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Immigration Enforcement Visibility and Consumer Spending

Uma De Balanzó

Bocconi University

Núria Rodríguez-Planas

Queens College, CUNY,
IEB and IZA@LISER

Jennifer Roff

Queens College, CUNY
and IZA@LISER

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Immigration Enforcement Visibility and Consumer Spending*

Abstract

We exploit the sharp escalation in community-based ICE enforcement following the January 2025 inauguration to estimate the causal effect of immigration enforcement on consumer spending. Using Synthetic Difference-in-Differences with cross-state variation in surge intensity as the identifying variation, we find that states experiencing the largest enforcement surges saw aggregate card spending decline by 1.7 percentage points relative to their SDiD counterfactual, an effect robust to covariate adjustment, alternative shock windows, and pre-tariff truncation. Null estimates for non-in-person spending rule out a broad regional demand shock, while null estimates for jail-based arrests (enforcement invisible to surrounding communities) isolate enforcement visibility as the operative mechanism. Sector-level estimates reveal two empirically distinct channels: in states with Democratic governors, aggregate spending fell by -4.1 pp ($p < 0.01$), driven by large declines in Accommodation and Food Services (-2.3 pp) and Arts, Entertainment, and Recreation (-7.3 pp), consistent with behavioral withdrawal from public commercial life in jurisdictions where community enforcement was most visible. In Trump-voting states, Home Improvement Centers and Transportation and Warehousing spending fell by -3.8 pp ($p < 0.1$) and -3.0 pp ($p < 0.01$) respectively, consistent with labor supply disruption among undocumented workers in construction and logistics. Our results indicate that the economic costs of enforcement extend well beyond the directly targeted population and depend critically on whether enforcement is visible to the surrounding community — not merely on its scale.

JEL classification

J15, R11, E21

Keywords

immigration enforcement, consumer spending, synthetic difference-in-differences, ICE arrests, local labor markets

Corresponding author

Núria Rodríguez-Planas

nrodriguezplanas@gmail.com

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Introduction

Household spending accounts for approximately two-thirds of U.S. GDP and sustains local businesses, service jobs, and communities that depend on consumer demand. A large literature documents how aggregate shocks, including recessions and pandemics, propagate through household spending to affect employment and business revenues well beyond the initial shock. [Chetty et al. \(2024\)](#) show that during COVID-19, the decreases in spending by affected households sharply reduced the revenues of small businesses, which in turn cut the employment of low-wage workers; [Moretti \(2010\)](#) establishes that local demand contractions transmit to non-tradable service employment through a local multiplier, so that a contraction in purchasing power among one group of workers ripples outward to affect jobs for others. However, the implications of these mechanisms have not been applied to a visible and growing class of policy shocks: interior immigration enforcement. Whether enforcement contracts household spending (and, through it, local economic activity) in ways that extend beyond directly affected immigrants remains an open empirical question. This paper provides the first national causal estimates of the answer.

Interior immigration enforcement can suppress consumer spending through two channels whose sectoral and geographic footprints differ: a *labor supply channel*, in which arrests remove wage earners from households and reduce local income, and a *behavioral channel*, in which fear of enforcement causes non-targeted households to withdraw from public commercial life independently of any income loss. Our sector-level estimates across governance contexts provide evidence on which channel is operative in different enforcement environments.

[East et al. \(2023\)](#) show that the Secure Communities program reduced employment and wages of both undocumented immigrants and U.S.-born workers, and find that the decline in U.S.-born workers' employment is concentrated in non-tradable industries, precisely the sectors where local demand contractions propagate most directly to employment.¹ [Amuedo-](#)

¹Tradable industries include agriculture and manufacturing, and nontradable sectors include construction, retail, wholesale, and nontradable services, such as dry cleaners.

[Dorantes and Antman \(2022\)](#) confirm that actual deportations reduce labor force participation and employment among likely undocumented immigrants, with effects concentrated among women and in industries with high shares of undocumented labor. Most directly relevant to our labor supply channel, [Cox and East \(2026\)](#) find that areas experiencing sudden large increases in ICE arrests in 2025 saw significant reductions in work among likely undocumented immigrants who remain in the U.S., with no offsetting gains for U.S.-born workers. Area-specific studies document labor market disruption in Minnesota ([Sojourner and Rosenthal, 2026](#)) and Texas ([Brizuela et al., 2025](#)), but are geographically limited. This literature documents the supply-side contraction; we document the demand-side transmission. Neither the labor supply nor the behavioral literature has estimated consumption effects directly; this paper fills that gap. Our paper complements this emerging literature by shifting the focus from labor supply to consumer demand and by documenting the sectoral distribution of spending responses that reveals the transmission mechanisms through which enforcement reshapes local economic activity.

The labor supply channel, however, is only part of the story. Even immigrants who are not arrested may withdraw from economic life out of fear of enforcement contact, and that fear propagates through social networks to households with no direct exposure at all. As noted above, [Cox and East \(2026\)](#) estimate that for every ICE arrest in 2025, approximately six male likely undocumented workers stopped working due to fear of enforcement contact — a chilling multiplier roughly 2.6 times larger than estimates from the Obama-era Secure Communities program ([East et al., 2023](#)). Reduced labor supply among immigrants who remain translates directly into reduced household income and, through it, consumer spending. Yet income loss alone cannot explain behavioral withdrawal among households with no direct enforcement exposure.

On the behavioral channel, [Watson \(2014\)](#) shows that heightened enforcement reduces Medicaid participation among citizen children of noncitizens, individuals without risk of deportation, establishing that behavioral responses extend well beyond the targeted population.

[Alsan and Yang \(2024\)](#) document that Secure Communities reduced safety-net participation among Hispanic citizen households and present evidence that fear of deportation propagating through social networks, rather than direct exposure, is the operative mechanism. Most recently, [Aslim et al. \(forthcoming, PNAS\)](#) find that the 2025 escalation in community-based ICE arrests reduced foreign-born women’s employment in center-based child care while driving reallocation toward less visible private household arrangements, evidence that the surge generated economically consequential behavioral responses in a specific labor market. [Dee \(2025\)](#) and [Camp et al. \(2026\)](#) document that the increase in immigration enforcement following January 2025 caused persistent increases in student absences, evidence of fear-induced behavioral withdrawal extending to families with school-age children. Across this literature, consumer spending remains a conjectured mechanism rather than a measured outcome. This paper moves consumption from conjecture to measurement.

We study the sharp escalation in community-based ICE enforcement following the January 20, 2025 inauguration. ICE arrests had been relatively stable through late 2024, averaging 8,000 to 9,000 per month nationally. Total arrests roughly doubled within weeks (from 7,955 in December 2024 to 16,947 in February 2025) and reached 32,667 by October 2025. More consequential than the level was the composition: community-based arrests increased 5.7-fold while jail-based arrests increased only 1.3-fold, so the community-based share of total arrests rose from 29 percent in the pre-period to 63 percent post-inauguration. Simultaneously, the share of those arrested with prior criminal convictions fell from 52 to 37 percent ([East et al., 2026](#)), consistent with enforcement reaching ordinary workers and consumers. The rescission of Biden-era protected-areas guidance opened schools, houses of worship, hospitals, and child-care centers to enforcement operations ([Pearson, 2025](#)), and heightened media coverage of large-scale operations across the country amplified enforcement salience well beyond what arrest counts alone capture ([Herbst and Tekin, 2025](#)).

Cross-state variation in *community-based* enforcement as compared to jail-based arrests is central to our identification. As shown in Figures A1 and A2, observable community-based

operations, including neighborhood sweeps, worksite raids, and targeted arrests in public spaces, increased rapidly following January 2025 across a range of states in different regions and with different political affiliations. We address identification in Section 2.3, but two falsification tests are worth underscoring here. First, if the spending response is driven by the visibility and salience of community enforcement rather than by enforcement intensity *per se*, then a parallel treatment variable constructed from jail-based arrests should produce estimates close to zero, which is precisely what we find. Second, if our results reflected a generic state-level demand shock correlated with enforcement intensity, we would expect non-in-person spending to decline alongside in-person categories; it does not. The null estimate for non-in-person spending (+0.70 pp, $p=0.28$) rules out broad regional demand shocks as the operative mechanism and isolates enforcement visibility as the channel.

Our treatment variable is a time-invariant, state-level surge statistic: the standardized change in community-based ICE arrest rates between February and October 2025 and the symmetric pre-period (February to October 2024). We pair this with monthly card transaction data from Affinity Solutions across 14 spending categories (Chetty et al., 2024) and estimate average treatment effects using the Synthetic Difference-in-Differences (SDiD) estimator of Arkhangelsky et al. (2021), which reweights control states to match treated states' pre-treatment spending trajectories and remains consistent under deviations from parallel trends that would invalidate standard difference-in-differences. The surge onset was near-simultaneous: 19 of 20 treated states (those above the 60th percentile of the surge distribution) first exceeded twice their pre-period arrest rate in January or February 2025, supporting the use of a common treatment date with cross-state variation in intensity as the identifying variation.² By contrast, the closest antecedents in the chilling-effect literature, East et al. (2023) and Alsan and Yang (2024), derived their identification from the staggered

²In our setting, 19 of 20 treated states first exceeded twice their pre-period arrest rate in January or February 2025, making timing heterogeneity empirically minimal. This supports a common treatment date with cross-state variation in intensity as the identifying variation, and renders staggered difference-in-differences estimators (Callaway et al., 2024) unnecessary here, though our results are robust to dropping one state at a time, including Arizona, the sole exception, which first crossed the threshold in June 2025.

county-by-county rollout of Secure Communities, a program with genuinely heterogeneous activation timing across jurisdictions. Our setting differs: the 2025 surge reached every state simultaneously, leaving intensity rather than timing as the source of cross-state variation. We adapt their treatment logic accordingly, classifying states by surge intensity and applying SDiD to construct the counterfactual.

We find that states in the top 40 percent of the enforcement surge distribution experienced aggregate card spending declines of 1.7 to 1.9 percentage points relative to the SDiD counterfactual, significant at the 5 percent level and stable across threshold specifications, covariate adjustment, state exclusions, and alternative shock windows. We contextualize the magnitude with respect to COVID-19 as follows. Since the COVID-19 pandemic reduced aggregate card spending by approximately 23.6 percentage points at the trough using the same Affinity Solutions data (Chetty et al., 2024)³; our aggregate enforcement effect represents approximately 7 percent of that decline, a meaningful magnitude for a targeted policy shock affecting a specific subpopulation rather than a universal health emergency.

The patterns in the sector-level estimates contribute to the interpretation of the mechanism. First, as discussed above, Non-In-Person Spending is null, ruling out a generic regional demand shock. Second, the seven sectors with significant community-enforcement effects, namely, Accommodation and Food Services (−1.8 pp), In-Person Services (−2.4 pp), Durable Goods (−2.7 pp), General Merchandise & Apparel (−3.54 pp), Sporting Goods and Hobbies (−3.6 pp), Transportation and Warehousing (−3.3 pp), and Home Improvement Centers (−5.9 pp), are precisely those that either employ significant shares of undocumented workers or require physical presence at public commercial establishments, consistent with behavioral withdrawal from visible public spaces and commercial life.

The jail-based falsification confirms the enforcement-visibility mechanism. Replacing the community-based surge with a parallel jail-based surge, the aggregate ATT collapses to a statistically null −0.85 pp ($p = 0.302$), and all seven significant sectors under community

³According to estimates from <https://economictracker.org/>, all consumer spending fell by 23.6 percentage points as of April 30, 2020, compared to January 1, 2020.

enforcement produce near-zero, statistically null estimates under jail enforcement. While jail-based enforcement showed lower levels than community-based enforcement and aggregate levels of jail-based enforcement fell overall, this alternative measure retains significant variation, with several states showing large increases in jail-based enforcement (see Appendix Figure A.2). The null effects under jail-based arrest, a channel not observed by the surrounding community, contrast sharply with the significant declines under community-based arrest, underscoring enforcement visibility as the operative mechanism.

Heterogeneity by governance provides evidence on the two channels. In states with Democratic governors, aggregate spending declined by -4.1 pp ($p < 0.01$), with large effects in Accommodation and Food Services (-2.3 pp, $p < 0.1$), General Merchandise Stores (-6.7 pp, $p < 0.10$), and Arts, Entertainment, and Recreation (-7.3 pp, $p < 0.01$), consistent with fear-induced withdrawal from public commercial life where community enforcement was visible and often unwelcome. Unsurprisingly, states whose electoral votes went to Harris show a similar pattern: large decreases in Accommodation and Food Service (-3.2 pp, $p < 0.05$), General Merchandise Stores (-7.9 pp, $p < 0.10$), and Arts, Entertainment, and Recreation (-7.2 pp, $p < 0.05$) In contrast, states with Republican governors or whose electoral votes were cast for Trump show statistically null aggregate ATT of -0.6 pp and -0.55 pp. However, Transportation and Warehousing is an exception: the estimate is significant in Republican-governor and Trump-voting states (-4.3 pp and -3.0 pp, both $p < 0.01$) and null in Democratic-governed states, consistent with a labor supply disruption operating through the heavy concentration of undocumented workers in agricultural transport and logistics (sectors that depend on bulk road and rail movement of food, and agricultural inputs). Similarly, the effects in Home Improvement Centers are concentrated in Trump-voting states (-3.8 pp, $p < 0.1$), consistent with enforcement-induced reductions in undocumented construction labor rather than a broader community response. Other Accommodation and Food Service, In-Person Services, and Durables are significant across governance contexts, suggesting a baseline behavioral response that crosses political geographies.

We interpret the concentration of Accommodation and Food Services, General Merchandise Stores, and Arts, Entertainment, and Recreation effects in Democratic-governed states as a *community behavioral withdrawal* channel, consistent with evidence that enforcement visibility generates fear-based avoidance of public commercial spaces that propagates through immigrant social networks well beyond those directly targeted (Watson, 2014; Alsan and Yang, 2024; Eastus et al., 2025). The concentration of Home Improvement Centers and Transportation and Warehousing effects in Republican-governed and Trump-voting states reflects a *labor supply disruption* channel whose footprint is governed by where undocumented workers are employed in construction and agricultural logistics rather than by community salience.

