The Social Costs of ICE Enforcement*

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Abstract: This paper provides the first causal estimates of the community-wide costs of U.S. immigration enforcement. We combine novel geocoded data on ICE enforcement activity in high-enforcement areas across the United States from January 2025 to January 2026 with weekly establishment-level foot traffic data to measure local social and economic activity. We find that visits to locations within 0.5 miles of reported ICE enforcement decline by 1.69 percent, with no detectable rebound over the subsequent seven weeks. Both contact and non-contact operations lead to declines in visits, consistent with community-wide deterrence rather than direct confrontation. Education facilities experience the largest declines in visits, followed by worker-intensive and consumer-facing establishments, while healthcare establishments show no detectable effects. The deterrence effect is not confined to directly targeted populations: visits decline among both Hispanic-concentrated and predominantly White neighborhoods, and across industries with both high immigrant and high native workforce shares. We find no evidence of substitution effects at untreated establishments elsewhere in the county. Combined per-establishment costs across labor, education, and consumer channels total $195,669 per month—approximately 11.4 times the DHS per-removal benchmark.

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1 Introduction

Immigration enforcement in the United States entered a new era in 2025. Following the rescission of the Sensitive Locations policy, community-based arrests expanded sharply across streets, workplaces, and education facilities, making Immigration and Customs Enforcement (ICE) a visible and sustained presence in many communities (East, Patler and Cox, 2026).[1] Such enforcement strategies may impose costs that extend well beyond those directly targeted, especially given that many individuals arrested have no criminal records. When ICE agents operate near schools, healthcare facilities, shopping areas, or industrial workplaces, residents may avoid grocery stores, skip medical appointments, or keep children home from school, while workers may stay away from their jobs. Although these responses may represent a wide range of social costs, systematic evidence remains limited.

This paper examines how recently expanded ICE community-based enforcement operations affect local economic and social activity in surrounding communities. Empirically identifying these effects presents three main challenges. The first is measuring ICE enforcement: commonly used administrative records of ICE arrests are reported at the level of field offices or states, making it impossible to identify the specific communities in which enforcement actions occurred.[2] The second challenge involves developing high-frequency and timely measures of economic and social activity. The third challenge arises from the potentially strategic nature of ICE enforcement (Ciancio and García-Jimeno, 2024), as areas targeted by ICE may differ systematically from those that are not. We address these challenges by integrating novel community-reported geocoded enforcement records with high-frequency foot traffic data and exploiting hyperlocal variation in both the spatial and temporal exposure to ICE events. This approach yields, to our knowledge, the first direct causal estimates of the magnitude, spatial reach, and persistence of immigration enforcement's chilling effects under the post-2025 enforcement regime.

To measure ICE community-based enforcement, we leverage a novel dataset compiled by the Pueblo Project through its ICEout database. Beginning in January 2025, ICEout has

[1] In particular, enforcement actions during Operation Metro Surge in Minneapolis in early 2026 were associated with multiple officer-involved shootings and widespread public unrest, highlighting the scale and intensity of the new enforcement environment.

[2] For example, FOIA-based ICE arrest data from the Deportation Data Project and the Transactional Records Access Clearinghouse (TRAC) lack detailed information on the precise locations of ICE enforcement events.

collected nationwide reports of community-based enforcement activity, covering both contact operations such as arrests and raids and non-contact operations such as officer presence in community spaces. The data provide precise geographic coordinates and exact dates for each reported event, originally collected to inform the public. These community-reported ICE data help address the first challenge of measuring enforcement at sufficiently fine geographic resolution, although they may be subject to underreporting. To address the second challenge, we use weekly POI-level foot traffic data from Advan Research, covering a wide range of location types, including commercial establishments, manufacturing facilities, and schools, to measure economic and social activity in near real time at a granular level. Addressing the third challenge requires an identification strategy that isolates causal effects from the systematic selection of targeted communities. By combining geocoded enforcement records with foot traffic data, we exploit hyperlocal spatial and temporal variation in exposure. We compare POIs located very close to an enforcement event with nearby POIs slightly farther away within the same neighborhood, before and after enforcement activity, so that any targeting patterns operating at a coarser geographic scale are absorbed.

Our sample includes 10,018 ICE enforcement events in high-enforcement areas between January 2025 and January 2026. We focus primarily on California, which accounts for 51.8 percent of reports, given its relatively high reporting rate compared to official sources and lower degree of underreporting. To enhance national relevance, we supplement the analysis with major enforcement cities in Illinois, Minnesota, New York, Oregon, and Texas.[3] For each event, we construct a sample of all POIs within three miles of the event over a window spanning four weeks before and seven weeks after the event date, restricting the sample to units that are never treated or not yet treated during this period. We use a data-driven approach to determine the spatial reach of the effects and find that they are concentrated within 0.5 miles of each event and attenuate with distance. We therefore define locations within 0.5 miles of each event as treated locations and estimate an average treatment effect of a 1.69 percent decline in weekly visits, with no detectable rebound over the seven-week observation window.

The pattern is consistent with a chilling effect as the primary channel. Non-contact operations generate a 1.30 percent decline, while contact operations generate a 2.23 percent decline, indicating that community awareness of enforcement, rather than direct confrontation,

[3]Chicago (IL); Minneapolis, Bloomington, Brooklyn Park, Burnsville, and Richfield (MN); New York (NY); Hillsboro (OR); and Dallas and Houston (TX).

can depress economic and social activity. Furthermore, high-force and low-force deployments generate statistically indistinguishable declines of 1.69 and 1.74 percent, respectively, consistent with ambient threat operating independently of operational visibility. Partitioning raids by the proximity of sensitive locations within 0.1 mile, we find significant neighborhood-wide declines across all three types of sensitive locations, including educational facilities (1.90 percent), healthcare establishments (1.96 percent), and workplaces (1.71 percent), as well as for raids with no sensitive location within 0.1 mile (1.32 percent). These results suggest that deterrence is not confined to contexts immediately adjacent to sensitive facilities.

Disruption varies by location type. Education facilities experience the largest effects: visits fall 4.82 percent and unique visitors fall 1.63 percent—approximately three times the baseline in each dimension. Worker-intensive establishments, where foot traffic largely reflects labor supply, decline by 2.76 percent in visits; consumer-facing establishments decline by 1.02 percent. Healthcare facilities are the notable exception, with neither visits nor unique visitors showing a statistically significant decline, consistent with acute clinical demand being relatively inelastic to ambient enforcement risk.

To better understand the affected population, we decompose visits by the racial composition of visitors' home census block groups (CBGs). We find that both the directly targeted population and the broader community reduce activity. Visits from Hispanic-concentrated CBGs decline by 1.05 percent, consistent with withdrawal by the directly affected population, while visits from predominantly White CBGs with less direct immigration exposure fall by 2.83 percent, indicating that deterrence extends well beyond those at direct risk of deportation. The response is similarly broad across establishments with differing immigrant employment intensity. Under NAICS-based classifications, industries with higher immigrant workforce shares decline by 2.52 percent, while those with lower shares decline by 3.19 percent, consistent with an ambient threat mechanism that operates across the full neighborhood economic landscape rather than targeting immigrant-origin sectors specifically. Our results may be consistent with individuals shifting their activity away from locations exposed to ICE enforcement rather than staying at home. However, a geographic substitution test finds no offsetting gains at same-type establishments elsewhere in the county, suggesting that these declines reflect genuine activity withdrawal rather than spatial reallocation.

These behavioral responses constitute real economic losses—borne by workers, students,

and families not directly targeted by enforcement—that do not appear in standard cost-benefit assessments. We translate the reduced-form estimates into monetary terms under conservative assumptions. Using a cross-state minimum wage ($600 per 40-hour work week) and the earnings return from Card (1999), the implied labor supply loss at a typical worker-intensive establishment amounts to $53,197 over one month; the foregone human capital value at a typical education facility totals $139,712 (capitalizing the lifetime earnings return to forgone schooling); and local business revenue losses add $2,760. Healthcare is excluded because facility-level visits show no significant decline. Combined across the three channels, the per-establishment community-side burden totals $195,669—approximately 11.4 times the $17,121 that DHS reports as the full per-person cost of arrest, detention, and removal (U.S. Department of Homeland Security, 2025). This likely represents a lower bound, as each event may affect multiple establishments within a half-mile radius.

This paper mainly contributes to the literature on the economic and social consequences of immigration enforcement in the United States (Amuedo-Dorantes and Arenas-Arroyo, 2019; Dee and Murphy, 2020; Alsan and Yang, 2024; East et al., 2023; Meadows, 2023; Rubalcaba, Bucheli and Morales, 2024; Ali, Brown and Herbst, 2024), including its effects on welfare program participation, educational outcomes, and labor market outcomes. Much of this literature focuses on immigrant households and individuals who are more directly targeted by enforcement, and most existing evidence predates the 2025 enforcement regime, whose defining feature is a sharp rise in community-based arrests of individuals without prior criminal records (East, Patler and Cox, 2026). Prior studies also typically rely on aggregate measures such as the county-level rollout of 287(g) agreements or the Secure Communities program, making it difficult to isolate the effects of community-based enforcement on local communities. Several recent exceptions examine the post-2025 period. East, Patler and Cox (2026) document this shift descriptively, showing a sharp increase in community-based arrests and a substantial decline in the share of arrested individuals with prior criminal convictions. Two concurrent studies document school-related effects using alternative, proxy measures of immigration enforcement. Dee (2025) exploits time-series variation in enforcement and finds that Operation Return to Sender is associated with a 22 percent increase in daily student absences in California's Central Valley. Figlio and Özek (2025) construct school-level exposure by combining students' countries of birth with state-level apprehension rates by country of origin in Florida, finding that increased enforcement lowers test scores for Spanish-speaking stu-

dents. Cravino et al. (2026) take a complementary macroeconomic approach, using a general equilibrium model to simulate the aggregate wage and price effects of removing 50 percent of carefully proxed unauthorized immigrants using 2023 ACS data.

Our paper advances the recent literature along three key dimensions. First, we leverage a novel geocoded dataset on ICE enforcement, which enables a distinct identification strategy to isolate the magnitude, spatial reach, and persistence of immigration enforcement's chilling effects at a highly localized level. This is particularly relevant in the post-2025 enforcement regime, characterized by a rise in community-based enforcement tactics. Second, our dataset captures not only arrest activities but also the presence of ICE in local communities, allowing us to disentangle the roles of direct exposure and perceived enforcement risk. Third, we use high-frequency foot traffic data to systematically capture the effects of ICE enforcement across a broad range of economic and social activities for different groups in near real time. In addition to student and labor market outcomes, we examine commercial establishments that have largely been overlooked in prior work but are central to understanding broader community-level impacts. Taken together, our approach provides the first event-level, high-frequency, and spatially explicit evidence on the mechanisms and community-wide impacts of immigration enforcement.

