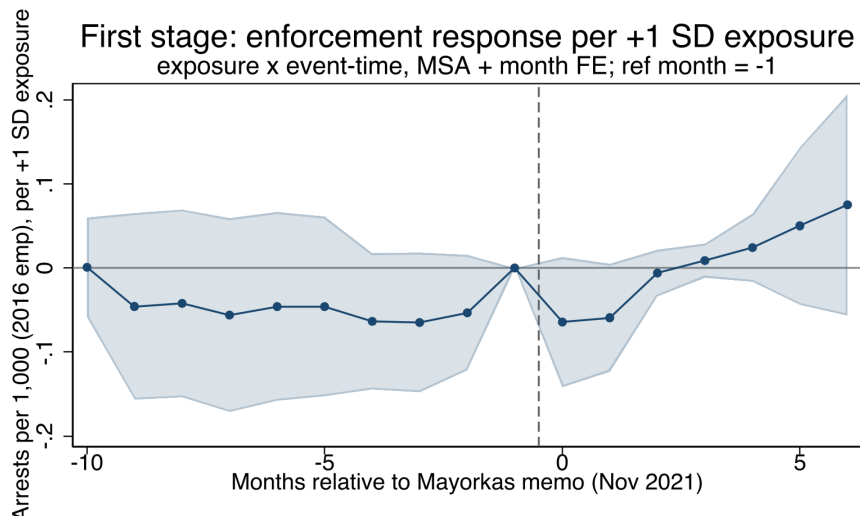


Figure 11: The *raw* de-escalation “first stage” (total arrests per 1,000 per +1 SD exposure). On the uncleaned measure the de-escalation appears to *raise* arrests, the opposite of interior de-escalation, because the 2021–22 counts are dominated by border-processing book-ins. This is the contamination the cleaned interior measure removes.

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