The paper makes three contributions. First, we provide the first nationally representative, causally identified estimates of how immigration enforcement affects consumer spending, directly measuring an outcome prior work has identified as a likely spillover mechanism (East et al., 2023) but has not estimated. Second, jail-based falsification provides direct evidence that community enforcement visibility, as opposed to general enforcement intensity or awareness, is the primary operative mechanism: jail-based surges, which also generate state-level enforcement awareness, produce near-zero spending effects across every sector where community surges produce significant declines. This advances the chilling-effect literature, which has established that behavioral responses propagate through fear and network effects (Watson, 2014; Alsan and Yang, 2024), by identifying community visibility as the specific trigger that activates those responses in consumer markets. Third, combining sector-level estimates with governance heterogeneity, we identify two empirically distinct channels that produce different sectoral footprints in different environments and that would be indistinguishable in analyses using aggregate spending or pooling across political geographies. Together, these findings establish that the economic costs of immigration enforcement depend critically on both the community response to enforcement as well as effects operating through undocumented worker labor supply.

Our paper sits at the intersection of two literatures. The first is the immigration en-

enforcement literature, which documents the effects of enforcement on labor markets (East et al., 2023; Amuedo-Dorantes and Antman, 2022; Cox and East, 2026; Brizuela et al., 2025; Sojourner and Rosenthal, 2026), program participation and behavioral responses (Watson, 2014; Alsan and Yang, 2024), sectoral employment (Aslim et al., forthcoming, PNAS; Ali et al., 2024; Herbst and Tekin, 2025), and, most recently, student attendance and achievement following the 2025 surge (Dee, 2025; Camp et al., 2026), but has not estimated consumption effects. The second is the high-frequency spending measurement literature, which uses the Affinity Solutions card transaction infrastructure (Chetty et al., 2024) to study how economic shocks propagate through consumer behavior, applied extensively to COVID-19 but not to enforcement shocks. We are the first to connect them. Our findings complement the concurrent education and employment literature by documenting the same behavioral withdrawal mechanism in consumer markets.

The remainder of the paper is organized as follows. Section 1 describes the data. Section 2 presents the empirical strategy. Section 3 reports the main results and falsification, the heterogeneity analysis and channel interpretation. Section 4 concludes.

1 Data

We merge two primary data sources: consumer spending data from the Opportunity Insights (OI) Affinity Solutions dataset as distributed by Chetty et al. (2024), and ICE arrest data from the Data Deportation Project (DDP), a collaboration based at the University of California, Berkeley, that publishes anonymized individual-level arrest records obtained through public litigation and Freedom of Information Act requests.⁴

⁴Both datasets were downloaded on March 10, 2026 and April 1, 2026, respectively, from <https://github.com/OpportunityInsights/EconomicTracker/tree/main/data> and <https://deportationdata.org/data/processed/ice.html>. We supplement these with Current Population Survey (CPS) data to construct state-level monthly population estimates used to normalize the arrest rate and to construct socio-demographic controls.

1.1 Consumer Spending Data

The OI dataset tracks credit and debit card spending by category at the state-month level from January 2020 through February 2026, drawing on transaction records from Affinity Solutions, a company that aggregates consumer card data for financial services applications and captures roughly 10% of U.S. debit and credit card spending. As [Chetty et al. \(2024\)](#) notes, the series covers essentially all consumer spending outside of housing, health care, and motor vehicles, categories in which cards are not the primary payment instrument. The dataset contains an aggregate spending index and 14 disaggregated spending categories available at monthly frequency for all 50 states plus DC.⁵

The Affinity Solutions indices are expressed as cumulative percentage-point deviations from a January 2020 baseline ([Chetty et al., 2024](#)). We rebase all series to September 2023 = 0 separately for each state, so that outcome variables measure percentage-point deviations from each state’s own pre-period spending level. This normalization applies a common additive shift to each state’s series and has no bearing on identification: the SDiD estimator identifies the average treatment effect from within-state variation relative to a synthetic control, and any additive level shift cancels in the differencing.

Consumer spending series exhibit strong seasonal variation that differs across categories and states. We apply a pre-treatment seasonal adjustment to each state-by-category series prior to estimation, computing calendar-month means over the pre-treatment window (January 2022 to December 2024) and subtracting these monthly means from the full series. Restricting the seasonal adjustment to the pre-treatment window is essential: incorporating

⁵The 14 categories are: Accommodation and Food Services (ACF); Apparel and Accessories (AAP); Arts, Entertainment, and Recreation (AER); Durable Goods; General Merchandise and Apparel (APG); General Merchandise (GEN); Grocery and Food Retail (GRF); Health Care and Social Assistance (HCS); Home Improvement Centers (HIC); Other In-Person Services; Non-Durable Goods; Remote Services; Sporting Goods and Hobbies (SGH); and Transport and Warehousing (TWS). We conduct analysis at the state level because the ICE arrest data are reported at the level of ERO Areas of Responsibility, which do not map cleanly to city boundaries. We exclude Remote Services from the sector-level analysis because President Trump’s return-to-office mandate for federal employees, implemented in the first weeks of 2025, generated a direct mechanical shift in remote spending that is contemporaneous with and independent of the ICE enforcement surge; its inclusion would confound enforcement effects with a separate federal policy shock.

post-treatment observations would mechanically attenuate the estimated treatment effect.

A limitation of our data is that it covers only credit and debit card spending and therefore excludes cash purchases. Cash purchases make up a little over 15% of aggregate consumer spending ⁶, with lower card usage among undocumented immigrants and those with lower incomes. To the extent that the undocumented have larger behavioral effects, our results may be a lower bound. ⁷

1.2 ICE Enforcement Data

The DDP raw file records 713,464 individual ICE arrests. Each record includes the date, state, apprehension method, landmark field, and ERO Area of Responsibility (AOR) of the arrest, along with demographic information about the arrestee. From these records, we construct state-by-month arrest counts in several steps.

We restrict to our analysis window of September 2023 through February 2026, leaving 511,486 arrests. We then deduplicate: roughly 2.62% of records are flagged as likely duplicates: cases where the same individual appears multiple times on the same day, typically reflecting case status updates rather than separate arrest events. We drop 229 arrests (less than 0.1% of the sample) with conflicting state information, defined as person-day groups where records disagree about the state of apprehension. After deduplication and conflict removal, we retain 494,837 arrests.⁸

We remove records corresponding to locations outside the 50 states and DC, including U.S. territories, military installations abroad, and foreign locations. We then impute missing

⁶<https://www.frbservices.org/news/research/2024-findings-from-the-diary-of-consumer-payment-choice>

⁷A natural concern is that the estimated decline in card spending reflects not a real reduction in consumption but a mechanical substitution from cards to cash by undocumented households facing heightened enforcement risk. However, undocumented immigrants account for approximately 3 percent of U.S. private-sector GDP and a smaller share of total consumption (Edwards and Ortega, 2017), so even a complete substitution from cards to cash by all undocumented households in treated states could account for only a fraction of the decline we estimate.

⁸Deduplication proceeded in two stages. First, person-day groups with conflicting non-null state values were dropped entirely. Second, among remaining person-day groups with multiple records, we retained the record with the highest geographic priority: non-missing state first, then non-missing landmark, then neither. Ties were broken arbitrarily, as duplicate records are typically identical except for case status.

state values using a multi-strategy approach that closely replicates [Aslim et al. \(forthcoming, PNAS\)](#).⁹ For 11 state-month observations where there are no community-based arrest counts in the administrative data, we set arrests to zero; these observations are concentrated in low-arrest states and do not materially affect our estimates.

We classify each arrest into three mutually exclusive categories by apprehension method: community-based arrests (non-custodial arrests, custodial arrests, worksite enforcement, and ERO reprocessed arrests); jail-based arrests (arrests made through federal, state, and local incarceration facilities and probation and parole); and border-based arrests. Our primary treatment variable is constructed exclusively from community-based arrests, as these most directly capture enforcement activity directed at individuals residing in the community and are therefore most likely to generate the behavioral responses in consumer spending we study. This classification is consistent with [Aslim et al. \(forthcoming, PNAS\)](#) and corroborated by [East et al. \(2026\)](#), who document that community-based arrests more than doubled as a share of total ICE arrests following the second inauguration while the share with prior criminal convictions fell sharply. The jail-based series is used to construct a parallel falsification treatment variable. Appendix Figure A.1 shows that community-based arrests account for the sharp post-January 2025 increase in total ICE arrests, especially after June 2025.

Individual records are aggregated to the state-month level and divided by state population to construct our primary enforcement measure, the monthly community-based arrest rate per 10,000 residents:

$$A_{st} = \frac{\text{CommunityArrests}_{st}}{\text{TotalPopulation}_{st}} \times 10,000, \quad (1)$$

where s indexes states and t indexes months. State-month population estimates are

⁹The four imputation steps, applied sequentially, are: first, for records assigned to one of 10 ERO offices that nominally cover a single state, we imputed the corresponding state; empirical validation confirms match rates of 97% or higher for each office. Second, we applied a curated landmark dictionary mapping high-frequency facility names to states, verified against records with known states. Third, we applied regex extraction scanning landmark text for embedded state names or abbreviations. Fourth, the remaining 932 unresolved unique landmarks were submitted to an LLM for geographic inference using city names, county names, institutional identifiers, and AOR context, resolving 708 of them. The final resolution rate (99.0%) closely matches the 98.1% reported in [Aslim et al. \(forthcoming, PNAS\)](#). A conservative variant that omits ERO-based imputation resolves 97.2% of records.

constructed from the CPS; we apply a three-month centered moving average to smooth month-to-month sampling variability.¹⁰

We scale by total state population rather than foreign-born population, as in [Aslim et al. \(forthcoming, PNAS\)](#), because our outcome aggregates purchases by all residents regardless of nativity. Enforcement may generate behavioral responses not only among directly targeted foreign-born individuals but also among U.S.-born family members, co-ethnic communities, and the broader local economy. Scaling by total population therefore better captures enforcement intensity relative to the full economic community whose spending we measure.

1.3 Enforcement Surge Measure and Treatment Assignment

Our primary treatment variable is a time-invariant, state-level standardized enforcement surge statistic:

$$\Delta_s^{\text{std}} = \frac{(\bar{A}_s^{\text{post}} - \bar{A}_s^{\text{pre}}) - \overline{(\bar{A}^{\text{post}} - \bar{A}^{\text{pre}})}}{\text{SD}(\bar{A}^{\text{post}} - \bar{A}^{\text{pre}})}, \quad (2)$$

where \bar{A}_s^{post} and \bar{A}_s^{pre} are the mean monthly community-based arrest rates per 10,000 residents in state s over February–October 2025 and February–October 2024, respectively. We exclude January 2025 as the transition month and use symmetric pre- and post-windows following [Aslim et al. \(forthcoming, PNAS\)](#). We standardize the surge distribution to have zero mean and unit variance across states.

The surge in community-based enforcement varied substantially across states. Appendix Figure A.3 plots the distribution of Δ_s^{std} : it is highly right-skewed, with the District of Columbia’s score of $\Delta_{\text{DC}}^{\text{std}} = 5.49$ more than three times the next highest state, reflecting the

¹⁰We correct for two CPS population control discontinuities within our sample. The January 2025 revision (approximately 2.9 million, driven by a methodology change in net international migration estimates) and the January 2026 revision, which disproportionately affected population counts for Black and Hispanic groups, are addressed by rescaling the relevant series to ensure consistency across the full sample period. For the January 2025 break, we compute the ratio of national January 2025 to December 2024 population and apply this factor to all pre-2025 state-month observations. For the January 2026 break, we scale post-2025 counts by the December 2025 to January 2026 ratio for each affected demographic group. Both adjustments affect levels but leave population shares unchanged by construction.

combination of a large undocumented population relative to total population in the metro area and the concentration of federal enforcement resources in the capital. The remaining states cluster in a narrow band. Figure 1 shows the distribution of Δ_s^{std} across states and Appendix Table A.1 documents the full cross-state variation: the largest surges occurred in the District of Columbia, Texas, New Mexico, Massachusetts, and Virginia; the smallest in Alaska, Hawaii, and Wisconsin.

States are assigned to the treated group if Δ_s^{std} exceeds the k th percentile of the cross-state distribution, for $k \in \{50, 60, 70\}$. Our primary specification uses $k = 60$, yielding 20 treated states and 31 control states (shown in Table 1). The treated group spans the political spectrum: our 20 P60-treated states include solidly Republican-governed states (Arkansas, Florida, Georgia, Louisiana, Oklahoma, Tennessee, Texas, Utah), Democratic-governed states (California, Colorado, Delaware, the District of Columbia, Massachusetts, New Jersey, New Mexico, Virginia), and mixed-governance states (Arizona, Maryland, Nebraska, Nevada). Crucially, the identifying variation is not a comparison of blue to red states; it is cross-state variation in community-based enforcement intensity among states heterogeneous in every observable respect. We exploit governance context in Section 3.3 as a source of mechanism identification, not as part of our primary identification strategy.

We use a time-invariant binary treatment mechanism as our primary specification for two primary reasons: first, we find that spending is relatively insensitive to enforcement at low levels and that the response is primarily threshold driven (see Section 2.2 for further discussion).¹¹ This is consistent with Eastus et al. (2025) who find negative mental health effects only with larger immigration enforcement. Secondly, the community-based surge was immediate and concentrated in the first two months of the Trump presidency, with 19 of the 20 treated states doubling their community-based arrest rates by February as shown in Appendix Figure A.3.¹²

¹¹The continuous-treatment static DiD estimate is directionally consistent but statistically insignificant ($\hat{\beta} = -0.5745$ pp, SE 0.660). Full results are reported in Appendix Table A.2.

¹²The sole exception is Arizona, which first crossed the threshold in June 2025; our results are robust to dropping one state at a time, including Arizona.

Appendix Table A.3 reports pre-treatment spending and enforcement intensity means and standard deviations for the P60 treated and control groups. Two features are worth noting. First, treated and control states differ in pre-treatment spending levels across several categories: treated states show lower pre-treatment means in Arts, Entertainment, and Recreation (0.075 versus 0.108), Accommodation and Food Services (0.009 versus 0.016), Home Improvement Centers (-0.058 versus -0.044), Transportation and Warehousing (0.040 versus 0.054), and Durable Goods (-0.034 versus -0.024); but these level differences are not a threat to identification. The SDiD estimator does not require treated and control states to begin at the same spending level; it requires only that their weighted trajectories evolve in parallel before treatment, which Figure 2 confirms over 36 pre-treatment months.

Second, control states exhibit somewhat greater within-category dispersion than treated states across nearly all spending categories, reflecting the broader demographic and economic heterogeneity of the control group, which spans the full range of low-surge states. This pattern is benign for identification: the SDiD unit-weight optimization selects the weighted combination of control states that best matches the treated group’s pre-treatment trajectory, so the wider support of the control distribution expands rather than constrains the counterfactual construction.