Second, we contribute to the literature on how perceived risk reshapes local economic activity. Economists have documented that crime reduces property values and labor supply (Cullen and Levitt, 1999; Linden and Rockoff, 2008), terrorism suppresses investment and commercial activity (Abadie and Gardeazabal, 2003; Brodeur and Wright, 2019), and disease outbreaks alter consumption and mobility (Chetty, Friedman and Stepner, 2024). Within the literature on localized threats directed at specific communities, police violence reduces crime reporting in affected neighborhoods (Desmond, Papachristos and Kirk, 2016), use-of-force incidents depress nearby students' academic performance (Ang, 2021), and hate crimes mobilize targeted communities politically (Li and Wu, 2024). The most closely related study to ours is Chiong, Kim and Kim (2025), who use retail foot traffic data to show that mass shootings reduce revenue at nearby stores by 19 percent, with disruption detectable up to 1.25 miles from the incident. Related work documents how historical injustices generate lasting distrust of public institutions (Alsan and Wanamaker, 2018). Our setting differs from all of these in two respects: the shock is a sustained state enforcement presence rather than an episodic event, and we measure its footprint across the full spectrum of neighborhood economic and

social activity. The hyperlocal spatial gradient we estimate provides direct evidence on how perceived risk propagates through geographic space—a quantity central to welfare analysis of enforcement policy but absent from prior work.

Third, we contribute to the spatial externalities literature and to the growing use of high-frequency geocoded data to study local economic activity. Place-based shocks—whether public investment, infrastructure, or regulatory intervention—generate effects that attenuate with distance (Busso, Gregory and Kline, 2013; Greenstone, Hornbeck and Moretti, 2010; Kline and Moretti, 2013; Glaeser and Gottlieb, 2008; Ahlfeldt et al., 2015). We document a novel instance of such an externality: enforcement operations that act as a localized negative amenity shock, depressing activity across a measurable geographic radius. The distance-decay function we estimate is directly analogous to treatment-distance gradients in the place-based policy literature, but with the sign reversed: suppression extends to private commercial establishments not themselves targeted, implying an effective footprint substantially larger than the operational perimeter. This design builds on a growing literature leveraging cell phone and GPS data to study urban mobility, local economic activity, and the effects of place-based policies (Goolsbee and Syverson, 2021; Couture and Handbury, 2020; Chen and Pope, 2020; Klopack and Luco, 2025; Cook et al., 2025; Athey et al., 2021; Chetty, Friedman and Stepner, 2024). We contribute to this literature by using foot traffic data to identify the causal effects of discrete enforcement events at 0.1-mile spatial resolution—an identification approach analogous to Chiong, Kim and Kim (2025) but applied to a sustained government enforcement presence rather than episodic violence, and spanning the full spectrum of neighborhood activity rather than a single sector.

The remainder of the paper is organized as follows. Section 2 provides institutional background on ICE enforcement operations and the mechanisms through which they may affect community behavior. Section 3 describes our data sources and sample construction. Section 4 presents the empirical strategy. Section 5 reports main results. Section 6 examines enforcement effects on formal labor demand and local crime rates. Section 7 translates the foot-traffic estimates into dollar costs. Section 8 concludes.

2 Background

2.1 Immigration Enforcement in the Interior

Immigration and Customs Enforcement (ICE) is the principal federal agency responsible for interior immigration enforcement in the United States. Within ICE, the Enforcement and Removal Operations (ERO) division carries out the identification, arrest, detention, and removal of individuals who are in the country without authorization or who have violated the terms of their immigration status. For much of the past two decades, the scale and targeting of interior enforcement fluctuated with successive administrations' priorities. Under the Obama administration, ICE adopted the Priority Enforcement Program, which focused resources on individuals with serious criminal convictions. The first Trump administration (2017–2021) broadened enforcement priorities but was constrained by limited personnel and legal challenges. The Biden administration narrowed priorities again, directing ICE to focus on recent border crossers, national security threats, and individuals with aggravated felony convictions.

The second Trump administration, beginning in January 2025, marked a qualitative shift in the character of interior enforcement. Three changes are particularly relevant to this study. First, on January 20, 2025, the Department of Homeland Security rescinded the longstanding "sensitive locations" policy—originally issued in 2011 and expanded in 2021—that had limited enforcement actions at or near education facilities, healthcare facilities, places of worship, and other community institutions.[4] Under the new guidance, ICE agents were instructed to use "common sense" in determining where to conduct operations, with no categorical restrictions on location. This policy change meant that the full range of community spaces—from churches to courthouses to education facility grounds—became potential sites of enforcement activity, fundamentally altering the calculus that community members faced when deciding whether to enter public spaces.

Second, the administration dramatically increased "at-large" arrests—operations in which agents apprehend individuals in community settings such as homes, workplaces, parking lots, and public streets, as opposed to transfers from jails and prisons. According to data from the Deportation Data Project, the number of community-based at-large arrests rose by approxi-

[4] The 2021 Biden-era memo, titled *Guidelines for Enforcement Actions in or Near Protected Areas*, expanded the original 2011 policy to cover social services establishments, places where children gather (including playgrounds and bus stops), homeless shelters, and public demonstrations.

mately eleven-fold in 2025 relative to the comparable period in 2024 (Hausman, Eckstein and Reny, 2025).[5] The composition of those arrested also shifted: by the fall of 2025, nearly half of individuals arrested by ICE in some jurisdictions had no criminal convictions, up from a substantially smaller share under previous administrations (see East, Patler and Cox, 2026, for a comprehensive analysis of ICE arrest patterns across the two Trump administrations). These operations were typically conducted by plainclothes agents, sometimes masked, arriving in unmarked vehicles—a mode of enforcement that was highly visible to bystanders even when the agents' specific targets were not publicly known.

Third, the administration launched large-scale, multi-day enforcement surges targeting specific metropolitan areas. These operations, which coordinated hundreds or thousands of DHS personnel across a single region, represented a departure from the diffuse, event-by-event enforcement that characterized prior administrations. The scale and visibility of these surges—and the community-wide disruption they generated—came into sharp relief during Operation Metro Surge in Minneapolis–Saint Paul, which deployed over 2,000 DHS personnel beginning in December 2025. During the operation, Minneapolis public schools transitioned to remote learning, hundreds of businesses closed in a coordinated show of solidarity with affected residents, and city officials estimated that the operation generated at least $203.1 million in economic impact within a single month from lost activity, disrupted livelihoods, and reduced food and shelter security (City of Minneapolis, 2026). Similar surges targeted Los Angeles, Chicago, Newark, Denver, and other cities with large immigrant populations. In California—the empirical setting for our analysis—ICE conducted extensive operations throughout 2025, including workplace raids at restaurants, food processing facilities, and construction sites, as well as sweeps at Home Depot parking lots where day laborers gathered.[6] ICE arrest data indicate that enforcement in California more than tripled between the first and second halves of 2025, with the share of arrestees lacking criminal records rising steadily throughout the year.

2.2 Qualitative Evidence of Community Behavioral Responses

Before presenting the systematic estimates, we briefly survey the journalistic and survey record of behavioral responses to the 2025 enforcement surge. This record motivates our

[5] ICE arrests through local jails approximately doubled over the same period. The at-large arrest increase reflected a deliberate strategic shift: in May 2025, senior White House officials directed ICE to substantially increase the daily rate of community-based apprehensions.

[6] California's sanctuary state law (SB 54, enacted in 2017) limits state and local law enforcement cooperation with federal immigration authorities but does not constrain ICE's own operational activities.

empirical investigation by illustrating that community withdrawal from education facilities, workplaces, healthcare facilities, and commercial establishments was already visible and documented before any systematic measurement was possible.

The most consistent accounts concern education facilities and healthcare. In California's Central Valley, absentee rates rose 22 percent cumulatively across five school districts in the weeks following nearby ICE raids; in Los Angeles, one school lost a quarter of its student body the week enforcement operations began in early June (EdSource, 2025). Attendance losses appear to respond to the perceived threat of enforcement as much as to confirmed operations: a single unverified rumor that agents had appeared near an elementary school was sufficient to generate a nearly 50 percent absence rate at one school on a single day. Healthcare shows a similar pattern. A national survey of 691 providers conducted in early 2026 found that 84 percent reported significant or moderate declines in patient visits following the January 2025 rescission of the sensitive locations policy (Physicians for Human Rights and Migrant Clinicians Network, 2026). Dallas public health clinics serving Hispanic children saw back-to-school vaccination visits fall from roughly 11,500 to 5,800 between summer 2024 and summer 2025. In Los Angeles, appointment no-show rates at one community health system more than tripled during the peak enforcement period, and physicians documented serious consequences from deferred care, including cases of a ruptured eardrum and a burst appendix that resulted from patients delaying treatment (Physicians for Human Rights and Migrant Clinicians Network, 2026).

The labor market and commercial effects are equally striking. A 2025 consumer survey found that 44 percent of Hispanic consumers nationally reported avoiding public spaces—restaurants, grocery stores, retail establishments—because of fear of enforcement (ThinkNow, 2025). Los Angeles restaurant owners reported sales declines of 30 to 40 percent, describing customers who had reduced outings to essential errands only (LA Public Press, 2025). On the supply side, workers across industries reported scanning social media for enforcement alerts each morning before commuting, staying home if agents were reported along their route. Construction sites in multiple cities were abandoned for weeks after nearby enforcement events, leaving projects incomplete (American Immigration Council, 2025). ICE's operations at California Home Depot parking lots—where day laborers had long gathered to find work—eliminated a labor market that had functioned for decades; Human Rights Watch documented nine such raids in the Los Angeles area alone, and one Guatemalan day laborer

was killed after fleeing onto a freeway to escape agents (Human Rights Watch, 2025). Food logistics warehouses in Chicago, staffed overwhelmingly by immigrant workers, saw their labor forces thin as investigators described enforcement threats that "shadow every shift" (Investigate Midwest, 2025). These accounts motivate the central empirical questions: whether enforcement operates as much through labor supply as through demand channels, and how far its behavioral footprint extends beyond the individuals directly targeted.

3 Data

This paper draws on four data sources: ICEout records of ICE enforcement activity from January 2025 to January 2026, high-frequency foot traffic data from Advan Research, employer job postings from LinkUp, and incident-level administrative crime data.

3.1 ICE Activity Data

We measure local enforcement exposure using the ICEout database maintained by the Pueblo Project Foundation, which collects community-submitted reports of possible ICE activity across the United States and publishes them in near real time on a publicly accessible platform.[7] Our sample covers the period from January 1, 2025 through January 31, 2026. Each report includes the date, geographic coordinates, and a classification of event type, and is accompanied by descriptions and photographic documentation of the observed activity. All submissions are reviewed by a moderator team prior to publication. Reports originate from three primary sources. First, a subset is submitted through trusted community partners, such as rapid response networks and nonprofit organizations, and is labeled as confirmed. Second, reports may be submitted directly by individual witnesses who observe enforcement activity. Third, some reports are compiled through monitoring publicly accessible online channels, including social media reporting groups (e.g., Facebook or Instagram) that track ICE activity. Although only submissions from trusted partners are formally labeled as confirmed, reports from the latter two sources are also reviewed by the moderator team and assessed based on the accompanying descriptions and photographic documentation prior to publication.

ICEout reports span a range of enforcement intensities. For our analysis, we classify events as *contact* operations—those involving direct engagement with individuals, including detainments, arrests, raids, questioning, or use of force—and *non-contact* operations, which include passive presence activities such as agents observed in an area, canvassing, or warnings of future activity. Reports involving other federal agencies without immigration authority or non-operational activity are excluded from the analysis sample. This binary classification allows us to examine whether community responses differ by the directness of enforcement contact.

The primary advantages of the ICEout data include comprehensive coverage of recent en-

[7] https://iceout.org/en/. Please see Figure A1 for illustration.

forcement activity, high spatial resolution, and broader documentation of enforcement events relative to publicly available administrative records. The most widely used dataset on Immigration and Customs Enforcement (ICE) activity is based on Freedom of Information Act (FOIA) requests compiled and released by the Deportation Data Project.[8] While official administrative data cover the relevant sample period and include all recorded arrests, a key limitation is their lack of geographic precision. Arrests are reported at the level of field offices or Areas of Responsibility, making it impossible to identify the specific communities in which enforcement actions occurred.[9] In contrast, ICEout provides precise geocoded locations and exact dates, which are essential for the hyperlocal research design we implement. Moreover, ICEout captures a broader range of ICE-related activities beyond arrests and encounters. In addition to active enforcement events, the dataset includes non-active presence activities, such as agents parked or idling in visible locations and routine activities (e.g., errands or eating). This broader coverage allows us to better distinguish underlying mechanisms and develop a more comprehensive understanding of how ICE presence affects local communities.

One potential limitation of the ICEout data is that they are generated through community reporting rather than official administrative records and may therefore be subject to incomplete reporting. Reporting intensity may vary systematically with the strength of local reporting networks and may also evolve over time. As a result, some enforcement events may go unobserved, potentially leading to the misclassification of treated units as controls. While this is a valid concern, our identification strategy does not require that all enforcement events be observed. Instead, it requires only that the events we observe are accurately dated and precisely geolocated, consistent with the high-frequency, high-resolution geographic data we employ. If underreporting is classical, meaning unrelated to potential outcomes, this would attenuate our estimates toward zero by introducing noise into treatment assignment. A more substantive concern would arise if reporting intensity were systematically correlated with local characteristics that also affect outcomes. However, our research design exploits hyperlocal spatial variation in exposure within small geographic areas and short event-time windows. By comparing outcomes in close spatial and temporal proximity to observed events, we limit the scope for broader reporting patterns to bias the estimated effects. To the extent that re-

[8] https://deportationdata.org/

[9] An alternative data source is the Transactional Records Access Clearinghouse (TRAC), which provides county-level arrest information after substantial data processing. However, the data extend only through 2018 and lack the spatial resolution necessary to assign enforcement exposure at the community level.

porting networks operate at the neighborhood level, differential reporting intensity is more likely to vary across, rather than within, small geographic areas. As a result, cross-community differences in reporting are unlikely to drive our estimates.

3.2 Foot Traffic Data

To measure weekly foot traffic from October 1, 2024 through February 28, 2026, we use high-resolution, near–real-time mobility data from the Advan Weekly Patterns Plus dataset.[10] Advan's foot traffic data are constructed from a large, anonymized panel of mobile devices in the United States, typically capturing signals from approximately 45 to over 100 million monthly active devices. These data are derived from GPS-based location signals collected through third-party smartphone applications whose users have opted in to share location information, in compliance with applicable privacy regulations. The Weekly Patterns Plus dataset aggregates device-level location signals to the point-of-interest (POI) × week level. POIs include retail stores, restaurants, schools, hospitals, and a broad range of other public-facing establishments. For each POI-week observation, the dataset reports the geographic coordinates, industry classification, and the total number of visits or unique visitors observed within Advan's mobile device panel.

Compared with traditional cellular tower–based data, Advan's GPS-based SDK mobility data generally provide higher spatial resolution, often achieving location accuracy on the order of tens of meters under typical conditions. The dataset records billions of daily location "pings," with the median device observed more than 100 times per day. This high-frequency tracking enables precise attribution of visits to specific POIs—including more than 150 million geofenced locations—and supports the construction of granular measures such as weekly visit counts. Advan's foot traffic data have been validated against external benchmarks, including corporate revenue reports and transaction data (Li et al., 2024; Klopack and Luco, 2025).[11] The dataset is widely used in academic research to measure a broad range of social and economic activities (Chen and Rohla, 2018; Chen and Pope, 2020; Allcott et al., 2020; Goolsbee and Syver-

[10] Prior to 2023, these data were distributed under the name SafeGraph Patterns. Although the underlying device-level data remain largely consistent, Advan employs a refined aggregation methodology. For example, Advan computes visits and unique visitors based on each POI's precise geometry, whereas the former SafeGraph approach allocated portions of visits to multiple POIs within a shared polygon when geometries overlapped. A detailed comparison of the methodologies is available at: https://docs.deweydata.io/docs/methodolgy-difference-from-safegraph-patterns.

[11] See also the official documentation for the Weekly Patterns Plus dataset at: https://docs.deweydata.io/docs/weekly-patterns-plus.

son, 2021; Chang et al., 2021; Chiong, Kim and Kim, 2025; Cook et al., 2025; Figlio and Özek, 2025).

3.3 Labor Demand and Crime Data

Job postings. We measure formal employer-side labor demand using LinkUp's RAW job-postings database, restricting to U.S. postings observed between October 1, 2024 and December 31, 2025. LinkUp collects vacancy information directly from employer career websites rather than third-party job boards, updating the underlying data daily; its RAW product covers tens of thousands of employers globally and contains historical postings indexed since 2007. Because the source is the employer's own career site, the data are designed to reflect firms' actual posted labor demand while reducing the duplicate, expired, and intermediary listings common in board-scraped datasets. Each record includes a unique job identifier, company name and identifier, city, state, ZIP code, MSA, posting and removal dates, SOC/O*NET occupation mappings, full job description, compensation fields, and employment type. The unit of observation is a posted vacancy on a company career site rather than a realized hire; the dataset captures the posting stage of the hiring process and is best interpreted as a measure of formal employer-side labor demand. Because these vacancies correspond to jobs filled through standard legal employment channels, they measure labor demand in the hiring channel accessible to native-born and legal workers; undocumented workers, who disproportionately participate in informal employment arrangements outside career-site pipelines, are generally unable to apply.

We classify each vacancy by skill level using O*NET Job Zone assignments embedded in the SOC code fields. Postings mapped to Job Zones 4–5 (requiring considerable or extensive preparation) are classified as high-skill; those mapped to Job Zones 1–2 (requiring little or some preparation) as low-skill; Job Zone 3 postings and those without a matched SOC code are classified as other. For job titles that cannot be matched directly to an O*NET Job Zone, we apply an SGDClassifier with character n-gram features (3–5-grams, hashing vectorizer) trained on the matched postings as labeled examples.[12]

[12]The classifier is trained on 32,292 job title–Job Zone pairs and evaluated on a held-out validation set of 8,074 pairs. Macro F1 is 0.81 and weighted F1 is 0.83; category-specific F1 scores are 0.85 for high-skill, 0.89 for low-skill, and 0.69 for the residual other category.

Crime incidents. We obtain incident-level crime records with geocoded locations and timestamps from the open-data portals of six jurisdictions with substantial ICE enforcement activity during our sample period: Los Angeles County and San Diego County in California; the District of Columbia; Cook County in Illinois; Hennepin County in Minnesota; and Harris County in Texas. These jurisdictions were included because they maintain publicly accessible, geocoded, time-stamped incident records current through 2025, enabling the hyperlocal stacked design used in Section 6.2. The data span January 1, 2025 to January 31, 2025. We classify all reported incidents into violent crimes (homicide, robbery, aggravated assault, and sexual assault) and non-violent crimes (burglary, property theft, motor vehicle theft, and other offenses). Each incident is geo-matched to its census block using the coordinates provided in the administrative record.

3.4 Descriptive Patterns of ICE Activities

In this section, we document descriptive patterns of ICE enforcement activity using ICEout data and compare them with official ICE arrest data collected by the Deportation Data Project.

Figure A2 compares the geographic distribution of ICE enforcement activity across states using two distinct data sources, covering the period from January 2025 to mid-October 2025, as the Deportation Data Project data are available only through mid-October 2025. Panel A reports the number of ICE arrest events compiled from ICEout, which aggregates community-reported sightings and enforcement activity. Panel B reports the number of officially recorded ICE community-based arrests using administrative data from the Deportation Data Project. Several patterns emerge. First, both data sources reveal substantial cross-state heterogeneity in enforcement intensity. States such as California, Illinois, Texas, Massachusetts, and New York consistently rank among the highest-activity states in both panels, suggesting that ICEout captures meaningful variation in enforcement exposure across locations. Second, the two measures differ conceptually and quantitatively. ICEout records reported ICE arrest events observed by community members. In contrast, official ICE data measure the number of individuals arrested in community-based operations. Importantly, a single enforcement event in ICEout may involve multiple individuals arrested, while official data count arrests at the individual level. As a result, the magnitudes are not directly comparable. Moreover, ICEout data may reflect variation in reporting intensity across communities and over time, whereas

official data reflect administrative records of arrests.

California stands out in both datasets as the state with the highest enforcement activity during January 2025–October 2025, accounting for 51.8 percent of all ICEout reports and ranking among the top states in official community-based arrests. The primary sample is California, which accounts for 51.8 percent of all ICEout reports in our sample period (January 2025–January 2026). To support external validity and mitigate concerns that California's distinctive economic and political environment drives results, we supplement it with the top enforcement cities and towns from five additional states: Illinois, Minnesota, New York, Oregon, and Texas. The combined sample covers six states and includes 10,018 events in total.

Figure A3 presents descriptive evidence on ICE enforcement activity in California using ICEout data. Panel A reports the distribution of reported activity types from January 2025 to January 2026. The largest categories include arresting ndividuals, loitering, and errands. The distribution indicates that ICEout captures both direct arrest events and broader enforcement presence or surveillance activity.

Panel B then plots the weekly number of reported enforcement events over time in California. Reported activity remains relatively low and stable through the first five months of the year, followed by a sharp increase in early June. Reported events peak during the week of June 8 and gradually decline thereafter, although levels remain elevated relative to the pre-June period for several weeks. Because ICEout is based on community reports, these fluctuations may reflect changes in both underlying enforcement activity and reporting intensity. In California, the pronounced peak may also coincide with heightened public attention and protest activity in Los Angeles related to ICE enforcement during this period. Together, these patterns demonstrate that ICEout provides high-frequency variation in reported enforcement exposure within the state.

4 Empirical Strategy

Causal evidence on the community-wide effects of immigration enforcement is limited, in part because enforcement operations are not randomly allocated across space. Areas targeted by ICE may differ systematically from untargeted areas along dimensions—such as immigrant population share, local economic conditions, or prior crime rates—that independently affect foot traffic and economic activity. Directly comparing locations near enforcement events to those in other neighborhoods is therefore likely to produce biased estimates.