The one pre-treatment difference that is directly relevant to the design is the arrest rate: treated states averaged 0.088 community-based arrests in the pre-period versus 0.047 for control states, reflecting higher baseline enforcement intensity in high-surge states. The identifying variation is not this level difference but the post-inauguration change, 0.556 versus 0.170 per 10,000, an approximately three-to-one ratio, which constitutes the enforcement shock we study. Figure 2 plots average spending trajectories for the two groups: the series track closely over 36 pre-treatment months and diverge sharply beginning in January 2025.

2 Empirical Strategy

2.1 Estimator

We estimate the average treatment effect on the treated (ATT) using the Synthetic Difference-in-Differences (SDiD) estimator of [Arkhangelsky et al. \(2021\)](#). SDiD generalizes both standard DiD and the synthetic control method ([Abadie et al., 2010](#)) by constructing weights over both control units and pre-treatment time periods, rather than assuming that a simple average of control states provides an adequate counterfactual.

Let Y_{st} denote the seasonally adjusted spending index for state s in month t . Define $W_{st} = \mathbf{1}[s \in \mathcal{T}, t \geq T_0]$, where \mathcal{T} is the set of treated states and T_0 denotes January 2025. The pre-treatment window spans January 2022 through December 2024 ($T_{\text{pre}} = 36$ months) and the post-treatment window spans January through September 2025 ($T_{\text{post}} = 10$ months).

SDiD constructs state weights $\hat{\omega}_s$ by solving the regularized quadratic program of [Arkhangelsky et al. \(2021\)](#):

$$(\hat{\omega}_0, \hat{\omega}) = \arg \min_{\omega_0, \omega \geq 0, \sum \omega_s = 1} \sum_{t=1}^{T_{\text{pre}}} \left(\omega_0 + \sum_{s \in \mathcal{C}} \omega_s Y_{st} - \frac{1}{N_T} \sum_{s \in \mathcal{T}} Y_{st} \right)^2 + \zeta^2 T_{\text{pre}} \|\omega\|_2^2, \quad (3)$$

where the ridge penalty $\zeta = (N_T T_{\text{post}})^{1/4} \hat{\sigma}$ is calibrated to the variance of first-differences in control outcomes. By construction, the weighted average of control states' pre-treatment spending path parallels that of the treated group.

SDiD constructs time weights $\hat{\lambda}_t$ that assign greater importance to pre-treatment months that best predict the post-treatment counterfactual trend. In our baseline P60 specification, nonzero time weights concentrate in four months (December 2023, February 2024, March 2024, and November 2024). The SDiD ATT is:

$$\hat{\tau}^{\text{sdid}} = (\bar{Y}_{\text{tr}}^{\text{post}} - \bar{Y}_{\text{tr}, \hat{\lambda}}^{\text{pre}}) - (\bar{Y}_{\hat{\omega}, \text{co}}^{\text{post}} - \bar{Y}_{\hat{\omega}, \hat{\lambda}, \text{co}}^{\text{pre}}), \quad (4)$$

We estimate equation (4) separately for the aggregate spending index and for each of the 14 disaggregated spending categories. Standard errors are computed using clustered bootstrap with 500 replications; we report jackknife standard errors as a robustness check in Table 2, column 9.

Why SDiD rather than standard DiD. SDiD’s unit weights absorb these pre-existing differences by finding the weighted combination of control states that matches the treated group’s pre-treatment path. [Arkhangelsky et al. \(2021\)](#) show that, under a latent factor model for the controls’ counterfactual outcomes, SDiD is consistent and asymptotically normal in settings where standard DiD is biased due to non-parallel trends. The joint F-test for pre-treatment event-time coefficients passes under a levels specification ($F = 0.86$, $p = 0.548$) but fails under logs ($F = 1.96$, $p = 0.080$; $F = 5.87$, $p < 0.001$ over a longer window). This sensitivity to functional form suggests that the parallel trends assumption is fragile, motivating SDiD as our primary estimator. Full results are reported in Appendix Table A.2.

2.2 Threshold Effects and Binary Treatment

We test the relationship between surge intensity and spending responses directly by implementing a linear approximation of the dose-response function, following [Callaway et al. \(2024\)](#). For each treated state i , we compute the simple pre-post spending change relative to the control group mean, $ATT_i = \Delta Y_i - \overline{\Delta Y}_{\text{control}}$, and regress these state-level estimates on Δ_s^{std} via OLS. While the ATT is significant at -2.1 percentage points ($p \leq 0.05$) for all three percentile definitions, the slope is small and statistically insignificant across all specifications: at P50 ($N_{\text{tr}} = 25$), $\hat{\beta} = -0.159$ ($p = 0.791$, $R^2 = 0.003$); at the primary threshold ($\Delta_s^{\text{std}} > 0$, $N_{\text{tr}} = 20$), $\hat{\beta} = +0.208$ ($p = 0.725$, $R^2 = 0.007$); and at P70 ($N_{\text{tr}} = 15$), $\hat{\beta} = +0.475$ ($p = 0.460$, $R^2 = 0.043$). The slope changes sign across specifications and surge intensity explains less than five percent of the variation in state-level spending responses in all cases. Spending responses do not scale with enforcement intensity; states that experienced larger

surges did not experience proportionally larger spending declines.

This finding is consistent with the mechanisms the chilling-effect literature identifies. [Watson \(2014\)](#) documents that enforcement reduces Medicaid participation among citizen children of noncitizens, households with no direct deportation risk, establishing that behavioral responses propagate through community-level salience rather than individual exposure. [Alsan and Yang \(2024\)](#) show that Secure Communities reduced participation in safety-net programs among Hispanic citizen households through network-based fear propagation, with spillovers on citizens as large as the direct effects on noncitizens. Both findings are more consistent with a mechanism that activates at some threshold of community awareness and then saturates than with one that scales proportionally with arrest counts. In the educational domain, [Camp et al. \(2026\)](#) document that the dominant effect of the same enforcement surge on student attendance operates through a persistent baseline shift rather than acute responses to specific enforcement events, consistent with a threshold mechanism that activates at some level of community awareness and then saturates.

Together, the flat dose-response and the salience-saturation mechanism motivate modeling treatment as binary. We assign states to the treated group if Δ_s^{std} exceeds the k th percentile of the cross-state distribution, for $k \in \{50, 60, 70\}$. Two features of the surge distribution reinforce this choice. First, the distribution is highly right-skewed: DC’s standardized surge score (5.49) is approximately 2.8 times that of the next highest state, Texas (1.95), so a continuous specification would concentrate identification almost entirely in a single observation. Second, the near-simultaneous onset of the surge, 19 of 20 treated states first exceeded twice their pre-period community-based arrest rate in January or February 2025, means that timing variation, which generated naturally binary treatment in prior work ([East et al., 2023](#); [Alsan and Yang, 2024](#)), is absent in our treatment variable; intensity is the only available source of cross-state variation, and the flat dose-response confirms that binarizing it loses no information about the treatment effect. The stability of ATT estimates across all three thresholds (discussed in Section 3.1 and shown in Table 2) and the leave-one-out

robustness check (Appendix Table A.4) confirm that results are not driven by threshold choice or by DC’s very high score.

2.3 Identification

Addressing non-random enforcement intensity is a recurrent challenge in the immigration enforcement literature (Amuedo-Dorantes and Antman, 2022; Dee, 2025; Aslim et al., forthcoming, PNAS; Camp et al., 2026; Cox and East, 2026). We now address the necessary assumptions for our results to be interpreted as causal.

First, in the absence of treatment, treated and control states must have followed parallel spending trajectories, an assumption SDiD relaxes relative to standard DiD by constructing unit and time weights that match pre-treatment paths by construction (Arkhangelsky et al., 2021). SDiD unit weights absorb pre-existing differences between treated and control states by matching their pre-treatment spending paths without requiring enforcement to be orthogonal to state characteristics. Arkhangelsky et al. (2021) prove formally that SDiD remains consistent under pre-trends that would invalidate standard DiD. The close trajectory match in Figures 2 and 3 across 36 pre-treatment months provides direct evidence that this condition is satisfied.

Second, treated states must not have begun adjusting spending before January 2025. At the aggregate level, Figure 2 is reassuring: treated and control series track closely through December 2024, with divergence beginning in January 2025. At the sectoral level, Figure 3 reveals some heterogeneity: Accommodation and Food Services, In-Person Services, and Transportation and Warehousing show clean parallel pre-trends, while Durable Goods and Home Improvement Centers display some treated-state divergence in the second half of 2024. We interpret this cautiously: it may partly reflect anticipation effects following the November 2024 election, or pre-existing differential trends in these sectors unrelated to enforcement intensity. Importantly, SDiD’s time-weighting assigns lower weight to pre-period months where trends diverge, providing some robustness to this concern even when TWFE would

not; formal pre-trend tests are reported in Appendix Table A.6.

Appendix Table A.6 reports two complementary sectoral diagnostics. The placebo-in-time estimates in column (1) are small and statistically indistinguishable from zero for twelve of the fourteen spending categories, indicating that the SDiD-reweighted control units do not generate spurious post-period gaps in the pre-treatment window. The pre-period fit diagnostics in columns (2)–(4) reinforce this conclusion: the actual RMSE between the treated mean and the SDiD-reweighted control mean falls well below the median of the placebo distribution in nearly every outcome, with rank pp p-values at or near 1.00, confirming that the synthetic control weights achieve pre-treatment balance within the range expected under parallel trends. Two exceptions are Arts, Entertainment & Recreation and Sporting Goods & Hobbies, which return marginally significant placebo-in-time estimates (-1.78^* and -2.16^{**} , respectively); we interpret results for these sectors with additional caution.

The placebo-in-time tests (Table 2, column 11, and Appendix Table A.7, column 9) suggest that our SDiD estimator is not mechanically recovering a spurious trend rather than a genuine post-treatment break. We nonetheless caution that the no-anticipation assumption is more credible for aggregate spending and service-sector outcomes than for Durable Goods, Home Improvement Centers, Arts, Entertainment & Recreation, and Sporting Goods & Hobbies.

Third, SUTVA: our results hinge on the assumption that the spending response in one state not spill over into others. Given that our outcome is state-level card spending and our treatment is a state-level surge measure, cross-state spillovers would require enforcement in one state to affect consumer behavior in another, a scenario we consider unlikely at the magnitudes we study.

Jail-based falsification. The most direct evidence against confounding effects is our jail-based enforcement test as jail-based arrests are less visible to the community. In contrast, community arrests occur in neighborhoods, are observed, and propagate through social

networks in ways that jail-based arrests do not. The jail-based aggregate ATT is -0.85 pp ($p = 0.302$), and all seven sectors with significant community enforcement effects produce near-zero, statistically null estimates under jail enforcement (Section 3.2). The falsification isolates visibility as the operative channel and rules out confounders that would affect both enforcement types equally.

Non-in-person null results. As a second falsification test, we construct a non-in-person spending variable aggregating all Affinity Solutions categories not classified as requiring physical presence. If our results reflected generic state-level demand shock correlated with enforcement intensity (whether from tariff-driven uncertainty, or aggregate enforcement), non-in-person spending should decline alongside in-person categories. It does not: Table 3 shows that non-in-person ATT is null and close to zero across all threshold specifications, ruling out broad regional demand shocks and isolating enforcement visibility as the operative channel.

Time-based placebo and other tests. We estimate our effects limiting both our pre- and post-period to the period before January 2025 to test whether uncorrected state-based trends might be driving our effects. These results shown in Table 2, column 11 yield a null result close to zero. Similarly, limiting our sample period to the period prior to April 2025 when tariffs were announced, does not materially affect our estimates (see Table 2, column 7), nor does dropping states in the DC metropolitan area (Table 2, column 5) which might be affected by confounders related to the implementation of DOGE.

Weak selection on observables. A regression of Δ_s^{std} on the state characteristics most likely to confound our estimates — median household income, foreign-born share, Democratic governance, and voted for Harris— yields $R^2 = 0.14$ with no predictor reaching the five-percent significance threshold (shown in Appendix A.5). We do not interpret this as evidence of random assignment as unobservables remain possible. But the failure of the most theoretically

motivated selection channels to predict surge intensity raises the prior that residual selection is limited.

Stability under covariate adjustment. The covariate-adjusted aggregate ATT is -1.89 pp versus the unadjusted -1.73 pp, a difference of 0.22 pp well within a standard error.¹³ The direction is inconsistent with negative selection bias: if states with larger surges were independently experiencing weaker spending growth, adding controls should attenuate the estimate, not amplify it. Full sector-level results under covariate adjustment are reported in Appendix Table A.7 (column 2) and discussed in Section 3.2.

3 Main Results

3.1 Aggregate Consumer Spending

Table 2 reports SDiD estimates of the effect of the 2025 community-based ICE enforcement surge on aggregate consumer spending. The primary specification (P60, column 2) yields an ATT of -1.73 pp (SE 0.76 , $p = 0.022$), indicating that states in the top 40 percent of the community-based enforcement surge distribution saw seasonally adjusted consumer spending fall by approximately 1.73 percentage points relative to the SDiD-reweighted counterfactual over the nine-month post-treatment window. Given the pre-treatment overall spending in the control states of 2.69 percentage points, the economic impact of the estimated enforcement decline on overall spending is approximately 64% of the pre-treatment counterfactual spending.

We contextualize the magnitude of the community-based ICE enforcement surge on aggregate consumer spending by comparing it to the effect of COVID-19 had on aggregate consumption as follows. Since the COVID-19 pandemic reduced aggregate card spending by approximately 23.6 percentage points at the trough according to the same Affinity Solutions

¹³Adding to this group of covariates, the time-varying state controls for Hispanic share, Black share, married share, and age-group shares delivers a similar covariate-adjusted ATT, namely -1.95 pp (SE 0.98).

data (Chetty et al., 2024)¹⁴; our aggregate enforcement effect represents approximately 7 percent of that decline, a meaningful magnitude for a targeted policy shock affecting a specific subpopulation rather than a universal health emergency.

Figure 2 displays the underlying SDiD trajectories: treated and reweighted control series track closely over 36 pre-treatment months and diverge sharply beginning in February 2025, providing visual confirmation that the identifying assumption is satisfied.