To address this concern, we exploit hyperlocal variation in proximity to ICE enforcement events. Our analysis compares changes in weekly visits over time at points of interest (POIs) located very close to an ICE event with those at POIs slightly farther away, yet still within the same local environment. Both groups of establishments share similar neighborhood characteristics and are subject to the same local economic conditions, differing primarily in their proximity to the enforcement operation. The behavioral effects of enforcement—driven by word-of-mouth information and localized fear of deployment—dissipate with distance from the event site. Our empirical strategy proceeds in two steps. We first estimate a spatial decay specification to determine the distance threshold $\bar{d}$ beyond which enforcement effects become negligible. We then use this empirically determined $\bar{d}$ to classify POIs as treated or control and estimate an event-study specification to characterize the temporal dynamics of the response.

4.1 Empirical Design

We implement our analysis using an empirical design that addresses the well-known biases arising from staggered treatment timing in two-way fixed effects frameworks (Goodman-Bacon, 2021; Callaway and Sant'Anna, 2021). For each ICE enforcement event in our sample, we construct a separate "stack" consisting of all POIs within a three-mile radius, observed over a window of four weeks before and seven weeks after the event date. To ensure clean identification, we restrict the sample to POIs that are either never treated (i.e., not located near any ICE event during the full sample period) or first treated during the current event window. This restriction eliminates the "forbidden comparisons" that arise when previously treated units serve as controls in staggered adoption designs. The stacked datasets are then combined for estimation, with equal weight assigned to each enforcement event.

4.2 Spatial Decay Specification

We begin by documenting how the impact of ICE enforcement operations varies with distance from the event location. Our hypothesis is that the effect on foot traffic declines with distance, reflecting the localized nature of enforcement-induced avoidance behavior. To test this hypothesis and determine the treatment threshold $\bar{d}$, we estimate the following spatial decay specification:

$$Y_{i,s,w} = \sum_{d=1}^{D} \beta_d \cdot \mathbf{1}[\text{Dist}_i \in \text{Bin}_d] \times \text{Post}_{s,w} + \alpha_{i,s} + \gamma_{s,w} + \eta_{q(i),w} + \varepsilon_{i,s,w} \tag{1}$$

where $Y_{i,s,w}$ denotes the main outcome of interest: total weekly visits to POI i in stack s during week w, normalized by the POI's pre-event mean within the stack so that coefficients are directly interpretable as percentage-point changes in activity. We study two outcomes: total weekly visits and weekly unique visitors. Total visits capture the full contraction in neighborhood activity, encompassing both the extensive margin—fewer distinct individuals visiting a location—and the intensive margin—fewer trips among continuing visitors. Unique visitors isolate the extensive margin. The two outcomes provide complementary evidence on the behavioral response to enforcement, and we report both throughout.

$\mathbf{1}[\text{Dist}_i \in \text{Bin}_d]$ is an indicator for whether POI i falls within distance bin d from the ICE event, measured in 0.1-mile intervals. $\text{Post}_{s,w}$ equals one if week w falls after the enforcement event in stack s. The coefficients of interest, β_d, capture the post-event percentage-point change in activity at POIs within each distance bin relative to the omitted bin (the outermost ring, 2.9–3.0 miles). We include POI×stack fixed effects ($\alpha_{i,s}$) to absorb time-invariant establishment characteristics within each stack, and week×stack fixed effects ($\gamma_{s,w}$) to account for aggregate shocks common to all POIs within a given event window in a given week. To allow for differential week-to-week fluctuations across establishments of different baseline scale, we additionally interact pre-period visit quartiles with week fixed effects ($\eta_{q(i),w}$), where $q(i) \in \{1, 2, 3, 4\}$ denotes the quartile of POI i's pre-event visit distribution. Standard errors are clustered at the census-tract level.

We define $\bar{d}$ as the distance beyond which the estimated coefficients $\hat{\beta}_d$ become statistically indistinguishable from zero. This threshold determines the spatial reach of enforcement effects and provides the treatment-control boundary for the event-study analysis that follows.

We present the results from this specification in Section 5.1.

4.3 Event-Study Specification

Given $\bar{d}$, we classify POIs within $\bar{d}$ of an enforcement event as treated and those between $\bar{d}$ and three miles as controls. We then estimate the dynamic effects of ICE enforcement operations on foot traffic with the following event-study specification:

$$Y_{i,s,w} = \sum_{k \neq -1} \beta_k \cdot \mathbf{1}[w - w_s^* = k] \times \text{Treat}_i(\bar{d}) + \alpha_{i,s} + \gamma_{s,w} + \eta_{q(i),w} + \varepsilon_{i,s,w} \tag{2}$$

where $\mathbf{1}[w - w_s^* = k]$ is an indicator equal to one if week w is k weeks relative to the enforcement event in stack s, and $\text{Treat}_i(\bar{d}) = \mathbf{1}[\text{Dist}_i \leq \bar{d}]$ indicates whether POI i is within distance $\bar{d}$ of the event. The coefficients β_k trace the difference in weekly visits between treated and control POIs at each week relative to the event, normalized to zero at $k = -1$ (the week before the enforcement operation). Fixed effects $\alpha_{i,s}$, $\gamma_{s,w}$, and $\eta_{q(i),w}$ are defined as in equation (1). Standard errors are clustered at the census-tract level.

The identifying assumption is that, in the absence of the ICE enforcement event, weekly visits to treated and control POIs would have evolved in parallel. While this assumption is inherently untestable, the event-study framework provides a built-in diagnostic: if the pre-event coefficients $\{\beta_k\}_{k<-1}$ are statistically indistinguishable from zero, this indicates that treated and control POIs exhibited similar trends prior to the enforcement event, supporting the parallel trends assumption.[13]

To summarize the average treatment effect, we also estimate a difference-in-differences specification that collapses the event-time variation into a single post-treatment indicator:

$$Y_{i,s,w} = \beta \cdot \text{Post}_{s,w} \times \text{Treat}_i(\bar{d}) + \alpha_{i,s} + \gamma_{s,w} + \eta_{q(i),w} + \varepsilon_{i,s,w} \tag{3}$$

[13]We acknowledge that enforcement events may coincide with time-varying, unobserved local shocks that independently affect foot traffic. Our rich set of fixed effects—which absorb stack-specific temporal patterns and industry-specific shocks—mitigates this concern. Moreover, the hyperlocal nature of our comparisons, in which treated and control POIs are located within the same three-mile radius, limits the scope for spatially varying confounders.

where $\text{Post}_{s,w}$ equals one if week w falls on or after the enforcement event in stack s. The coefficient β captures the average post-event percentage-point change in weekly visits at treated relative to control POIs. All other variables and fixed effects are defined as in equation (2).

4.4 Summary Statistics

Table 1 summarizes the analysis sample. Panel A describes the 10,018 ICE enforcement events. 59.7 percent of events involve direct contact with individuals (e.g., arrests, questioning, or use of force); the remainder are non-contact operations (e.g., observed presence, canvassing, or warnings of future activity). The average operation involves 4.0 officers (SD = 5.1). Using a 0.1-mile proximity criterion (see Section 5.4), 1.1 percent of events have at least one education facility within 0.1 mile, 1.7 percent have at least one healthcare establishment, and 8.3 percent have at least one workplace; 91.7 percent have no sensitive location within 0.1 mile.[14]

[Table 1 about here]

Panel B describes the stacked analysis panel, comprising 12.4 million POI×week observations across all event stacks. Mean weekly visits per POI are 1,454 (SD = 4,489), and mean distance to the event is 1.77 miles. Treated POIs—those within 0.5 miles of an event—account for 8.9 percent of the panel; the remainder serve as within-neighborhood controls. At the POI×stack level (1.02 million unique cells), education facility POIs comprise 3.8 percent of observations, healthcare POIs 2.7 percent, and any workplace POI 93.3 percent (of which 67.4 percent are consumer-facing and 28.0 percent are worker-intensive).[15]

One concern of our identification strategy is that treated establishments—those located closer to an ICE event—may differ systematically from control establishments on dimensions that independently predict foot traffic trends. To address this concern, we conduct two complementary analyses. First, Table 2 presents a balance test comparing pre-event visit levels at week -1 between treated and control POIs. In the raw data, treated POIs exhibit higher

[14]Groups are non-mutually exclusive. The 0.1-mile proximity criterion is preferred over a nearest-POI assignment because the Advan dataset covers only a sample of establishments, so the nearest POI in the data may not be the nearest establishment in the full population, and forcing a single nearest-type label introduces measurement error that attenuates category-specific estimates.

[15]Consumer-facing establishments include retail, groceries, restaurants, personal care, and accommodation; worker-intensive establishments include manufacturing, construction, agriculture, utilities, warehousing and trucking, and administrative support, as classified by NAICS codes. The balance table further stratifies worker-intensive POIs by the immigrant-workforce intensity of their 2-digit NAICS industry, based on BLS workforce composition data.

visit counts than controls—a pattern expected given that ICE operations tend to occur near dense commercial areas. Crucially, these unconditional differences vanish once we condition on census-tract and baseline size-quartile fixed effects: the full-sample difference narrows to 25.9 visits (SE = 20.7) and becomes statistically indistinguishable from zero. The same pattern holds for education facilities, healthcare facilities, and consumer-facing establishments. This is consistent with between-tract sorting rather than within-tract selection: treated and control establishments within the same local area and size class are broadly comparable in pre-event activity. Second, the event-study estimates reported in Section 5 display flat pre-trends across all specifications, providing direct evidence against differential pre-existing visit trajectories.

[Table 2 about here]

5 Main Results

We organize the results as follows. Section 5.1 establishes the baseline effects on total visits and unique visitors. Section 5.2 examines heterogeneity by enforcement type and force scale to investigate the mechanism. Section 5.3 characterizes variation across POI types. Section 5.4 explores amplification by institutional context. Section 5.5 decomposes visitor flows by demographic origin. Section 5.6 tests for geographic substitution.

5.1 Effects on Foot Traffic

Total visits capture both the intensive margin—how often the same individuals reach a location—and the extensive margin—how many distinct individuals do so. Unique visitors isolate the extensive margin alone: a decline in this measure indicates that fewer individuals are reaching treated POIs, not merely that a stable set of individuals is visiting less often.

Figure 1 reports coefficients from the spatial decay specification in equation (1). The response is most negative at the closest distance bins, attenuates monotonically with distance from the event site, and becomes statistically indistinguishable from zero at approximately 0.5 miles in both panels. The gradient is smooth and continuous rather than step-like, which argues against confounding by sharp neighborhood or administrative boundaries. This pattern motivates the treatment-control classification used throughout: POIs within 0.5 miles are assigned to the treated group and those between 0.5 and three miles serve as the within-neighborhood control.

[Figure 1 about here]

Figure 2 reports weekly event-study estimates from equation (2). Panel (a) uses total visits as the outcome and panel (b) uses unique visitors. Pre-event coefficients are close to zero in both panels, consistent with parallel pre-trends. At event week zero the path shifts sharply downward in each. Visits decline by a statistically significant 1.69 percent relative to the pre-event mean of 1,555.5 visits per week; unique visitors decline by a statistically significant 1.04 percent relative to a pre-event mean of 1,331.0 per week. The visitor estimate is somewhat smaller than the visits estimate, indicating that enforcement depresses both margins of activity, but that the intensive margin—reduced visit frequency among those who do come—contributes to the overall decline alongside the reduction in distinct visitors. Table 3 reports the corresponding collapsed difference-in-differences estimates; Panel A covers visits

and Panel B covers unique visitors.

[Figure 2 about here]

The temporal profile is informative. Rather than a spike-and-recovery pattern characteristic of transient avoidance, the post-event paths in both panels are sustained across the seven-week observation window with no upward trend toward baseline. This persistence is inconsistent with intertemporal reallocation and is more consistent with belief updating: residents learn about local enforcement risk from the event, revise their perceived probability of future encounters in the area, and maintain adjusted mobility patterns over the medium run.

Table 3 confirms that the baseline estimates are robust. Panel A reports total visits and Panel B reports unique visitors; both show negative, statistically significant effects across all seven specification variants. Within the fixed-effects alternatives for visits, dropping baseline size×week fixed effects yields −1.60 percent of the pre-event mean; adding 2-digit NAICS×week fixed effects yields −1.58 percent; adding 4-digit NAICS×week yields −1.31 percent. The stability across these columns indicates that the effect is not confounded by differential trends across industries or establishment size classes. Dropping new entrants or exiters leaves the visits estimate essentially unchanged at −1.69 percent. A balanced panel yields −1.72 percent, significant at the 1-percent level. Panel B shows that the visitor estimates track the visits panel closely: dropping the size quartile fixed effects yields −0.96 percent, adding 2-digit NAICS×week yields −0.92 percent, and adding 4-digit NAICS×week yields −0.78 percent. Sample restriction variants leave the visitor baseline unchanged at −1.04 percent; the balanced panel yields −1.06 percent, also significant at the 1-percent level.

[Table 3 about here]

5.2 Mechanism: Enforcement Type and Force Scale

Having established the baseline effect, we investigate which features of enforcement operations drive the behavioral response. We examine two dimensions: the nature of the enforcement activity (contact versus non-contact), and the operational scale. The two analyses together illuminate the mechanism: whether behavior responds to any enforcement signal regardless of its character, and whether salience amplifies the community-wide response.

5.2.1 Contact versus Non-Contact Enforcement

Under a direct-confrontation hypothesis, only operations involving arrests, use of force, or aggressive tactical activity should alter behavior; passive surveillance and non-confrontational enforcement should produce negligible effects. Under an ambient-threat interpretation, any operation that generates community awareness—regardless of whether physical contact occurs—constitutes a behavioral trigger, so contact and non-contact operations should produce comparable responses.

[Figure 3 about here]

Figure 3 estimates the event-study separately for contact enforcement and non-contact enforcement, with visits in panels (a) and (c) and unique visitors in panels (b) and (d). Contact enforcement generates a statistically significant decline of 2.23 percent in visits and 0.79 percent in unique visitors. Non-contact enforcement generates a statistically significant decline of 1.30 percent in visits and 1.28 percent in unique visitors. In the visits dimension, contact operations show a somewhat larger point estimate; in the visitors dimension, the two types are essentially equal. Both outcomes decline significantly under both enforcement types.

The result that non-contact operations—plainclothes surveillance, stationary monitoring, driving patrols—produce behavioral responses of broadly similar magnitude to contact operations involving arrests and confrontations is the central finding of this subsection. Direct observation of confrontational activity is not a prerequisite for community-wide deterrence. These patterns are consistent with an ambient-threat mechanism: any enforcement event that reaches community awareness through informal networks can trigger behavioral change, with residents updating perceived local risk regardless of whether any use of force occurred.

5.2.2 Force Scale

We next classify events by operational scale—large-scale operations (classified as "Large" or "Operation" in the ICEout records) versus small-scale deployments (classified as "Solo," "Small," or "Medium")—to test whether the visibility of an operation amplifies the community-wide response.

[Figure 4 about here]

Figure 4 presents event-study estimates for high-force operations in panels (a) and (b)

and low-force operations in panels (c) and (d). High-force operations generate a statistically significant decline of 1.69 percent in visits and 1.45 percent in unique visitors. Low-force operations generate a statistically significant decline of 1.74 percent in visits and 0.71 percent in unique visitors. At the visits level the two estimates are nearly identical; at the visitor level the low-force estimate is somewhat smaller, though both are non-trivial in magnitude and both are precisely estimated relative to zero.

The comparable magnitude across high- and low-force deployments reinforces the ambient-threat interpretation. Small, discreet deployments generate foot-traffic declines that are statistically and economically comparable to those following highly visible raids. Operational visibility does not appear to be the primary driver of community-wide deterrence.

Taken together, the two analyses converge on a consistent picture: community information diffusion is the primary channel through which enforcement affects local activity. Non-contact and low-force operations produce responses comparable to contact and high-force operations alike, indicating that neither direct confrontation nor operational scale is required for deterrence to materialize across the broader neighborhood.

5.3 Effects by POI Type

We now characterize how enforcement effects vary across POI types. We examine four institutional domains where the welfare consequences of enforcement-induced disruption are most direct.

5.3.1 Workplaces

Figure 5 reports event-study estimates for all workplace POIs. Panel (a) shows visits declining by a statistically significant 1.35 percent relative to a pre-event mean of 1,531.3; panel (b) shows unique visitors declining by 0.94 percent relative to a pre-event mean of 1,327.5. Both outcomes show flat pre-event paths and persistent post-event declines.

[Figure 5 about here]

To understand whether this response operates through demand or through supply, Figure 6 separates consumer-facing establishments—retail, restaurants, grocery, and personal services (panels (a) and (b))—from worker-intensive establishments, such as manufacturing, construction, agriculture, and warehousing (panels (c) and (d)). These NAICS-based categories

serve as proxies for the predominant type of foot traffic at each establishment (see Section 4.4).

[Figure 6 about here]

Consumer-facing establishments show a statistically significant decline of 1.02 percent in visits and 0.88 percent in unique visitors, relative to a pre-event mean of 1,352.7. Worker-intensive establishments show a larger and statistically significant decline of 2.76 percent in visits and 1.32 percent in unique visitors, relative to a pre-event mean of 1,973.3. Both establishment types show flat pre-event paths and persistent post-event declines in both outcomes. Both demand-side and supply-side activity contract simultaneously, with the larger effect at worker-intensive sites consistent with workers facing heightened personal exposure risk relative to customers making discretionary visits.

Appendix Figure A4 extends this analysis by splitting workplace POIs according to the immigrant-workforce intensity of their underlying NAICS industries (see Section 4.4 for classification details). Industries with higher immigrant employment shares—food manufacturing, construction, agriculture, restaurants, accommodation, and warehousing—show a statistically significant post-event decline of 2.52 percent in visits and 1.18 percent in unique visitors. Industries with higher native-born employment shares—finance, professional services, and information—show a statistically significant decline of 3.19 percent in visits and 1.26 percent in unique visitors. The broadly comparable magnitudes across both industry types are consistent with a wide ambient-threat response: foot traffic falls across establishments regardless of their workforce composition.

5.3.2 Education Facilities

Attendance at education facilities is among the most welfare-relevant margins affected by enforcement. Disruptions translate directly into lost instructional time, with potential consequences for human capital accumulation that are difficult to reverse. Because school schedules are fixed and attendance is partly compulsory, a sustained reduction in mobility near education facilities reflects genuine avoidance rather than intertemporal substitution.

[Figure 7 about here]

Figure 7 reports event-study estimates for education facility POIs. Panel (a) shows visits declining by a statistically significant 4.82 percent relative to a pre-event mean of 1,573.7 visits per week—approximately three times the baseline all-POI effect. Panel (b) shows unique

visitors declining by a statistically significant 1.63 percent relative to a pre-event mean of 1,377.7. The visit decline reflects a combination of both the intensive margin (existing attendees reducing trip frequency) and the extensive margin (fewer distinct individuals reaching the facility); the visitor estimate isolates the extensive margin and confirms that fewer distinct individuals—students, staff, and accompanying family members—are reaching the facility. Pre-event coefficients are close to zero in both panels and the post-event decline shows no evidence of recovery over the seven-week window. The scale of the disruption is consistent with direct evidence on student attendance: Dee (2025) documents a 22 percent increase in daily student absences following Operation Return to Sender in California's Central Valley school districts. The persistent attendance shortfall carries potential implications for student achievement and human capital accumulation (Figlio and Özek, 2025).

5.3.3 Healthcare Establishments

Unlike education facilities or workplaces, visits to healthcare establishments are driven largely by acute clinical need rather than routine schedule or employment obligation.

[Figure 8 about here]

Figure 8 reports event-study estimates for healthcare establishments. Panel (a) shows visits at -0.72 percent and panel (b) shows unique visitors at -0.77 percent. Both are small in magnitude and statistically indistinguishable from zero, with flat pre-event paths. The null results in both outcomes suggest that enforcement does not detectably reduce foot traffic to healthcare establishments. One interpretation is that healthcare demand is relatively inelastic to ambient enforcement risk: patients with pressing clinical needs continue to seek care despite nearby enforcement activity, in contrast to the discretionary and routine activities that appear more responsive to local enforcement signals.

5.3.4 Commercial POIs

Commercial establishments—retail, restaurants, personal services—measure community-wide disruption at the everyday margin of neighborhood economic life.

[Figure 9 about here]

Figure 9 reports event-study estimates for commercial POIs. Visits decline by a statistically significant 1.01 percent and unique visitors decline by 0.87 percent, relative to pre-event

means of 1,354.9 and 1,163.8, respectively. Both outcomes show flat pre-event paths and persistent post-event declines, consistent with sustained contraction in everyday neighborhood commerce.

5.4 Amplification by Institutional Context

The analyses above characterize the average effect of enforcement events on POIs of specific types. We now ask a complementary question: does the institutional context *surrounding* a raid amplify its neighborhood-wide impact? We classify each raid by the institutional context within 0.1 mile of its geocoordinates. Specifically, we define a raid as proximate to an education facility (healthcare establishment, workplace) if at least one POI of that type lies within 0.1 mile; the residual group—raids with no school, healthcare, or workplace POI within 0.1 mile—forms a fourth category. Groups are non-mutually exclusive for the three sensitive-location types. We estimate the event-study specification separately for each subsample, using total neighborhood foot traffic across all POI types as the outcome.

We adopt a proximity criterion rather than a nearest-POI assignment for two reasons. First, the Advan foot-traffic dataset covers only a sample of commercial and institutional establishments, so the nearest POI in the data may not reflect the nearest establishment in the full population. Second, assigning each raid to a single nearest type introduces classification error that attenuates any category-specific amplification. The 0.1-mile radius directly captures the set of establishments in the immediate vicinity of each operation.

[Figure 10 about here]

Figure 10 presents event-study estimates for all four groups in a single four-panel figure, with visits as the outcome. Panel (a) shows that raids within 0.1 mile of a workplace generate a statistically significant neighborhood-wide decline of 1.71 percent (SE = 0.36; $t = -4.77$), broadly in line with the overall baseline effect.

Panel (b) shows that raids within 0.1 mile of an education facility generate a statistically significant neighborhood-wide decline of 1.90 percent (SE = 0.60; $t = -3.19$). The pre-event path is flat, and the effect is persistent over the seven-week window.

Panel (c) shows that raids within 0.1 mile of a healthcare establishment generate a statistically significant decline of 1.96 percent (SE = 0.62; $t = -3.17$). The result indicates that

enforcement near healthcare establishments suppresses broader neighborhood activity, consistent with ambient-threat deterrence.