Columns 1 to 3 in Table 2 show that the estimate is stable across alternative threshold definitions. The P70 specification, which restricts the treated group to the 15 highest-surge states, yields an ATT of -1.75 pp (SE 0.81, $p = 0.031$), virtually identical to the P60 baseline. The P50 estimate of -1.24 pp is somewhat smaller in magnitude and noisier (SE 0.89), which is expected: including lower-surge states in the treated group dilutes the average treatment intensity and, under the salience-based mechanism described in Section 2.2, should attenuate the estimated effect. Taken together, the three specifications trace out a dose-response that is flat in enforcement intensity, consistent with the Callaway et al. (2024) dose-response analysis documented in Section 2.2, but stable in the direction and approximate magnitude of the ATT.

Column 4 adds time-varying state controls for median household income, foreign-born share, and metropolitan share. The covariate-adjusted ATT is -1.89 pp (SE 0.84), marginally larger in absolute value than the unadjusted baseline.¹⁵ The direction of the change suggest that the omitted variable bias would, if anything, underestimate the true effect. Importantly, the stability of the estimate across adjusted and unadjusted specifications indicates that the SDiD unit weights have already achieved effective balance on the dimensions most likely to confound.

Three further columns address the most plausible sources of concurrent confounding. Column 5 drops Virginia, Maryland, and DC, states with large shares of federal workers

¹⁴According to estimates from <https://economictracker.org/>, all consumer spending fell by 23.6 percentage points as of April 30, 2020, compared to January 1, 2020.

¹⁵Adding to this group of covariates, the time-varying state controls for Hispanic share, Black share, married share, and age-group shares delivers a similar covariate-adjusted ATT, namely -1.95 pp (SE 0.98).

disproportionately exposed to DOGE-related workforce reductions and that share the labor market with DC (an outlier in our Δ_s^{std} outcome variable), and yields an ATT of -1.81 pp (SE 0.83, $p = 0.028$), confirming that these two states and DC are not driving the main result. Column 6 drops the three states that are at the upper tail of the community-based arrest surge distribution: New Mexico, Texas, and DC. Doing so, yields a tad smaller ATT estimate of -1.42 pp (SE 0.675, $p = 0.035$).

Column 7 truncates the post-treatment window to January to March 2025, before the April 2025 tariff shock raised the average effective tariff rate from approximately 3.4% to 10.2%. The pre-tariff ATT is a tad smaller in size: -1.22 pp (SE 0.60, $p = 0.040$), yet it remains significant at the five-percent level and still represents about 5.2% of the peak COVID decline, despite the short post-January 2025 period. Importantly, this rules out that our estimates are driven by the tariffs. Column 8 uses a non-symmetric shock window (December 2023 to December 2024 pre; January 2025 to February 2026 post) and finds an ATT of -2.12 pp (SE 0.86, $p = 0.013$), if anything larger, ruling out sensitivity to the symmetric window assumption.

Appendix Table A.4 reports leave-one-out estimates dropping each of the 51 states in turn. The range spans -1.25 pp (dropping New Mexico, $p = 0.076$) to -2.01 pp (dropping Rhode Island); 44 of 51 are significant at the 5% level and 51 at the 10% level. Dropping DC yields -1.58 pp (SE 0.81, $p < 0.05$), confirming the result is not a DC artefact. Column 9 replaces the wild cluster bootstrap with jackknife standard errors and finds an identical point estimate (ATT = -1.73 pp, SE 0.74, $p = 0.019$), confirming that inference does not depend on the SE method.

Placebo and falsification. Column 10 redefines treatment using the jail-based arrest surge. The jail-based aggregate ATT is -0.85 pp (SE 0.83, $p = 0.302$), indistinguishable from zero. This falsification result, discussed in detail alongside the sector analysis in Section 3.2, provides the most direct evidence that the community-based spending decline reflects the

visibility and salience of community enforcement rather than a generic confound correlated with any form of ICE attention.

The last column of Table 2 (column 11) presents a placebo-in-time test where the real P60 treated set is retained, but treatment is assigned a fake onset date of July 2024 and the SDiD ATT is estimated over a window that runs from Jan 2022 to Dec 2024. As the aggregate placebo estimate is close to 0 (0.07 pp) and statistically null (SE 0.23), it is suggestive that our SDiD estimator is not mechanically recovering a spurious trend rather than a genuine post-treatment break.

3.2 Sector-Level Estimates and the Jail-Based Falsification

Table 3 decomposes the aggregate ATT into 13 spending categories plus a non-in-person residual, reporting estimates under P50, P60, and P70 alongside the jail-based falsification. We classify seven sectors to be robust: those demonstrating point-estimate stability and significance at the P60 specification and across the majority of robustness checks in Appendix Table A.7.

Spending on Home Improvement Centers (HIC) shows the largest point estimate at -5.92 pp (SE 2.71, $p < 0.05$). This sector relies heavily on immigrant labor for both supply (construction and renovation workers) and demand (homeowners and renters undertaking improvement projects), making it particularly sensitive to both labor-supply withdrawal and fear-driven demand reduction. When we exclude the post-tariff period, the estimate declines to -2.72 pp (SE 1.45, $p < 0.10$), representing a 47% decline relative to the pre-treatment treatment mean of -5.82 pp. All other robustness checks yield estimates close to the P60 baseline, and the effect passes both the falsification and placebo tests. Consistent with lower construction and remodeling activity, spending on Durable Goods also falls by -2.66 pp (SE 0.98, $p < 0.01$), or 78% of the pre-treatment treatment mean (shown in Appendix Table A.3). This estimate is robust to all sensitivity analyses including the pre-tariff window, and passes both the falsification and placebo tests.

Also consistent with reduced labor supply by immigrant workers and reduced restaurant and hospitality patronage, spending on Accommodation and Food Services (ACF) declines by -1.77 pp (SE 0.76, $p < 0.05$). All but one robustness test are of similar economic size and significant at the 95% level. Furthermore both falsification and placebo tests are satisfied.

Spending on In-Person Services declines by -2.44 pp (SE 0.70, $p < 0.01$), representing a 77% decline relative to the pre-treatment mean. Spending on Sporting Goods and Hobbies (SGH), General Merchandise and Apparel (APG) and Transportation and Warehousing (TWS) fall by -3.59 pp (SE 2.11, $p < 0.1$); -3.54 pp (SE 1.93, $p < 0.1$), and -3.31 pp (SE 1.40, $p < 0.05$), respectively. When restricted to the pre-tariff period, the TWS estimate declines to -2.21 pp (SE 1.23, $p < 0.1$), while the SGH estimate holds at -3.46 pp (SE 2.03, $p < 0.1$) and APG widens to -3.78 pp (SE 1.52, $p < 0.05$). These two latter estimates are consistent with a robust chilling effect as Sporting Good and Hobbies and General Merchandise and Apparel retail establishments are frequently co-located with home improvement and durable goods stores in large-format retail corridors that were subject to heightened and media-reported ICE enforcement activity.

Viewed through the lens of [Moretti \(2010\)](#), many of these sectors form a coherent pattern: they are precisely those he classifies as non-tradable –food services, in-person services, merchandise and apparel, and construction– where production requires the physical co-presence of local workers and consumers and cannot be substituted by purchasing from outside the local market. The concentration of enforcement effects in non-tradable sectors implies that the spending declines we estimate are likely to generate downstream employment consequences for the low-wage service workers most exposed to local demand contractions, many of whom are not themselves immigrants. This connects our consumer spending findings directly to the labor market spillover mechanism that [East et al. \(2023\)](#) identify and [Cox and East \(2026\)](#) document, extending it to the demand side.¹⁶

¹⁶Durable Goods and Sporting Goods and Hobbies are tradable in Moretti’s strict classification. Their effects most plausibly operate through the local retail channel, the physical stores where these goods are purchased, rather than the goods themselves being nontradable. The co-location of SGH and HIC retail establishments in large-format retail corridors, noted above, is consistent with this interpretation.

Sector falsification tests. As shown in Table 3, across every sector where community enforcement produces a significant decline in consumption, the jail-based estimate is considerably smaller (frequently near zero) and statistically insignificant. The contrast is sharpest for In-Person Services (community: -2.44 pp; jail: -0.60 pp), ACF (community: -1.77 pp; jail: -0.21 pp), and SGH (community: -3.59 pp; jail: $+1.01$ pp). For HIC, the jail estimate of -2.08 pp (SE 2.92) is directionally smaller and statistically null. The one exception is Arts, Entertainment, and Recreation (AER), with similar estimates under the jail- and community- specifications (-3.55 pp, SE 1.94 for the jail-based, and -3.30 pp, SE 2.00 for the community-based), albeit in both cases, statistical significance is at 90% level; as AER is not among the seven robust sectors, the jail estimate does not represent attenuation of a community effect. Importantly, Non-In-Person Spending is also null ($+0.70$ pp, SE 0.64): activities conducted online and at a distance from the community are insulated from the salience of neighborhood enforcement operations.

Treatment onset and timing of the effects. We define the treatment period as beginning in January 2025, because it is when executive orders directing a substantial expansion of interior enforcement operations were signed ([Executive Office of the President, 2025](#)). The spending trajectories in Figures 2 and 3 are consistent with this choice: treated and control series track closely through December 2024 and diverge beginning in January or February 2025. The placebo-in-time test (Table 2, column 11), which assigns a fake treatment onset of July 2024 while retaining the true P60 treated set, yields a null aggregate ATT of $+0.03$ pp, confirming that our SDiD estimator is not recovering a spurious pre-existing trend. The sectoral trajectories in Figure 3 reveal heterogeneity in the timing of the spending response that is itself informative about the operative mechanism. Aggregate spending, In-Person Services, and Sporting Goods and Hobbies show an immediate post-inauguration decline, consistent with a behavioral channel that activates as soon as community enforcement becomes visible and salient to surrounding communities. Accommodation and Food Services, and

Home Improvement Centers exhibit a modest delay of one to two months, consistent with a labor supply channel that takes time to manifest as workers are removed from households and construction and renovation projects are disrupted. This sectoral timing pattern corroborates the two-channel interpretation we develop further in the next section.

3.3 Heterogeneity Analysis

Governance heterogeneity and channel identification. States with Democratic governors experienced an aggregate ATT of -4.09 pp (SE 1.51, $p < 0.01$), more than six times the -0.61 pp (SE 0.46) estimated for Republican-governor states, which is statistically indistinguishable from zero. This contrast does not reflect differences in community arrest intensity across governance contexts as Appendix Figure A.2 shows substantial community surges in both Democratic- and Republican-governed treated states. Rather, it reflects differences in how communities perceived and responded to enforcement of comparable visibility. The rescission of Biden-era protected-areas guidance opened schools, houses of worship, hospitals, and child-care centers to enforcement operations (Pearson, 2025), and heightened media coverage of large-scale operations amplified enforcement salience well beyond what arrest counts alone capture (Herbst and Tekin, 2025). A large literature documents that enforcement generates fear-based withdrawal from public spaces and social networks among Latino immigrants and their families, including among individuals with no direct deportation risk, through psychological distress and network-based fear propagation (Watson, 2014; Alsan and Yang, 2024; Wang and Kaushal, 2019; Eastus et al., 2025). These responses appear larger where enforcement salience was highest, where protected-area rescissions and media coverage of large-scale operations amplified fear beyond what arrest counts alone would predict.

At the sector level, Democratic-governor treated states show large significant declines in Accommodation and Food Services (-2.31 pp, $p < 0.10$), General Merchandise (-6.72 pp, $p < 0.10$), Arts, Entertainment, and Recreation (-7.31 pp, $p < 0.05$), and Sporting Goods and Hobbies (-5.79 pp, $p < 0.10$): mostly discretionary, publicly-visible categories where

behavioral withdrawal from public commercial life concentrates. Republican-governor states show fewer and smaller significant estimates in discretionary categories, with In-Person Services (-2.40 pp, $p < 0.05$), Accommodation and Food Services (-1.48 pp, $p < 0.10$), and Durable Goods (-1.72 pp, $p < 0.10$), suggesting a baseline behavioral response that crosses political geographies.

Transportation and Warehousing is a clear exception, significant in Republican-governor (-4.29 pp, $p < 0.01$) and Trump-voting states (-3.03 pp, $p < 0.01$) but null in Democratic-governor and Harris-voting states, consistent with labor supply disruption through the concentration of undocumented workers in agricultural transport and logistics in Republican-leaning states. Home Improvement Centers (-3.76 pp, $p < 0.10$ in Trump-voting states, null in Harris-voting states) follow the same pattern, consistent with enforcement-induced removal of undocumented construction workers depressing both contractor supply and homeowner demand (East et al., 2023; East and Cox, 2026).

We interpret the concentration of Accommodation and Food Services, General Merchandise, Arts, Entertainment, and Recreation, and Sporting Goods effects in Democratic-governed states as a *behavioral withdrawal channel* operating through fear-induced avoidance of public commercial life (Watson, 2014; Alsan and Yang, 2024), and the concentration of Home Improvement Centers and Transportation and Warehousing effects in Republican-governed and Trump-voting states as a *labor supply disruption channel* whose footprint is governed by where undocumented workers are employed (namely, in construction and agricultural logistics).

Demographic heterogeneity. Appendix Table A.8 reports SDiD estimates of aggregate and sector-level spending estimated separately by demographic subgroups. It shows that above-median-income states and states with above-median shares of foreign-born people show larger point estimates, but all demographic subgroup contrasts are statistically imprecise, reflecting the small number of treated states per cell ($N_T = 4-15$). We do not draw strong

conclusions from these comparisons.

4 Discussion and Conclusion

This paper provides the first national, causally identified, sector-level estimates of how immigration enforcement affects consumer spending. Exploiting the 2025 surge in community-based ICE arrests as a quasi-experimental shock with cross-state variation in intensity, and using synthetic difference-in-differences to construct the counterfactual, we find that states in the top 40 percent of the enforcement surge distribution experienced aggregate card spending declines of approximately 1.7 percentage points relative to their synthetic controls. Significant effects appear in seven sectors: Accommodation and Food Services, General Merchandise and Apparel, Other In-Person Services, Home Improvement Centers, Durable Goods, Sporting Goods and Hobbies, and Transportation and Warehousing. Grocery spending is null throughout. The aggregate decline represents approximately 64% of the pre-treatment counterfactual spending level and is equivalent to approximately 7% of the peak COVID-19 spending decline in the same Affinity Solutions data (Chetty et al., 2024), a meaningful magnitude for a targeted policy shock affecting a specific subpopulation rather than a universal health emergency. Concurrent work documents that the same enforcement surge generated persistent increases in student absenteeism (Dee, 2025; Camp et al., 2026) and increased labor force participation among US-born teenagers in mixed-status households at the expense of school enrollment (East and Cox, 2026); our consumer spending estimates complement these findings by quantifying the market-level aggregate of the same behavioral and income channels. Most directly relevant to our labor supply channel, Cox and East (2026) find that areas experiencing sudden large increases in ICE arrests in 2025 saw significant reductions in work among likely undocumented immigrants who remain in the U.S., with no offsetting gains for U.S.-born workers. Area-specific studies also document labor market disruption in Minnesota (Sojourner and Rosenthal, 2026) and Texas (Brizuela et al., 2025). This literature

documents the supply-side contraction; we document the demand-side transmission.