Panel (d) shows that raids with no sensitive location within 0.1 mile generate a statistically significant decline of 1.32 percent (SE = 0.51; $t = -2.59$), confirming that neighborhood-wide deterrence extends even to contexts without an immediately adjacent sensitive facility.

Appendix Figure A5 reports the corresponding estimates for unique visitors; the directional patterns are broadly consistent across all four panels.

5.5 Visitor Composition: Who Responds to Enforcement?

The preceding sections establish how much foot traffic falls and across which types of locations. The aggregate visit count does not reveal who is changing behavior. This distinction matters for mechanism interpretation: if the behavioral response is concentrated among Hispanic households, it is consistent with a targeted ambient-threat operating through perceived deportation risk; if non-Hispanic residents also withdraw, it implies broader neighborhood-level deterrence extending beyond the directly threatened population.

To decompose visit counts by visitor origin, we exploit Advan's visitor home-location data, which record the number of weekly visits to each POI from each census block group (CBG). We characterize each CBG's racial composition using 2020–2024 five-year American Community Survey estimates. A CBG is classified as Hispanic-concentrated if the Hispanic population share exceeds 50 percent; as predominantly White (non-Hispanic) if the non-Hispanic White share exceeds 50 percent; as Asian-concentrated if the Asian share exceeds 50 percent; and as predominantly Black (non-Hispanic) if the non-Hispanic Black share exceeds 50 percent. CBGs that do not meet the 50 percent threshold for any single group are excluded as mixed. For each POI-week cell, we aggregate visit counts from CBGs in each category and rerun the baseline event-study specification.

[Figure 11 about here]

Figure 11 reports weekly event-study estimates separately by CBG racial composition, using total visits as the outcome. Visits from Hispanic-concentrated CBGs decline by a statistically significant 1.05 percent. Visits from Asian-concentrated CBGs decline by 1.02 percent but are imprecisely estimated. Visits from predominantly White CBGs decline by a statistically significant 2.83 percent. Visits from predominantly Black CBGs yield an imprecise estimate,

reflecting the small number of Black-majority CBGs in the sample cities. For reference, visits from non-Hispanic CBGs as a group decline by a statistically significant 2.23 percent. Pre-event coefficients are close to zero across all panels.

[Figure 12 about here]

Figure 12 replicates the analysis using unique visitors as the outcome. Unique visitors from Hispanic-concentrated CBGs decline by 1.05 percent. Unique visitors from predominantly White CBGs decline by 0.90 percent, marginally significant. Unique visitors from non-Hispanic CBGs decline by 0.75 percent. Estimates for Asian and Black CBGs remain imprecise. The visitor-based results broadly reinforce the visit-based patterns, though with wider standard errors that reflect the smaller variation in distinct individuals relative to total visit counts.

The withdrawal among visitors from Hispanic-concentrated CBGs is consistent with the directly targeted population responding to elevated personal exposure risk—an expected response given the enforcement operations' targeting patterns. The central finding is the large and precisely estimated decline in visits from predominantly White CBGs. Because White non-Hispanic residents face no direct deportation risk, their significant behavioral response confirms that enforcement-induced deterrence extends well beyond the population with direct immigration exposure. This is consistent with the ambient-threat mechanism: broad community awareness of local enforcement activity suppresses mobility across the neighborhood regardless of immigration status.

5.6 Geographic Substitution: Do Workers and Consumers Relocate?

The estimates in Sections 5.1–5.5 measure foot traffic at treated POIs relative to nearby control POIs within the same event radius. A natural concern is that observed declines reflect geographic displacement rather than genuine withdrawal: workers who avoid a treated establishment may shift to a similar untreated workplace elsewhere in the county; parents may redirect children to an education facility outside the affected area; patients may seek care at a more distant clinic. Under pure displacement, aggregate county-level activity would be unaffected, and our estimates would overstate the true social cost of enforcement.

We address this concern by estimating whether enforcement operations generate positive spillovers to untreated POIs of the same functional type located outside the three-mile

estimation window but within the same county. For each of four domain-specific subsamples, we measure the post-event effect on same-type POIs elsewhere in the county, controlling for POI fixed effects and county×week fixed effects. A positive spillover coefficient is consistent with geographic substitution; a null or negative coefficient indicates genuine withdrawal.

Appendix Figure A6 reports spillover estimates for all four domains using visits as the outcome, and Appendix Figure A7 reports the corresponding unique-visitor estimates. Across both outcomes and all four domains, the post-event spillover coefficients are statistically indistinguishable from zero. These uniformly null results indicate that the foot-traffic declines documented in earlier sections reflect genuine withdrawal from economic and social activity, not geographic reallocation. Workers do not commute to more distant establishments; students are not transferred to unaffected education facilities; patients do not seek care at alternative facilities; consumers do not shift patronage elsewhere in the county. The null displacement pattern across all domains corroborates the persistence of the event-study declines and is consistent with the ambient-threat interpretation: the behavioral response is a genuine contraction of activity rather than a spatial redistribution.

6 Policy Rationales: Formal Labor Demand and Local Crime

The preceding section documents that enforcement imposes a broad behavioral disruption on surrounding neighborhoods: foot traffic falls, school attendance declines, and labor supply contracts, with effects persisting across the full seven-week observation window. Whether enforcement simultaneously produces benefits that might offset these costs is an empirical question the foot-traffic data cannot answer. Two such benefits are prominently featured in policy discussions of interior enforcement. First, enforcement is commonly presented as expanding employment opportunities for workers legally authorized to work; we assess this by examining formal job postings, which capture the hiring channel available to the legally authorized workforce—native-born and foreign-born alike—but inaccessible to undocumented workers who cannot apply to career-site vacancies. Second, reducing the undocumented population is expected to lower local crime rates. We examine each of these margins in turn, using independent data sources and identification strategies as closely parallel to the main analysis as the data allow.

6.1 Formal Labor Demand

6.1.1 Empirical Strategy

Using the job-postings data described in Section 3, we aggregate vacancy counts to a city-week panel of total, high-skill, low-skill, and other postings. Because LinkUp's finest geographic identifier is the city, the analysis necessarily operates at a coarser spatial resolution than the POI-level panels used in the main analysis. At that scale, the relevant comparison is between cities rather than between establishments, and cities within the same county can differ substantially in their demographic composition, labor market structure, and immigrant population. A credible research design therefore requires control cities that are comparable to treated cities along these dimensions.

We construct a stacked city-week sample in which the identifying shock is the first ICE raid observed in a given city during our sample period. For each treated city, we draw the control group from never-treated cities in the same county—ensuring geographic comparability and shared institutional environment—and then apply propensity score matching to select among those candidates. Specifically, we estimate the probability of experiencing an ICE en-

forcement event as a function of 2022 American Community Survey city-level covariates: log population, median household income, poverty rate, Hispanic share, foreign-born share, unemployment rate, and college share. We implement 1:1 nearest-neighbor matching on the logit propensity score within a caliper of 0.25 standard deviations; treated cities with no within-caliper match are excluded to maintain common support. Covariate balance before and after matching is reported in Appendix Table A1. Because the postings represent formal hiring channels that undocumented workers generally cannot access, a null effect on formal postings is consistent with any labor market adjustment flowing through informal arrangements unobservable in our data, not only with zero employment effects.

Event time is defined relative to the treated city's first-raid week and is binned to the window $[-5, 7]$, with endpoints pooling all earlier or later observations. The estimating equation is

$$\text{JobPosts}_{c,t,s} = \sum_{\tau=-5}^{7} \delta_\tau \cdot \mathbf{1}[\text{Treated}_{c,s}] \cdot \mathbf{1}[t - t_s^* = \tau] + \mu_c + \lambda_{d(c),t} + \varepsilon_{c,t,s}, \tag{4}$$

where c indexes cities, t indexes week-of-sample, s indexes stacks, μ_c are city fixed effects, and $\lambda_{d(c),t}$ are county-by-week fixed effects that absorb common shocks to all cities within a county. We estimate equation (4) by OLS. Standard errors are clustered at the city level.

Because the treated city's first raid is the event, the event-study coefficients δ_τ capture the dynamics of entering an enforcement regime and subsume any subsequent raids in that city. To examine whether the ongoing intensity of enforcement within a city is separately associated with posting activity, we complement the event study with a regression of weekly posting counts on the contemporaneous weekly raid count within the matched stacked sample, controlling for city and county-by-week fixed effects. This specification exploits within-city, within-county variation in enforcement frequency against a matched comparison group; it characterizes a conditional correlation rather than a causal effect. Both the event study and the raid-count regression speak to whether the formal hiring channel responds to enforcement along either its extensive margin (onset of the enforcement regime) or its intensive margin (frequency of subsequent raids).

6.1.2 Results

Figure 13 presents event-study estimates for total job postings (Panel A), high-skill postings (Panel B), low-skill postings (Panel C), and other postings (Panel D). Pre-event coefficients are small and statistically indistinguishable from zero across all four panels, consistent with the parallel trends assumption and with the comparability of treated and matched control cities documented in Appendix Table A1. In the post-event window, no panel displays a significant departure from zero at any event-time bin. The confidence intervals are narrow enough to rule out economically meaningful increases in posting activity following enforcement. There is no evidence that entering an enforcement regime produces a systematic expansion of formal labor demand in any skill category, including the low-skill vacancies most directly tied to occupations with high immigrant labor force shares.

[Figure 13 about here]

The event-study design captures the effect of the onset of enforcement and subsumes all subsequent raids within the post-period. Appendix Table A2 reports a supplementary exercise that asks whether week-to-week variation in raid frequency within a city is associated with contemporaneous posting activity. Across all four skill categories, the estimated coefficients are small in magnitude and statistically indistinguishable from zero. As noted above, this specification does not admit a pre-period placebo test and should be read as descriptive; it is nonetheless consistent with the event-study evidence in suggesting no systematic relationship between enforcement activity and formal hiring.

Together, these results indicate that enforcement is not associated with a measurable increase in formal labor demand. The null is consistent with two mechanisms: employers may respond to reduced availability of unauthorized workers by contracting output or hours rather than shifting to formal recruitment; or reallocation may occur through informal arrangements—direct recruitment, labor brokers, word-of-mouth hiring—that are by construction absent from career-site postings.

6.2 Local Crime Rates

A second commonly cited benefit of interior enforcement is the expectation that it reduces local crime. Whether immigration affects local crime rates has been studied extensively across countries and time periods; the broad conclusion is that immigrants, including the unautho-

rized, are incarcerated at lower rates than comparable native-born individuals and that immigrant inflows do not systematically raise local crime (Marie and Pinotti, 2024; Abramitzky et al., 2024). The question here is narrower: whether a specific enforcement shock—an ICE raid—is followed by a reduction in reported crime in the surrounding neighborhood in its immediate aftermath. We examine this using geocoded incident-level crime records and a design that mirrors the main analysis.