The jail-based falsification is the paper’s most direct test of the mechanism. Because jail-based and community-based arrests are comparable in their direct labor market impact but differ categorically in community visibility, the near-zero estimates across all seven significant sectors under the jail specification identify community enforcement visibility (not the aggregate removal of workers from labor markets) as the operative mechanism. The rescission of Biden-era protected-areas guidance, which opened schools, houses of worship, hospitals, and child-care centers to enforcement operations (Pearson, 2025), and heightened media coverage of large-scale operations amplified enforcement salience well beyond what arrest counts alone capture (Herbst and Tekin, 2025), helping to explain why spending declines are large relative to the share of the population directly targeted. This finding has a direct policy implication: the economic costs of immigration enforcement depend substantially on the technology through which it is deployed. Enforcement that operates invisibly through correctional facilities generates little measurable impact on consumer spending; enforcement that is publicly visible generates large, broad spending declines that extend well beyond the directly targeted population.

The governance heterogeneity results reveal two empirically distinct transmission channels concealed in aggregate estimates. The *behavioral withdrawal channel* operates with greatest force in Democratic-governed states, where community arrest surges were accompanied by higher enforcement salience and media visibility: these states experience an aggregate ATT of -4.09 pp, with large significant declines in Accommodation and Food Services; General Merchandise; Arts, Entertainment, and Recreation; and Sporting Goods and Hobbies — discretionary, publicly-visible categories where fear-induced withdrawal (Watson, 2014; Alsan and Yang, 2024) from public commercial life concentrates. The *labor supply disruption channel* operates through a different mechanism and footprint: spending in Home Improvement Centers declines by -3.76 pp in Trump-voting states, consistent with enforcement-induced removal of undocumented construction workers depressing both contractor demand and consumer

demand for home improvement; Transportation and Warehousing declines significantly in Republican-governor states (-4.29 pp), consistent with disruption to undocumented labor in agricultural transport and logistics (East et al., 2023; East and Cox, 2026). Cox and East (2026) provide direct evidence of this mechanism, finding that areas with sudden ICE arrest surges in 2025 experienced significant reductions in the stock of likely undocumented workers, consistent with enforcement-induced labor supply withdrawal depressing activity in construction and agricultural transport.

Finally, we observe that Durable Goods and In-Person Services decline immediately across governance contexts, consistent with a baseline behavioral response that crosses political geographies. Analyses pooling across governance contexts, or focusing on aggregate spending alone, would fail to detect either channel distinctly.

Because many of the affected sectors are precisely those Moretti (2010) classifies as non-tradable, that is, food services, in-person services, construction, and local retail, the spending declines we document are likely to generate downstream employment consequences for the low-wage service workers most exposed to local demand contractions, many of whom are not themselves immigrants.¹⁷ The immediate sectoral declines we observe are consistent with direct behavioral withdrawal and labor supply disruption; the Moretti multiplier represents a likely subsequent propagation through local labor markets rather than a contemporaneous mechanism. The economic costs of the enforcement surge thus extend beyond the directly targeted population through two reinforcing mechanisms: behavioral withdrawal from public commercial life and employment disruption in immigrant-intensive sectors, each likely to propagate through the local economy via the multiplier that Moretti (2010) identifies for non-tradable employment.

Several limitations should inform how these results are interpreted. Our outcome is card-

¹⁷A concurrent policy analysis by Sojourner and Rosenthal (2026), using synthetic difference-in-differences applied to Homebase payroll data, finds that a large-scale DHS enforcement surge in the Minneapolis–Saint Paul metropolitan area in January 2026 reduced employees working by 2.8% and total hours worked by 1.9% relative to synthetic controls, with business locations open falling 1.7%. These labor market effects are consistent with the labor supply disruption channel we identify in Transportation and Warehousing and Home Improvement and Construction spending.

channel expenditure, which understates true spending effects in high-cash sectors and among undocumented households, who have lower card penetration. Grocery, the highest-cash sector, shows a null result, and the true food consumption effect of enforcement may be nonzero but invisible in card data. The Home Improvement Centers full-period estimate likely reflects a mixture of enforcement and tariff effects; the pre-tariff estimate of -2.72 pp should be treated as the enforcement-specific component. Finally, our state-level data permit identification of the channels through their sectoral footprints and governance heterogeneity, but do not support separate quantification of behavioral withdrawal, direct income destruction, and secondary labor market contractions within sectors.

The enforcement episode studied here is unusually large and unusually visible, but the framework, combining sector-level spending profiles with enforcement technology variation to identify distinct transmission channels, applies wherever high-frequency outcome data and cross-sectional variation in enforcement intensity or technology are available. As enforcement regimes continue to evolve, the distinction between visible and invisible enforcement technology identified here offers a tractable basis for evaluating the economic consequences of specific policy designs.

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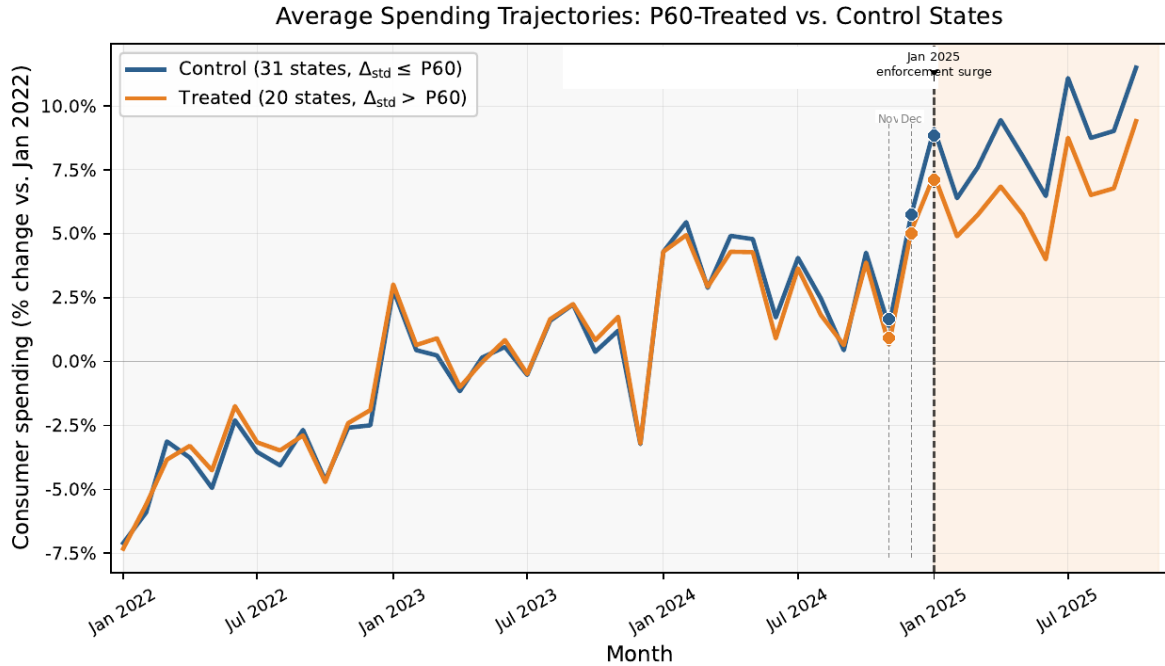


Figure 2: Average Consumer Spending Trajectories, P60-Treated vs. Control States. *Notes:* Unweighted cross-sectional means of seasonally adjusted consumer spending (`spend_s_all_sa22`, Affinity Solutions / Opportunity Insights), expressed as the percentage change relative to January 2022, by treatment status. Treated states (orange, $N = 20$) are those whose standardized surge intensity Δ_i^{std} exceeds the 60th percentile of the pooled cross-state distribution; control states (blue, $N = 31$) are below or at the cutoff. The dashed vertical line marks January 2025, the start of the community-based enforcement surge; Jan 2022–Dec 2024 are the pre-treatment months. The post-period gap of roughly 2 percentage points emerges within the first quarter of 2025. The corresponding SDiD ATT is -1.73 percentage points ($p = 0.022$; Table 2, column 2).

Average Sector-Level Spending Trajectories: P60-Treated vs. Control States

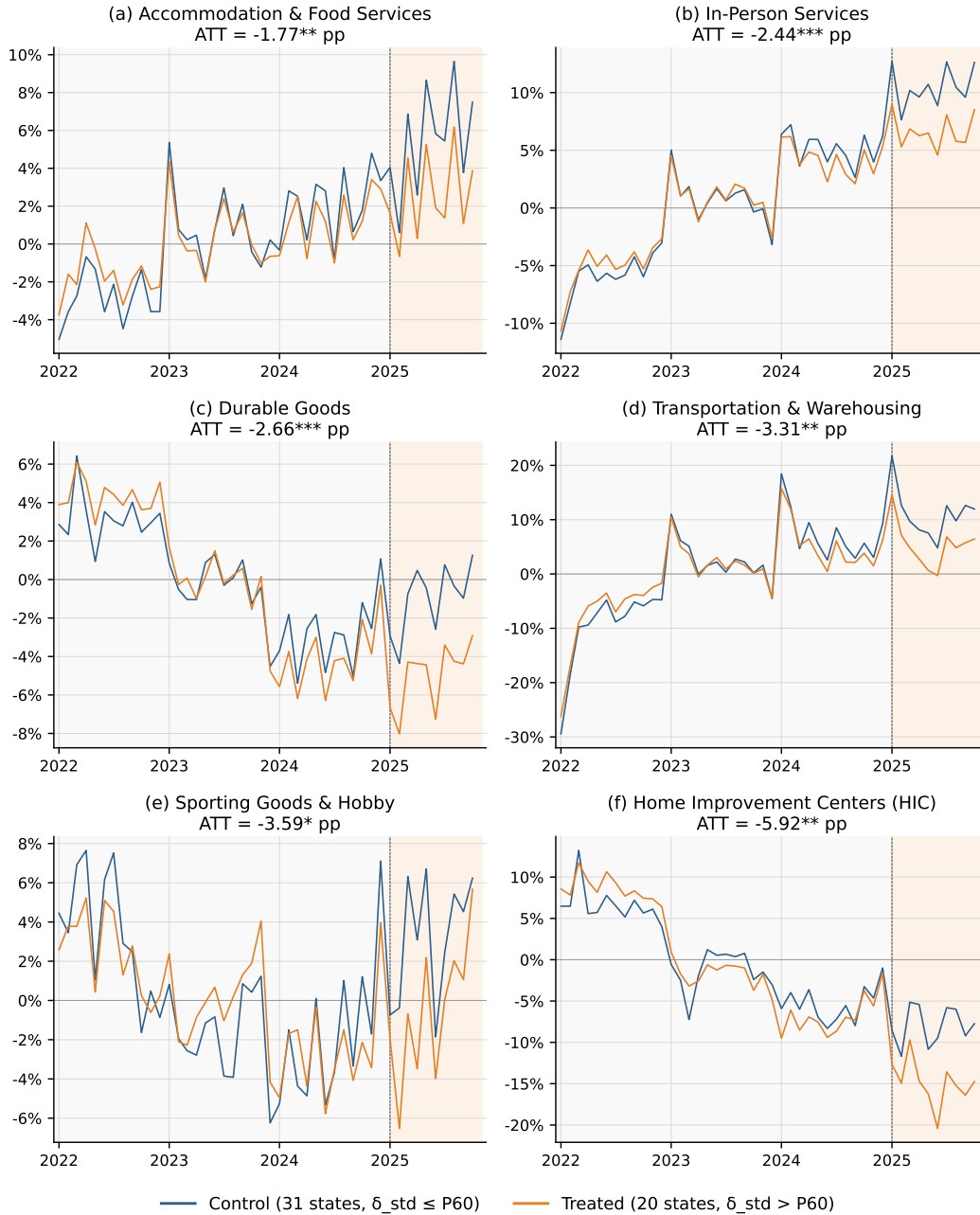


Figure 3: Average Consumer Spending Trajectories by Sector, P60-Treated vs. Control States.

Notes: Unweighted cross-sectional means of seasonally adjusted sector-level consumer spending (`spend_s*_sa22`, Affinity Solutions / Opportunity Insights), expressed as the percentage change relative to January 2022, by treatment status. Treatment assignment is identical to Figure 2: treated states (orange, $N = 20$) are those whose standardized surge intensity Δ_i^{std} exceeds the 60th percentile of the pooled cross-state distribution; control states (blue, $N = 31$) are below or at the cutoff. The dashed vertical line marks January 2025; Jan 2022–Dec 2024 are the pre-treatment months. Panels (a)–(f) are ordered by ATT magnitude (smallest to largest, left-to-right then top-to-bottom). Sector ATTs above each panel are SDiD point estimates from Table 3 column 2; significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Y-axes vary across panels; note that Home Improvement & Construction (HIC) and Transportation & Warehousing (TWS) span wider ranges than the others.

Table 1: Treatment Group Composition by Surge-Intensity Threshold

Spec.	N_{tr}	Treated states
P50	25	AL, AR, AZ, CA, CO, DC, DE, FL, GA, LA, MA, MD, MS, NE, NJ, NM, NV, OK, PA, TN, TX, UT, VA, WV, WY
P60	20	AR, AZ, CA, CO, DC, DE, FL, GA, LA, MA, MD, NE, NJ, NM, NV, OK, TN, TX, UT, VA
P70	15	AR, AZ, CO, DC, DE, FL, GA, LA, MA, NJ, NM, OK, TX, UT, VA
Control	26–36	All remaining states

Notes: Treatment groups are defined by the standardized surge-intensity statistic δ_i^{std} exceeding the indicated percentile of the cross-state distribution. δ_i^{std} is the cross-state z -score of the change in each state’s mean monthly community-based ICE arrest rate per capita between the pre-treatment window (February–October 2024) and the post-treatment window (February–October 2025), standardized to mean zero and unit variance across all 51 units (50 states + DC). The control group at each threshold consists of all remaining units; N_{co} ranges from 26 (P50) to 36 (P70). P60 is the primary specification ($N_{tr} = 20$, $N_{co} = 31$).

Table 2: Synthetic DiD Estimates of ICE Enforcement Effects on All Consumer Spending (Affinity)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	P50	P60	P70	P60, covs	Drop VA/MD/DC	Drop outliers	Pre- tariffs	Non-sym. window	Jack- knife SE	Jail- based	Pla- cebo
ATT (pp)	-1.236	-1.730**	-1.747**	-1.891**	-1.810**	-1.424**	-1.224**	-2.117**	-1.730**	-0.851	0.027
Std. error	(0.889)	(0.757)	(0.810)	(0.837)	(0.826)	(0.675)	(0.598)	(0.856)	(0.739)	(0.825)	(0.232)
<i>p</i> -value	[0.164]	[0.022]	[0.031]	[0.024]	[0.028]	[0.035]	[0.040]	[0.013]	[0.019]	[0.302]	[0.906]
<i>N</i> treated	25	20	15	20	17	17	20	20	20	20	20
<i>N</i> control	26	31	36	31	31	31	31	31	31	31	31

Notes: Synthetic Difference-in-Differences estimates (Arkhangelsky et al., 2021) of the effect of the 2025 ICE enforcement surge on seasonally adjusted consumer spending (Affinity Solutions / Opportunity Insights), measured as the percentage-point change relative to January 2022 baseline. Unit of observation is state-month; outcome is `spend_s_all_sa22`. Treatment is a binary indicator equal to one for states whose standardized surge intensity δ_{std} (cross-state *z*-score of the change in community-based ICE arrest rate per capita between Feb–Oct 2024 and Feb–Oct 2025) exceeds a given percentile of the pooled distribution. Time-varying treatment: $W_{it} = D_i \cdot \mathbf{1}[t \geq \text{Jan 2025}]$. Baseline sample is 51 units (50 states + DC); estimation window Jan 2022–Oct 2025. Treated state composition for cols (1)–(3) is reported in Table 1.

Column (1): P50 threshold. Column (2) P60; column (3) P70. Column (4) P60 with covariates (median income, foreign-born share, and percent metro), **optimized** method. Column (5) drops VA, MD, and DC (federal-worker-heavy; all originally P60-treated) to address DOGE-related spending confounders; P60 cutoff preserved from the full 51-unit distribution. Column (6) drops NM, TX, and DC—the three states in the upper tail of the δ_{std} distribution (see Appendix Figure A.2; all three are originally P60-treated)—to assess whether the result is driven by right-tail outliers; P60 cutoff and δ_{std} preserved from the full 51-unit distribution. Column (7) truncates the post-period to Jan–Mar 2025 to isolate the enforcement effect from the April 2025 tariff shock. Column (8) uses a non-symmetric shock window (pre: Dec 2023–Dec 2024; post: Jan 2025–Feb 2026) with estimation extended through Feb 2026. Column (9) reports jackknife standard errors in place of the wild cluster bootstrap. Column (10) redefines treatment using jail-based rather than community-based ICE arrest rates; a null result supports community-based enforcement (workplace/home raids generating labor-supply withdrawal) as the operative mechanism. Column (11) is a placebo-in-time test targeting pre-trend contamination of the δ_{std} surge window: the real P60 treated set is retained, but treatment is assigned a fake onset date of July 2024 and the estimation window runs Jan 2022–Oct 2024 (ending pre-election).

Standard errors in parentheses are from the clustered bootstrap with 500 replications (Algorithm 2 in Arkhangelsky et al., 2021), except column (9) which uses jackknife. *p*-values in brackets are from a two-sided test. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Synthetic DiD Estimates of ICE Enforcement Effects on Consumer Spending by Sector and Surge-Intensity Threshold

	(1)	(2)	(3)	(4)
	Community-based			Jail-based
	P50	P60	P70	P60
All Spending	-1.24 (0.89)	-1.73** (0.75)	-1.75** (0.82)	-0.85 (0.87)
Accommodation & Food Serv. (ACF)	-1.66** (0.81)	-1.77** (0.76)	-1.69** (0.75)	-0.21 (0.73)
Apparel & Accessories (AAP)	-1.35 (1.39)	-1.65 (1.38)	-1.80 (1.73)	0.69 (1.54)
Arts, Entertainment & Rec. (AER)	-1.10 (2.36)	-3.30* (2.00)	-1.59 (2.03)	-3.55* (1.94)
Durable Goods	-2.55** (1.13)	-2.66*** (0.98)	-2.89*** (1.06)	-1.06 (1.00)
Gen. Merchandise & Apparel (APG)	-3.60* (2.05)	-3.54* (1.93)	-2.88* (1.68)	-0.79 (2.32)
General Merchandise Stores (GEN)	-4.89* (2.70)	-4.03* (2.26)	-4.28** (1.98)	-1.90 (3.40)
Grocery & Food (GRF)	-0.22 (1.40)	-1.02 (1.34)	-1.04 (1.24)	0.10 (1.05)
Health Care & Social Assist. (HCS)	-0.27 (1.41)	-1.95* (1.08)	-1.28 (1.23)	-0.62 (1.58)
Home Improvement Centers (HIC)	-5.63* (2.91)	-5.92** (2.71)	-6.13** (2.94)	-2.08 (2.92)
In-Person Services	-0.96 (0.84)	-2.44*** (0.70)	-2.12*** (0.68)	-0.60 (0.88)
Non-Durable Goods	-1.25 (1.13)	-1.26 (0.90)	-1.54 (0.98)	0.50 (0.87)
Sporting Goods & Hobby (SGH)	-3.77* (2.21)	-3.59* (2.11)	-3.35 (2.22)	1.01 (1.79)
Transportation & Warehousing (TWS)	-3.14* (1.64)	-3.31** (1.40)	-2.89* (1.58)	-0.86 (1.61)
Non In-Person Services	0.23 (0.82)	0.70 (0.64)	0.45 (0.64)	-0.38 (0.97)
<i>N</i> treated	25	20	15	20

Notes: Synthetic Difference-in-Differences estimates (Arkhangelsky et al., 2021) of the effect of the 2025 ICE enforcement surge on seasonally adjusted consumer spending, by sector (rows) and surge-intensity threshold (columns). Each cell reports the ATT in percentage points, with the clustered bootstrap standard error (500 replications, Algorithm 2 in Arkhangelsky et al., 2021) in parentheses immediately below. Unit of observation is state-month; outcomes are Affinity Solutions / Opportunity Insights card-spending indices (`spend.s.*.sa22`) indexed to Jan 2022. Estimation window Jan 2022–Oct 2025. Sample is 51 units (50 states + DC).

Columns (1)–(3) define treatment using community-based ICE arrests: a binary indicator equal to one for states whose standardized surge intensity δ_i^{std} (cross-state z -score of the change in community-based arrest rate per capita between Feb–Oct 2024 and Feb–Oct 2025) exceeds the 50th, 60th, or 70th percentile of the pooled 51-unit distribution, respectively. Column (4) redefines treatment using jail-based ICE arrests (an analogous standardized cross-state surge measure), with the threshold set at the 60th percentile of the jail-based distribution.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Synthetic DiD Estimates of ICE Enforcement Effects on Consumer Spending by Sector and Political Subgroup (P60)

	(1)	(2)	(3)	(4)	(5)
	Full	2024 Pres. vote		Governor (Jan 2025)	
	sample	Harris	Trump	Dem.	Rep.
All Spending	-1.73** (0.80)	-4.41** (1.84)	-0.55 (0.43)	-4.09*** (1.51)	-0.61 (0.46)
Accommodation & Food Serv. (ACF)	-1.77** (0.76)	-3.15** (1.27)	-1.35* (0.77)	-2.31* (1.28)	-1.48* (0.87)
Apparel & Accessories (AAP)	-1.65 (1.31)	-2.82 (2.48)	-1.62 (1.88)	-2.66 (2.08)	-1.48 (1.92)
Gen. Merchandise & Apparel (APG)	-3.54* (1.96)	-6.23 (4.38)	-1.87 (2.04)	-5.20 (3.23)	-2.54 (2.37)
Durable Goods	-2.66*** (0.98)	-3.09 (2.07)	-1.86** (0.73)	-3.80** (1.74)	-1.72* (0.98)
General Merchandise Stores (GEN)	-4.03* (2.25)	-7.90* (4.59)	-2.12 (2.65)	-6.72* (3.72)	-3.35 (3.21)
Grocery & Food (GRF)	-1.02 (1.32)	-3.43 (3.01)	0.74 (0.91)	-2.59 (2.53)	0.02 (0.96)
Health Care & Social Assist. (HCS)	-1.95* (1.11)	-3.75 (2.32)	-1.10 (1.24)	-2.61 (1.66)	-1.13 (1.57)
Home Improvement Centers (HIC)	-5.92** (2.92)	-1.11 (7.03)	-3.76* (2.09)	-4.02 (4.30)	-3.47 (2.36)
In-Person Services	-2.44*** (0.66)	-3.00*** (1.01)	-2.23** (0.88)	-2.42*** (0.87)	-2.40** (0.97)
Non-Durable Goods	-1.26 (0.90)	-4.07 (2.66)	-0.31 (0.91)	-3.40 (2.10)	-0.56 (0.72)
Sporting Goods & Hobby (SGH)	-3.59* (2.12)	-5.54 (4.25)	-2.53 (2.84)	-5.79* (3.45)	-3.38 (3.31)
Transportation & Warehousing (TWS)	-3.31** (1.42)	-3.33 (2.82)	-3.03*** (1.06)	-3.77 (2.74)	-4.29*** (1.00)
Arts, Entertainment & Rec. (AER)	-3.30* (2.00)	-7.20** (2.98)	-3.02 (2.16)	-7.31** (2.84)	-1.79 (2.49)
N states	51	19	32	24	27
N treated	20	9	11	9	11

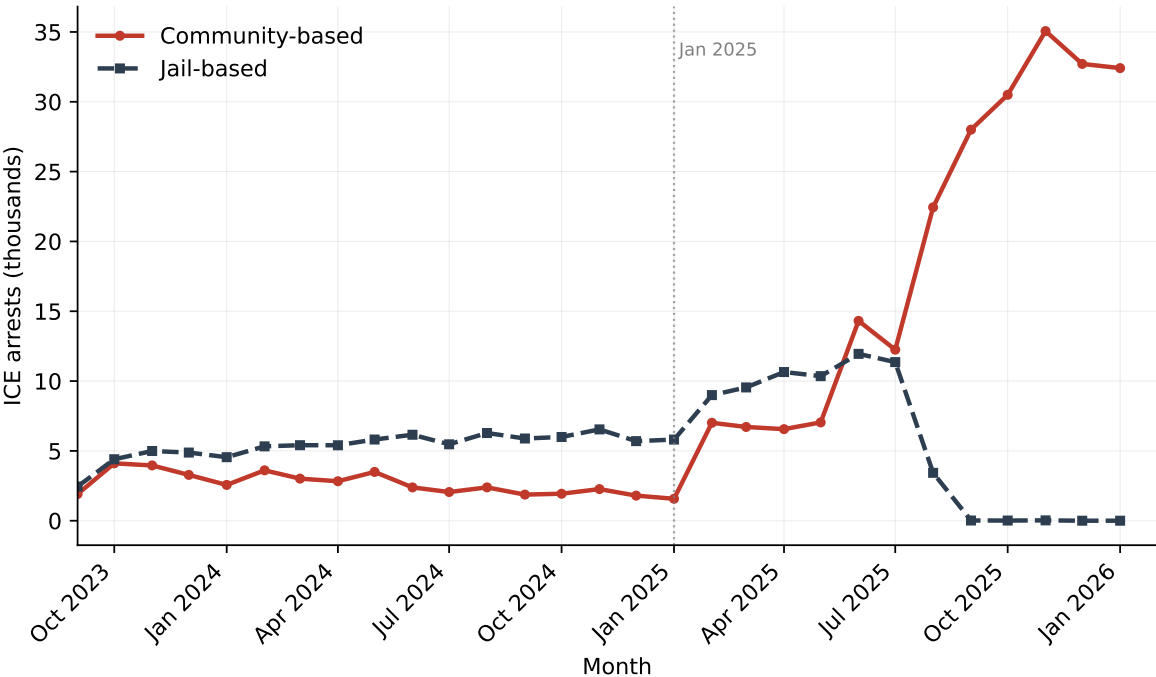
Notes: Synthetic Difference-in-Differences estimates (Arkhangelsky et al., 2021) of the effect of the 2025 ICE enforcement surge on seasonally adjusted consumer spending, by sector (rows) and political subgroup (columns). Each cell reports the ATT in percentage points, with the clustered bootstrap standard error (500 replications, Algorithm 2 in Arkhangelsky et al., 2021) in parentheses immediately below. Unit of observation is state-month; outcomes are Affinity Solutions / Opportunity Insights card-spending indices (`spend_s.*_sa22`) indexed to Jan 2022. Estimation window Jan 2022–Oct 2025.

Treatment: a binary indicator equal to one for states whose standardized surge intensity δ_i^{std} (cross-state z -score of the change in community-based ICE arrest rate per capita between Feb–Oct 2024 and Feb–Oct 2025) exceeds the 60th percentile of the *full* 51-unit distribution (20 treated states). The threshold is fixed and is not re-estimated within subsamples. Column (1) results for full sample.