6.2.1 Empirical Strategy

Using the crime incident data described in Section 3, we aggregate incident counts to the census block-week level, separately for total crime, violent crime, and non-violent crime. For each ICE enforcement event in our six-jurisdiction sample, we apply the same hyperlocal stacked design used in the foot-traffic analysis. Census blocks within 0.5 miles of the event coordinate are assigned to the treated group; blocks 0.5 to three miles from the event serve as controls. Event time is defined relative to the event week, and we study a window of four pre-event and seven post-event weeks. The estimating equation is analogous to equation (2), with block-week crime counts replacing foot-traffic outcomes. All specifications include block×stack, week×census-tract, and week×crime-category fixed effects; standard errors are clustered at the census-tract level. The identifying assumption is that treated and control blocks within the same neighborhood would have followed parallel trends in crime rates absent the enforcement event—the same assumption maintained in the main analysis.

6.2.2 Results

[Figure 14 about here]

We find no statistically significant effect of ICE enforcement events on total crime, violent crime, or non-violent crime in the surrounding neighborhood.[16] Pre-event coefficients are flat in all specifications. Post-event coefficients are small in magnitude and statistically indistinguishable from zero throughout the seven-week observation window, for both violent and non-violent outcomes.

The absence of a crime-reduction effect, placed alongside the substantial behavioral disruption documented in Section 5, implies that the enforcement events in our sample impose

[16] [PLACEHOLDER: Precise estimates, pre-event means, and confidence interval bounds to be reported upon finalization of city-level data.]

measurable costs on the surrounding community without generating a detectable public safety return. This result is consistent with the broader evidence that immigrants are incarcerated at lower rates than comparable native-born individuals and that immigration does not raise local crime rates (Marie and Pinotti, 2024; Abramitzky et al., 2024).

7 Social Costs per Enforcement Event

The reduced-form estimates in Section 5 document a persistent, community-wide behavioral retreat from neighborhoods where ICE has recently operated. Workers stay home from factories and construction sites, parents keep children away from education facilities, and families avoid local shops—yet none of these absences appear in standard enforcement accounting, which records only administrative costs. The analyses in Section 6 find no evidence that enforcement generates measurable gains along the two dimensions most commonly cited in its favor: formal job postings do not rise, and local crime rates do not fall. This section presents a conservative accounting exercise that maps the estimated activity declines into lower-bound dollar losses and compares the resulting community-side burden to the government's reported expenditure per removal.

7.1 Accounting Setup

The event-study design identifies, for each behavioral channel $k \in \{w,\ c,\ s\}$ (labor supply, consumer demand, and education), the proportional decline in activity at affected establishments relative to the pre-event baseline. Denote this estimated decline $\hat{\beta}^k$ and the pre-event baseline activity per week $\bar{Y}^k$; then $\hat{\beta}^k \cdot \bar{Y}^k$ is the estimated weekly activity loss per establishment. Assigning a conservative dollar value v^k per foregone unit of activity and summing over a T-week horizon, the dollar cost attributable to channel k per affected establishment is:

$$C_k \;=\; \hat{\beta}^k \;\cdot\; \bar{Y}^k \;\cdot\; v^k \;\cdot\; T. \tag{5}$$

We set $T = 4$ weeks as the accounting horizon, which corresponds to one calendar month and is conservative: the event study documents no recovery in the seven-week observation window, so costs persist beyond this cutoff. Each dollar value v^k is drawn from an established economic literature and calibrated conservatively, as detailed in Section 7.2. Table 4 summarizes the inputs and results for all three channels.

[Table 4 about here]

These estimates are partial-equilibrium lower bounds on the community-side burden. They measure foregone activity within the observed channels and do not capture general-equilibrium spillovers to nearby firms, peers, or local public finance. Because the three chan-

nels may partially overlap, we present margin-specific estimates rather than a grand total; the appropriate inference is the magnitude of disruption within each channel, not their sum. Healthcare establishments are excluded from this accounting because Section 5.3 finds no statistically significant decline in visits to healthcare POIs; enforcement does not detectably suppress neighborhood foot traffic near such facilities (Section 5.4), and facility-level utilization does not fall detectably, consistent with patients facing acute need being less responsive to ambient threat signals.

The framework applies to other localized threat events whenever high-frequency spatial data permit identification of activity declines by location type and conservative dollar values can be sourced from established literatures. The geographic substitution analysis in Section 5 supports the interpretation that the observed foot-traffic declines represent net activity losses rather than spatial displacement: untreated establishments of the same type in the same county show no offsetting gain. Substitution across county boundaries cannot be ruled out, but the persistence of effects over seven weeks—longer than typical commuting or school-transfer adjustment horizons—suggests withdrawal is the dominant margin.

7.2 Channel Estimates

Labor supply. Establishments in industries where workers predominantly constitute foot traffic—including manufacturing, construction, agriculture, and warehousing—lose a statistically significant 1.32 percent of weekly unique visitors relative to a pre-event visitor mean of 1,674.1 per week. Because worker arrivals account for the majority of unique-visitor counts at these establishments, the unique-visitor outcome provides a more direct approximation of workforce attendance changes than total visit counts, which may also reflect changing trip frequencies among continuing visitors. Each absent worker-week is valued at $v^w = \$600$, applying the minimum statutory wage of \$15.00—the floor in Illinois, below California's \$16.90 and Minnesota's \$10.85 blended rate—times 40 hours. This is a lower bound on the foregone labor income: median usual weekly earnings for construction workers in our sample states range from approximately \$950 in Texas to over \$1,200 in California (Bureau of Labor Statistics, 2025), 58 to 100 percent above the floor applied here. The implied cost per affected establishment over the one-month window is

$$C_w \;=\; 0.0132 \;\times\; 1{,}674.1 \;\times\; \$600 \;\times\; 4 \;=\; \$53{,}197.$$

Local business revenue. Establishments primarily serving walk-in customers—including retail stores, restaurants, and grocery outlets—lose a statistically significant 1.02 percent of total weekly visits relative to a pre-event mean of 1,352.7. Foot traffic at these locations is composed primarily of customers; on-site workers account for a small and largely stable share of recorded visits. We measure this channel as lost local business revenue rather than as a welfare loss: each foregone visit represents a transaction that does not occur at the affected establishment, valued at $v^c = \$50$ per trip—the approximate average per-trip expenditure at food and retail establishments (BLS Consumer Expenditure Survey, 2024). The estimated revenue loss per establishment over the one-month window is

$$C_c = 0.0102 \times 1{,}352.7 \times \$50 \times 4 = \$2{,}760.$$

To the extent that some spending is reallocated to unaffected establishments rather than foregone entirely, this figure overstates the net revenue loss; we therefore treat it as an indicative bound on local business disruption rather than a welfare estimate.

Education. Education facility POIs lose a statistically significant 4.82 percent of total weekly visits relative to a pre-event mean of 1,573.7, and a statistically significant 1.63 percent of weekly unique visitors relative to a pre-event mean of 1,377.7. Because the welfare-relevant quantity is the number of distinct individuals who forego a school visit—primarily students who miss instructional time—we use the unique-visitor estimate for the cost calculation. The unique-visitor count measures the number of distinct individuals who did not appear at the facility in a given week, without double-counting individuals who would otherwise have made multiple visits. We value each absent school week using the private return to schooling from Card (1999): one year of schooling raises annual earnings by approximately 7 to 10 percent; at the lower end of this range ($\rho = 7\%$) and a mean annual wage of $50,000 in high-immigration counties across our sample states (Bureau of Labor Statistics QCEW, 2024), the first-year earnings return per school week is $0.07 \times \$50{,}000 \div 36$ weeks $= \$97.22$. Capitalizing this annual earnings premium at a 5 percent discount rate over a 35-year career yields a present-value multiplier of $\sum_{j=1}^{35}(1.05)^{-j} \approx 16$; the lifetime human capital value of one foregone school week is therefore $v^s = \$97.22 \times 16 \approx \$1{,}556$. The implied human capital cost per affected

education facility over the one-month window is

$$C_s = 0.0163 \times 1{,}377.7 \times \$1{,}556 \times 4 = \$139{,}712.$$

Social returns to education further exceed private returns (Moretti, 2004), so this estimate remains a lower bound on the full social cost of forgone attendance.

The estimates also omit additional margins of community burden that the foot-traffic data cannot capture: psychological distress, family disruption, the indirect health effects of deferred care documented in prior work (Alsan and Wanamaker, 2018), and general-equilibrium spillovers to peer firms and local public finance. We interpret each channel estimate as a partial-equilibrium lower bound tied to the measured behavioral responses; taken together they constitute a conservative floor on community-side costs rather than a complete welfare accounting.

7.3 Comparison to Fiscal Enforcement Costs

The Department of Homeland Security reports $17,121 as the full average fiscal cost per person arrested, detained, and removed (U.S. Department of Homeland Security, 2025). This comparison is illustrative rather than a benefit-cost ratio: it contrasts the government's fiscal cost per removal with the community-side burden generated at a single affected establishment over the same calendar month. The labor income loss at one worker-intensive establishment ($53,197) is approximately 3.1 times the DHS per-person figure; the human capital cost at one education facility ($139,712) is approximately 8.2 times; and the local business revenue loss per customer-facing establishment ($2,760) is roughly one-sixth. Summing across the three channels, the per-establishment community-side burden totals $195,669 over one month—approximately 11.4 times the DHS per-removal benchmark. Each enforcement event affects multiple establishments of each type within the half-mile radius simultaneously, so the community-side burden in aggregate scales with the local density of economic activity—these per-establishment figures are therefore themselves lower bounds on the total event-level impact.

7.4 Comparison to Non-Causal City-Level Assessments

The per-establishment estimates above can be benchmarked against a city-level assessment that independently attempts to quantify the same phenomenon. The City of Minneapolis released a preliminary economic impact report on Operation Metro Surge, estimating at least $203.1 million in total losses during January 2026, including $47 million in lost wages and $81 million in small-business revenue losses (City of Minneapolis, 2026).[17] Understanding how they arrive at these figures clarifies both the strengths of their approach and the identification challenges it cannot resolve.

The wage estimate follows a top-down accounting chain. Using American Community Survey data, the report calculates the total monthly wage bill of the foreign-born workforce in Minneapolis at approximately $112.9 million. Drawing on community surveys, it then assumes that approximately 42 percent of this wage income was not received in January—through a combination of workers not showing up, reducing hours, or exiting the labor force entirely—yielding $0.42 \times \$112.9\,\text{M} \approx \$47$ million. The business revenue estimate follows a complementary bottom-up chain: 82 business owners were surveyed about revenue losses, and their responses were extrapolated to the full population of approximately 2,350 affected establishments citywide to obtain $81 million.

The critical limitation of both calculations is the absence of a counterfactual. The 42 percent wage-reduction rate is derived from survey responses rather than from observed payroll or employment data; without an untreated comparison month or comparison group of workers, it is impossible to determine whether reported income shortfalls reflect the enforcement operation, seasonal patterns in January, or measurement error in self-reported earnings. Similarly, the extrapolation from 82 surveyed businesses to 2,350 establishments rests on a sampling fraction of roughly 3.5 percent; if survey respondents are disproportionately businesses that experienced larger disruptions—a plausible form of participation bias—the extrapolated total overstates average losses. The authors acknowledge these limitations and describe their figures as preliminary.