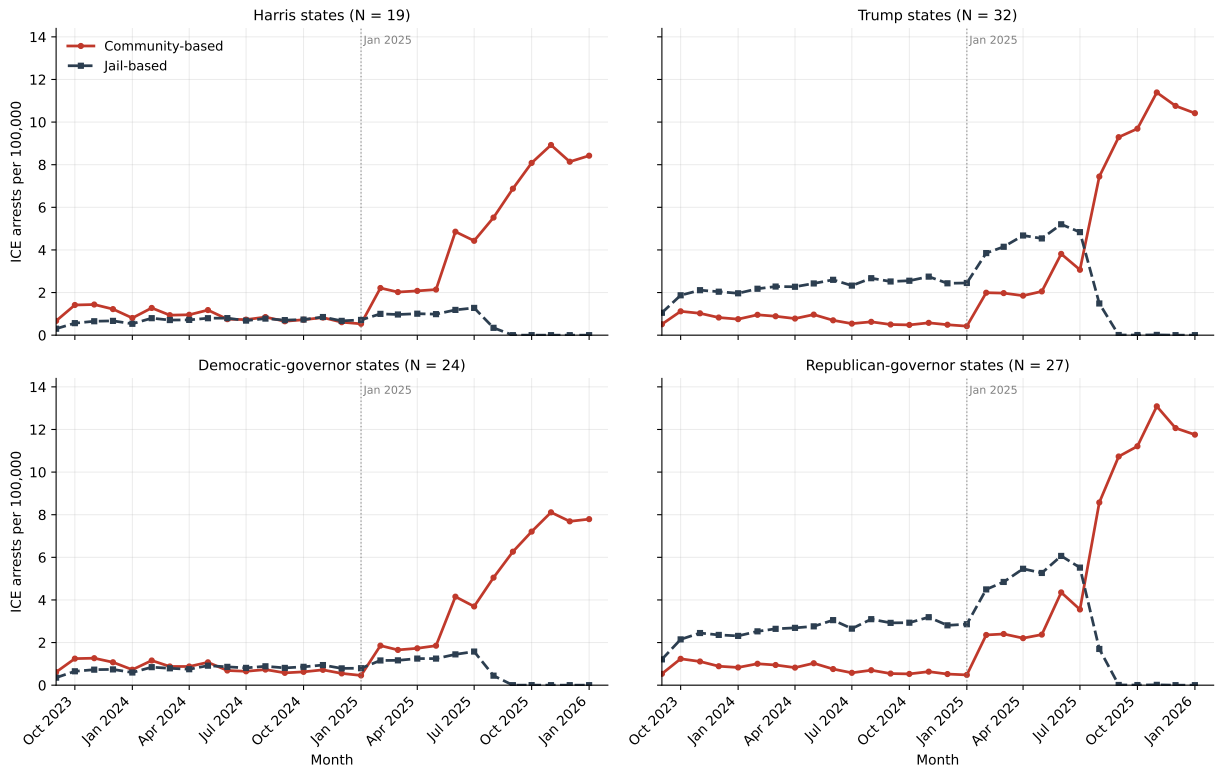
Columns (2)–(3) restrict to states carried by Harris vs. Trump in the 2024 presidential election. Columns (4)–(5) restrict to states with Democratic vs. Republican governors as of January 2025 (after all January 2025 inaugurations). **Harris** (N=19): CA, CO, CT, DC, DE, HI, IL, MA, MD, ME, MN, NH, NJ, NM, NY, OR, RI, VA, WA. **Trump** (N=32): AK, AL, AR, AZ, FL, GA, IA, ID, IN, KS, KY, LA, MI, MO, MS, MT, NC, ND, NE, NV, OH, OK, PA, SC, SD, TN, TX, UT, VT, WI, WV, WY. **Democratic governor** (N=24): AZ, CA, CO, CT, DC, DE, HI, IL, KS, KY, MA, MD, ME, MI, MN, NC, NJ, NM, NY, OR, PA, RI, VA, WI. **Republican governor** (N=27): AK, AL, AR, FL, GA, IA, ID, IN, LA, MO, MS, MT, ND, NE, NH, NV, OH, OK, SC, SD, TN, TX, UT, VA, VT, WV, WY. DC has a mayor (Bowser, D) rather than a governor and is assigned to the Democratic governor column. Sources: Associated Press, 2024 presidential election results, <https://apnews.com/projects/election-results-2024/?office=P>; Ballotpedia, Partisan composition of governors, https://ballotpedia.org/Partisan_composition_of_governors.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

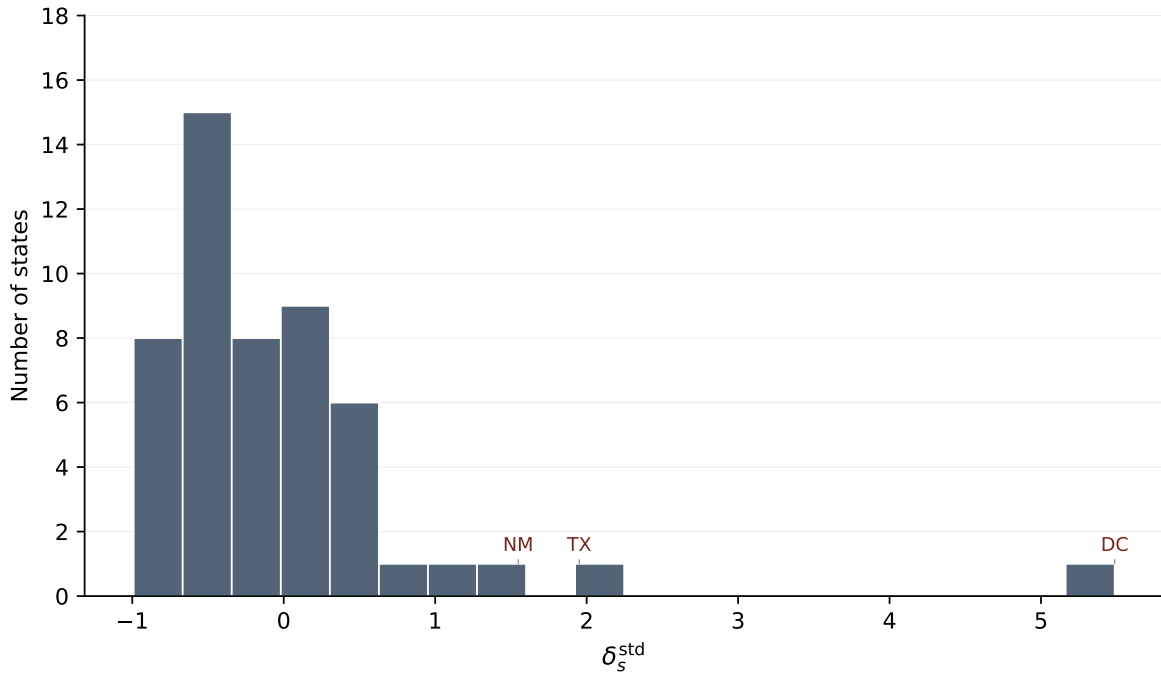
Appendix: Supplementary Figures and Tables



Appendix Figure A.1. Monthly ICE arrests by enforcement type, September 2023–January 2026. Series are summed across all 50 states and the District of Columbia. The vertical dotted line marks January 2025. Community-based arrests rise sharply beginning in early 2025 and account for the post-January 2025 increase in total arrests, while jail-based arrests decline through the second half of 2025. Source: Deportation Data Project.



Appendix Figure A.2. Monthly ICE arrests per 100,000 population by enforcement type and political subgroup, September 2023–January 2026. Top row splits states by 2024 presidential vote (statewide winner); bottom row splits states by governor party as of January 2025 (after January 2025 inaugurations). Within each panel, series are computed as the sum of arrests in the subgroup divided by the sum of population in the subgroup, multiplied by 100,000. The vertical dotted line marks January 2025. The post-January 2025 community-based surge appears in all four subgroups, but is larger in absolute and per-capita terms in Trump and Republican-governor states; the pre-2025 jail-based rate is also higher in those states. The District of Columbia is assigned to the Democratic-governor column. Source: Deportation Data Project; population from the merged state-month panel.



Appendix Figure A.3. Distribution of the standardized state-level enforcement surge Δ_s^{std} across all 50 states and the District of Columbia. For each state, Δ_s is the change in mean monthly community-based arrests per capita between the pre-period (September 2023–December 2024) and the post-period (January 2025–February 2026); Δ_s^{std} standardizes this measure to mean zero and unit standard deviation across states. The distribution is right-skewed (skewness = 2.42), with the District of Columbia, Texas, and New Mexico in the upper tail.

Table A.1: Cross-State Distribution of Standardized Surge Intensity (Δ_i^{std})

State	Δ_i	Δ_i^{std}	State	Δ_i	Δ_i^{std}
AK	0.55	-0.879	MT	0.21	-0.991
AL	3.08	-0.044	NC	1.31	-0.627
AR	4.24	0.337	ND	1.36	-0.612
AZ	4.42	0.397	NE	3.69	0.154
CA	3.80	0.191	NH	1.63	-0.523
CO	4.00	0.257	NJ	3.94	0.238
CT	1.53	-0.556	NM	7.91	1.548
DC	19.86	5.487	NV	3.51	0.096
DE	4.01	0.260	NY	2.13	-0.359
FL	4.65	0.473	OH	1.10	-0.698
GA	4.82	0.529	OK	4.84	0.535
HI	0.80	-0.798	OR	1.18	-0.670
IA	1.82	-0.461	PA	3.13	-0.030
ID	1.52	-0.560	RI	2.67	-0.181
IL	2.15	-0.351	SC	2.31	-0.299
IN	1.82	-0.460	SD	1.29	-0.636
KS	2.51	-0.233	TN	3.80	0.193
KY	1.95	-0.417	TX	9.14	1.951
LA	4.60	0.457	UT	3.96	0.244
MA	6.81	1.183	VA	6.03	0.928
MD	3.23	0.003	VT	0.44	-0.916
ME	0.84	-0.785	WA	1.65	-0.517
MI	1.53	-0.556	WI	0.82	-0.790
MN	1.52	-0.559	WV	2.72	-0.165
MO	1.41	-0.595	WY	3.15	-0.021
MS	2.69	-0.172			

Notes: This table reports the cross-state variation in surge intensity used to define treatment in Tables 1 and 2. For each of the 51 units (50 states + DC), Δ_i is the change in the mean monthly community-based ICE arrest rate per capita between the pre-period (Feb–Oct 2024) and post-period (Feb–Oct 2025), expressed as arrests per 100,000 population per month. Δ_i^{std} is the cross-state z -score of Δ_i (mean zero, unit variance across the 51 units). Treatment in the SDiD specifications is a binary indicator equal to one for states whose Δ_i^{std} exceeds a given percentile of the pooled distribution.

P50 threshold ($\Delta_i^{\text{std}} > -0.181$; 25 treated, 26 control): AL, AR, AZ, CA, CO, DC, DE, FL, GA, LA, MA, MD, MS, NE, NJ, NM, NV, OK, PA, TN, TX, UT, VA, WV, WY.

P60 threshold ($\Delta_i^{\text{std}} > -0.021$; 20 treated, 31 control): AR, AZ, CA, CO, DC, DE, FL, GA, LA, MA, MD, NE, NJ, NM, NV, OK, TN, TX, UT, VA.

P70 threshold ($\Delta_i^{\text{std}} > 0.193$; 15 treated, 36 control): AR, AZ, CO, DC, DE, FL, GA, LA, MA, NJ, NM, OK, TX, UT, VA.

Source: Deportation Data Project (community-based ICE arrests; Sep 2023–Feb 2026 panel). Population denominator is `total_pop_smooth`.

Table A.2: Continuous Difference-in-Differences: Alternative Specifications

	(1)	(2)	(3)	(4)
	Aslim et al. (2026)		East et al. (2023)	
$\hat{\beta}$ (pp)	-0.574 (0.660)	-0.579 (0.659)	-0.604 (0.475)	-0.596 (0.472)
State FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Covariates	No	Yes	No	Yes
N	2,346 (51 states \times 46 months, Jan 2022–Oct 2025)			

Notes: Continuous difference-in-differences estimates of the effect of the 2025 ICE community-based enforcement surge on All Spending (`spend_s_all_sa22`). Outcome is the seasonally adjusted card-spending index, expressed as a percentage-point deviation from the January 2022 baseline; coefficients are reported in percentage points. Estimation by OLS with state and time fixed effects. Standard errors clustered by state in parentheses.

Aslim et al. (2026) specification (cols. 1–2). $y_{it} = \alpha_i + \gamma_t + \beta (\Delta_i^{\text{std}} \cdot \mathbf{1}[t \geq \text{Jan 2025}]) + X'_{it}\gamma + \varepsilon_{it}$, where Δ_i^{std} is the cross-state z -score of the change in the mean monthly community-based ICE arrest rate per capita between the pre-period (Feb–Oct 2024) and the post-period (Feb–Oct 2025). β is interpreted as the percentage-point change in spending associated with a one-standard-deviation increase in surge intensity, after Trump’s inauguration.

East et al. (2023) specification (cols. 3–4). $y_{it} = \alpha_i + \gamma_t + \beta A_{it} + X'_{it}\gamma + \varepsilon_{it}$, where A_{it} is the time-varying community-based ICE arrest rate, in arrests per 10,000 population. β is interpreted as the percentage-point change in spending per additional arrest per 10,000 population per month.

Covariates. Time-varying median household income, foreign-born share of the population, and metropolitan population share.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.3: Pre-Treatment Descriptive Statistics by Treatment Group (P60 Specification)

	Treated ($N = 20$ states)	Control ($N = 31$ states)
<i>Consumer spending indices (pp deviation from Jan 2022 baseline, SA)</i>		
All Spending	0.0244 (0.0257)	0.0269 (0.0305)
Accommodation & Food Services	0.0094 (0.0241)	0.0160 (0.0323)
Apparel & Accessories (AAP)	-0.0278 (0.0542)	-0.0280 (0.0539)
Arts, Entertainment & Rec. (AER)	0.0751 (0.0831)	0.1076 (0.1639)
General Merch. & Apparel (APG)	0.0358 (0.0635)	0.0457 (0.0686)
Durable Goods	-0.0339 (0.0304)	-0.0241 (0.0349)
General Merchandise	0.0766 (0.0914)	0.0910 (0.1042)
Grocery & Food Retail	0.0147 (0.0241)	0.0136 (0.0242)
Health Care & Social Assist.	0.0615 (0.0632)	0.0611 (0.0870)
Home Improvement Centers	-0.0582 (0.0649)	-0.0441 (0.0829)
In-Person Services	0.0315 (0.0296)	0.0377 (0.0461)
Non-Durable Goods	0.0066 (0.0274)	0.0037 (0.0284)
Sporting Goods & Hobby (SGH)	-0.0164 (0.0543)	-0.0152 (0.0766)
Transport & Warehousing (TWS)	0.0396 (0.0581)	0.0544 (0.0725)
<i>Enforcement intensity</i>		
Arrest rate (per 10,000 pop.)	0.0884 (0.0572)	0.0474 (0.0551)
State-month observations	320	496

Notes: Means and standard deviations (in parentheses) of state-month observations during the pre-treatment period (September 2023–December 2024). Treatment is defined by the P60 threshold of the standardized cross-state surge in community-based ICE arrest rates per capita (see Table A.1). Spending variables (`spend_s*_sa22`) are seasonally adjusted Affinity Solutions / Opportunity Insights card-spending indices, expressed as percentage-point deviation from the January 2022 baseline. The arrest rate is the monthly community-based ICE arrest count per 10,000 state population.

Table A.4: Leave-One-Out Robustness: All Spending, P60 Specification

Drop	ATT (pp)	SE	p	Drop	ATT (pp)	SE	p
AK	-1.750**	(0.722)	[0.015]	MT	-1.820**	(0.792)	[0.022]
AL	-1.777**	(0.818)	[0.030]	NC	-1.827**	(0.805)	[0.023]
AR	-1.550**	(0.718)	[0.031]	ND	-1.741**	(0.758)	[0.022]
AZ	-1.501*	(0.795)	[0.059]	NE	-1.642**	(0.825)	[0.047]
CA	-1.545**	(0.758)	[0.042]	NH	-1.764**	(0.745)	[0.018]
CO	-1.501*	(0.802)	[0.061]	NJ	-1.553*	(0.800)	[0.052]
CT	-1.730**	(0.778)	[0.026]	NM	-1.253*	(0.706)	[0.076]
DC	-1.584**	(0.805)	[0.049]	NV	-1.593**	(0.805)	[0.048]
DE	-1.503*	(0.787)	[0.056]	NY	-1.761**	(0.697)	[0.012]
FL	-1.541**	(0.761)	[0.043]	OH	-1.786**	(0.760)	[0.019]
GA	-1.660**	(0.801)	[0.038]	OK	-1.598**	(0.764)	[0.036]
HI	-1.387**	(0.540)	[0.010]	OR	-1.749**	(0.724)	[0.016]
IA	-1.741**	(0.788)	[0.027]	PA	-1.792**	(0.795)	[0.024]
ID	-1.970***	(0.759)	[0.009]	RI	-2.009***	(0.772)	[0.009]
IL	-1.698**	(0.740)	[0.022]	SC	-1.827**	(0.788)	[0.020]
IN	-1.738**	(0.826)	[0.035]	SD	-1.784**	(0.800)	[0.026]
KS	-1.656**	(0.788)	[0.036]	TN	-1.627**	(0.800)	[0.042]
KY	-1.840**	(0.753)	[0.015]	TX	-1.585**	(0.761)	[0.037]
LA	-1.640**	(0.778)	[0.035]	UT	-1.566**	(0.777)	[0.044]
MA	-1.585**	(0.763)	[0.038]	VA	-1.586**	(0.751)	[0.035]
MD	-1.582*	(0.843)	[0.060]	VT	-1.701**	(0.776)	[0.028]
ME	-1.563**	(0.759)	[0.039]	WA	-1.860**	(0.774)	[0.016]
MI	-1.710**	(0.781)	[0.028]	WI	-1.710**	(0.761)	[0.025]
MN	-1.783**	(0.768)	[0.020]	WV	-1.710**	(0.774)	[0.027]
MO	-1.639**	(0.780)	[0.036]	WY	-1.695**	(0.767)	[0.027]
MS	-1.561*	(0.851)	[0.067]				

Notes: Each row reports the SDiD ATT, bootstrap standard error (in parentheses), and two-sided p -value (in brackets) from a separate estimation in which the indicated state is dropped from the panel. The standardized surge intensity Δ_i^{std} and the P60 cutoff are re-estimated on the remaining 50 units in each iteration; treatment is then re-assigned. Outcome is All Spending (`spend_s_all_sa22`); estimation window Jan 2022–Oct 2025; $W_{it} = D_i \cdot \mathbf{1}[t \geq \text{Jan 2025}]$. Bootstrap standard errors use 500 replications.

Across all 51 iterations, $\hat{\tau}$ ranges from -1.253 to -2.009 pp (mean -1.672); 44/51 are significant at the 5% level and 51/51 at the 10% level. The reference estimate from Table 1, column (2) is -1.730 pp.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.5: Predictors of state-level enforcement surge intensity

	δ_s^{std}
Median household income (\$1000s)	−0.001 (0.019)
Foreign-born share	0.812 (3.821)
Metro population share	1.991* (1.106)
Democratic governor	−0.374 (0.240)
Harris 2024	0.214 (0.465)
Constant	−1.439 (2.331)
States	51
R^2	0.139
F	2.57

Notes: OLS regression of the standardized cross-state enforcement surge δ_s^{std} on state-level pre-period covariates, $N = 51$. δ_s is the change in mean monthly community-based ICE arrests per capita between September 2023–December 2024 and January 2025–February 2026; δ_s^{std} is the cross-state z-score. Median household income, foreign-born share, and metro population share are state-level means over January 2022–December 2024. Democratic governor is an indicator for governor party as of January 2025; Harris 2024 is an indicator for the 2024 presidential vote. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.6: Pre-Trend Diagnostics for the SDiD Estimator: Placebo-in-Time and Pre-Period Fit Across Outcomes

Outcome	(1)	(2)	(3)	(4)
	Placebo-in-time ATT (pp)	Actual	Placebo P50	Rank p
All Spending	+0.03 (0.24)	0.24	1.16	1.00
Accommodation & Food Serv. (ACF)	+0.03 (0.32)	0.91	1.39	0.91
Apparel & Accessories (AAP)	+0.27 (0.73)	1.01	2.23	0.94
Arts, Entertainment & Rec. (AER)	-1.78* (0.94)	0.13	4.28	1.00
Gen. Merchandise & Apparel (APG)	+0.34 (0.56)	0.37	1.90	1.00
Durable Goods	+0.06 (0.35)	0.63	1.71	1.00
General Merchandise Stores (GEN)	+0.48 (0.61)	0.87	2.46	1.00
Grocery & Food (GRF)	+0.08 (0.24)	0.35	1.09	1.00
Health Care & Social Assist. (HCS)	-0.56 (0.53)	0.23	2.37	1.00
Home Improvement Centers (HIC)	-0.44 (0.97)	1.53	3.63	0.97
In-Person Services	-0.26 (0.33)	0.43	1.57	1.00
Non-Durable Goods	+0.77*** (0.27)	0.20	1.10	1.00
Sporting Goods & Hobby (SGH)	-2.16** (0.96)	1.82	3.44	1.00
Transportation & Warehousing (TWS)	-0.60 (0.59)	2.21	2.90	0.75

Notes: Two diagnostics for differential pre-trends. Both use the P60 specification (treatment $D_i = 1[\delta_i^{\text{std}} > P_{60}]$, computed from the 51-unit cross-state distribution of the change in community-based ICE arrest rates per capita between Feb–Oct 2024 and Feb–Oct 2025).

Column (1) reports a placebo-in-time SDiD: the actual P60 treated set (20 states) is retained, treatment is assigned a fake onset of July 2024, and the estimation window runs Jan 2022–Oct 2024 (ending pre-election). A nonzero estimate suggests SDiD weights generate spurious post-period gaps from pre-trend divergence rather than from real treatment. Standard errors (in parentheses) are from the clustered bootstrap with 500 replications. The All Spending row replicates Table 2 column 11.

Columns (2)–(4) report a pre-period fit diagnostic following Abadie, Diamond, and Hainmueller (2010) adapted to SDiD. “Actual” is the SDiD pre-period RMSE between the treated mean and the SDiD-reweighted control mean. “Placebo P50” is the median pre-period RMSE from a placebo distribution: each of the 31 P60 control states is iteratively assigned as a singleton placebo-treated unit, with SDiD re-fit on the remaining 30 control states (the 20 actually-treated states are excluded entirely from this exercise), yielding a 31-element reference distribution. “Rank p ” is the share of placebo RMSEs that meet or exceed the actual RMSE. Values > 0.10 indicate the actual pre-fit is not in the upper tail of the placebo distribution.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.7: Synthetic DiD Estimates of ICE Enforcement Effects on Consumer Spending: All Sectors Across Specifications

	(1) P60	(2) P60, covs	(3) Drop VA/MD/DC	(4) Drop outliers	(5) Pre- tariffs	(6) Non-sym. window	(7) Jack- knife SE	(8) Pla- cebo
All Spending	-1.73** (0.80)	-1.89** (0.84)	-1.81** (0.83)	-1.42** (0.68)	-1.22** (0.60)	-2.12** (0.86)	-1.73** (0.74)	0.03 (0.23)
Accommodation & Food Serv. (ACF)	-1.77** (0.75)	-1.75** (0.72)	-1.72** (0.79)	-1.94** (0.78)	-0.97 (0.63)	-1.91** (0.76)	-1.77*** (0.62)	0.03 (0.33)
Apparel & Accessories (AAP)	-1.65 (1.36)	-1.36 (1.44)	-1.76 (1.42)	-1.27 (1.37)	-1.30 (1.27)	-2.08 (1.35)	-1.65 (1.43)	0.27 (0.74)
Arts, Entertainment & Rec. (AER)	-3.30* (1.87)	-3.12 (2.05)	-3.00 (1.89)	-3.49* (1.91)	-2.60 (1.85)	-2.54 (2.15)	-3.30* (1.83)	-1.78* (0.99)
Durable Goods	-2.66*** (1.01)	-2.66** (1.08)	-3.03*** (1.05)	-2.70*** (1.03)	-2.03*** (0.77)	-3.03*** (1.09)	-2.66*** (0.99)	0.06 (0.35)
Gen. Merchandise & Apparel (APG)	-3.54* (1.90)	-3.49* (2.10)	-3.18* (1.87)	-3.11* (1.80)	-3.78** (1.52)	-3.54* (1.97)	-3.54** (1.79)	0.34 (0.56)
General Merchandise Stores (GEN)	-4.03* (2.30)	-2.60 (2.73)	-3.82 (2.33)	-3.31 (2.24)	-4.10** (1.83)	-3.73 (2.48)	-4.03 (2.47)	0.48 (0.64)
Grocery & Food (GRF)	-1.02 (1.28)	-0.87 (1.25)	-1.28 (1.30)	-0.76 (1.20)	-0.02 (0.91)	-1.29 (1.42)	-1.02 (1.28)	0.08 (0.25)
Health Care & Social Assist. (HCS)	-1.95* (1.10)	-2.15* (1.24)	-2.23* (1.20)	-1.82* (1.07)	-0.54 (1.06)	-2.49** (1.26)	-1.95 (1.20)	-0.56 (0.51)
Home Improvement Centers (HIC)	-5.92** (2.70)	-7.05** (3.06)	-7.33*** (2.84)	-6.35** (2.84)	-2.72* (1.45)	-5.57** (2.74)	-5.92** (2.88)	-0.44 (1.01)
In-Person Services	-2.44*** (0.68)	-2.62*** (0.69)	-2.54*** (0.74)	-2.40*** (0.68)	-1.81*** (0.59)	-2.44*** (0.73)	-2.44*** (0.62)	-0.26 (0.33)
Non-Durable Goods	-1.26 (0.95)	-1.14 (0.92)	-1.32 (0.99)	-0.92 (0.79)	-0.95 (0.69)	-1.55 (1.06)	-1.26 (1.14)	0.77*** (0.27)
Sporting Goods & Hobby (SGH)	-3.59* (2.11)	-3.71* (2.22)	-3.78* (2.24)	-3.15 (2.21)	-3.46* (2.03)	-3.00 (2.31)	-3.59** (1.64)	-2.16** (0.99)
Transportation & Warehousing (TWS)	-3.31** (1.43)	-3.53** (1.54)	-3.35** (1.50)	-2.80** (1.30)	-2.21* (1.23)	-3.74** (1.60)	-3.31** (1.32)	-0.60 (0.61)
N treated	20	20	17	17	20	20	20	20
N control	31	31	31	31	31	31	31	31

Notes: Synthetic DiD estimates (Arkhangelsky et al., 2021) of the 2025 ICE enforcement surge on seasonally adjusted consumer spending (Affinity Solutions / Opportunity Insights), measured as percentage-point change relative to January 2022 baseline. Unit of observation: state-month. Treatment: $D_i = \mathbf{1}[\Delta_{\text{std},i} > P_{60}]$, where Δ_{std} is the cross-state z -score of the change in community-based ICE arrest rate per capita between Feb–Oct 2024 and Feb–Oct 2025; $W_{it} = D_i \cdot \mathbf{1}[t \geq \text{Jan 2025}]$. Estimation window: Jan 2022–Oct 2025; 51 units (50 states + DC).

Columns: (1) Baseline P60. (2) P60 + covariates (median income, foreign-born share, % metro). (3) Drops VA, MD, DC (federal-worker-heavy; all P60-treated); P60 cutoff from full distribution. (4) Drops NM, TX, DC (upper-tail outliers in Δ_{std} ; see Fig. A.3); P60 cutoff preserved. (5) Post-period truncated to Jan–Mar 2025 (pre-tariff). (6) Non-symmetric window (pre: Dec 2023–Dec 2024; post: Jan 2025–Feb 2026). (7) Jackknife SEs. (8) Placebo-in-time: real P60 treatment set, fake onset July 2024, window Jan 2022–Oct 2024.

Standard errors (parentheses) from clustered bootstrap, 500 replications (Algorithm 2, Arkhangelsky et al., 2021), except col. (7) jackknife. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.8: Subgroup Heterogeneity: All Spending, P60 Specification

	ATT (pp)
<i>A. Median household income</i>	
Above median	-2.34* (1.36) [$N_{tr} = 12$]
Below median	-1.77 (1.09) [$N_{tr} = 8$]
<i>B. Foreign-born share</i>	
Above median	-2.09 (1.69) [$N_{tr} = 16$]
Below median	-0.66 (0.78) [$N_{tr} = 4$]
<i>C. Metro share</i>	
Above median	-2.03 (1.49) [$N_{tr} = 15$]
Below median	-2.35 (1.53) [$N_{tr} = 5$]
<i>D. 2024 presidential vote</i>	
Harris states	-4.41** (1.74) [$N_{tr} = 9$]
Trump states	-0.55 (0.47) [$N_{tr} = 11$]
<i>E. Governor party (Jan 2025)</i>	
Democratic	-4.09** (1.65) [$N_{tr} = 9$]
Republican	-0.61 (0.48) [$N_{tr} = 11$]

Notes: SDiD estimates of the effect of the 2025 ICE community-based enforcement surge on All Spending (`spend_s_all_sa22`) within subsamples defined by state characteristics. ATT in percentage points; bootstrap standard errors (500 replications, clustered by state) in parentheses; N_{tr} in brackets is the number of P60 treated states in each subsample. The P60 cutoff is fixed from the full 51-state Δ^{std} distribution. Continuous variables (panels A–C) are split at the cross-state median of pre-period state-level averages (Jan 2022–Dec 2024); both treated and control states are restricted by the same condition.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.