Our approach is methodologically complementary. Rather than assuming a reduction rate, we measure the enforcement-induced change in foot traffic directly, relative to nearby establishments that experienced no enforcement—thus isolating the causal effect from sea-

[17] City of Minneapolis, *Preliminary Impact Assessment and Relief Needs: Operation Metro Surge*, February 2026.

sonality and other concurrent trends. Rather than extrapolating from a small convenience sample, we observe activity at the POI level across thousands of establishments. The tradeoff is unit of analysis: our estimates are per establishment per event, whereas the Minneapolis report aggregates over an entire city-wide operation spanning many simultaneous enforcement actions.

To bridge the two levels, consider a back-of-envelope scaling exercise. A typical urban half-mile enforcement radius in our sample contains on the order of tens of worker-intensive establishments, one to two education facilities, and tens of customer-facing businesses. Applying our per-establishment estimates—$53,197 per worker-intensive establishment, $139,712 per education facility, and $2,760 per commercial establishment—to these densities yields an event-level community-side cost in the range of $1 to $3 million per enforcement event per month. Operation Metro Surge involved hundreds of enforcement actions across the Minneapolis metropolitan area over January 2026; at these per-event rates, the aggregate cost accumulates to tens or hundreds of millions of dollars, broadly consistent with the Minneapolis report's order of magnitude despite the two calculations using entirely different methodologies and data sources. The convergence is informative: it suggests that neither approach is generating implausibly large or small numbers, and that the true community-side burden of large-scale urban enforcement operations is very likely in the hundreds of millions over a single month of sustained activity.

8 Conclusion

This paper provides the first causal estimates of how ICE enforcement operations affect the full spectrum of neighborhood economic and social activity. The central finding is that enforcement imposes a persistent, community-wide behavioral shock: foot traffic within 0.5 miles of an event falls by a precisely estimated 1.69 percent on average and does not recover over seven weeks. The results are consistent with information diffusion as the primary channel rather than direct confrontation: non-contact enforcement generates a 1.30 percent decline, broadly comparable to the 2.23 percent decline from contact enforcement, and high-force and low-force deployments produce statistically indistinguishable responses—1.69 percent and 1.74 percent, respectively—indicating that neither direct confrontation nor operational scale is required for community-wide deterrence to materialize.

The behavioral costs are concentrated at education facilities and workplaces. Visits to education facilities fall 4.82 percent and unique visitors fall 1.63 percent—both approximately three times the baseline—with no evidence of recovery over seven weeks. Among workplaces, enforcement suppresses labor supply and consumer demand simultaneously: worker-intensive establishments decline by 2.76 percent and consumer-facing establishments by 1.02 percent. The response is broad rather than targeted: foot traffic falls across both immigrant-workforce-intensive and native-workforce-intensive industries (2.52 and 3.19 percent, respectively, under NAICS-based classifications), and visits from predominantly White CBGs decline by 2.83 percent, confirming that deterrence extends to residents with no direct immigration exposure. Healthcare establishments show no statistically significant decline. Conservative lower-bound estimates place the labor supply loss at a single worker-intensive establishment at $53,197 per month—approximately 3.1 times the DHS per-person cost of $17,121 (U.S. Department of Homeland Security, 2025)—and the human capital cost at a single education facility at $139,712, approximately 8.2 times the DHS figure. Local business revenue losses add $2,760 per consumer-facing establishment. Summing the three channels, the per-establishment community-side burden totals $195,669—approximately 11.4 times the DHS benchmark—with each enforcement event simultaneously affecting multiple establishments within the half-mile radius.

Analyses in Section 6 find no detectable response in formal job postings and no reduction in local crime rates around enforcement events, suggesting that the community-side costs

documented here are not offset by gains along the margins most commonly advanced in enforcement policy discussions.

Our analysis has three principal limitations. Although the sample spans 10,018 events across six states (CA, IL, MN, NY, OR, TX), reporting intensity in ICEout may still vary across communities in ways that affect estimates. The seven-week observation window precludes inference about long-run neighborhood adjustment. And the cost estimates in Section 7 cover only three foot-traffic-measurable channels (labor supply, consumer revenue, and education), omitting healthcare (where facility-level utilization does not fall detectably), housing, cumulative human capital effects, psychological distress, and general-equilibrium spillovers.

The findings carry direct implications for enforcement policy. The evidence is consistent with disruption propagating primarily through community information networks rather than through direct operational contact: both contact and non-contact operations, and both high-force and low-force deployments, generate substantial and comparable behavioral responses. Enforcement near education facilities—now permitted following the January 2025 rescission of the sensitive locations policy—is associated with facility-level visit declines approximately three times the baseline; the full scope of that disruption, and its longer-run consequences for student outcomes, warrants continued investigation. Using a proximity-based classification, we find that raids within 0.1 mile of education facilities, healthcare establishments, and workplaces each generate significant neighborhood-wide foot-traffic declines of 1.7–2.0 percent—comparable in magnitude to the overall baseline—suggesting that enforcement near sensitive locations amplifies disruption broadly rather than solely at the targeted facility type. Any assessment of enforcement policy that counts only administrative costs is likely to understate its societal burden.

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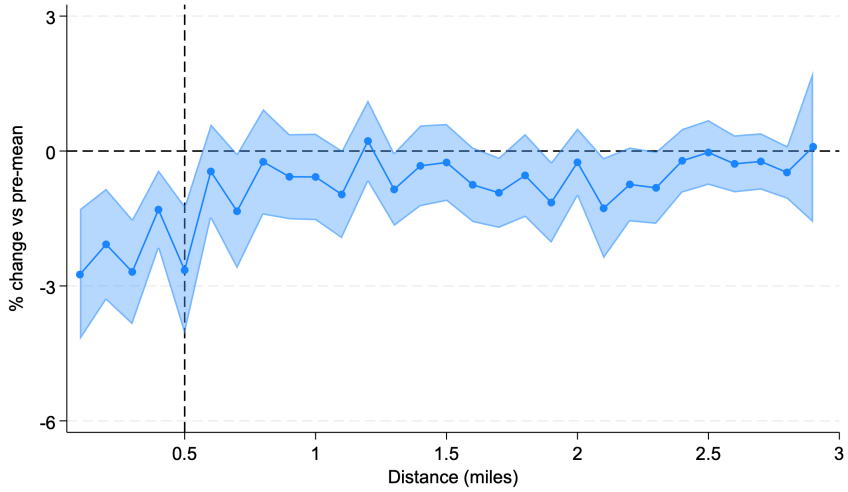
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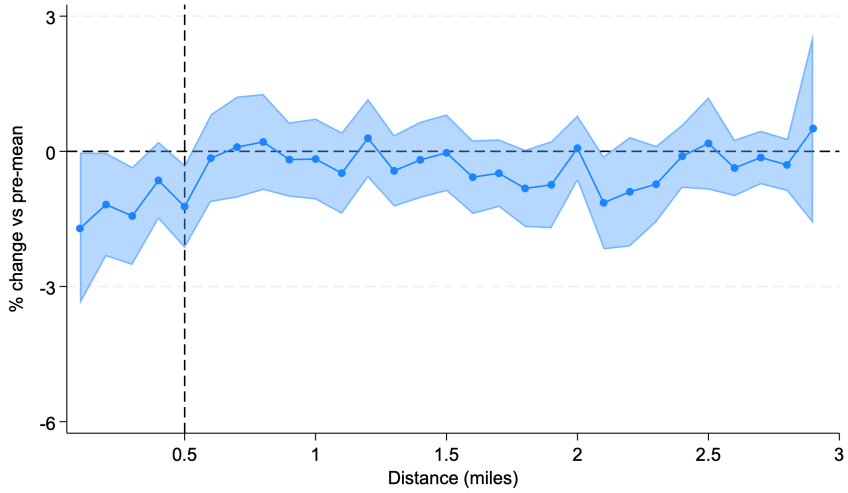
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Figures



(a) Total visits (weekly)

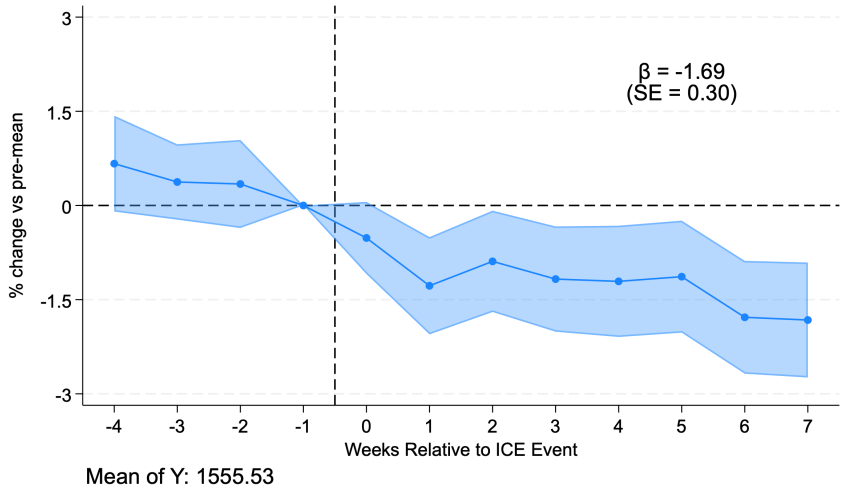


(b) Unique visitors (weekly)

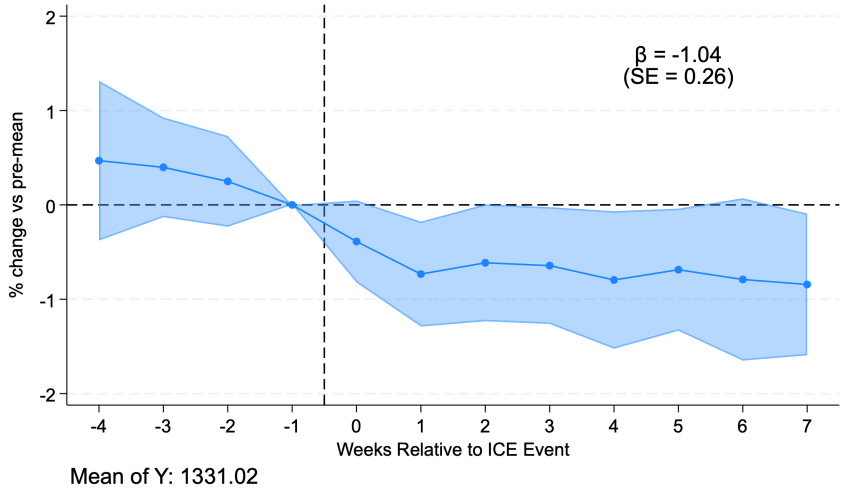
Figure 1: Spatial Decay of ICE Enforcement Effects on Foot Traffic

Notes: This figure reports estimates from the distance-decay specification in equation (1), expressed as percent changes. POIs are grouped into 0.1-mile distance bins relative to the event location, with the 2.9–3.0 mile bin as the reference category. Panel (a) uses total visits (weekly), and Panel (b) uses unique visitors (weekly). All specifications include POI×stack, week×stack, and pre-period visit quartile×week fixed effects. Standard errors are clustered at the census-tract level. Shaded areas denote 95% confidence intervals.

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(a) Total visits (weekly)

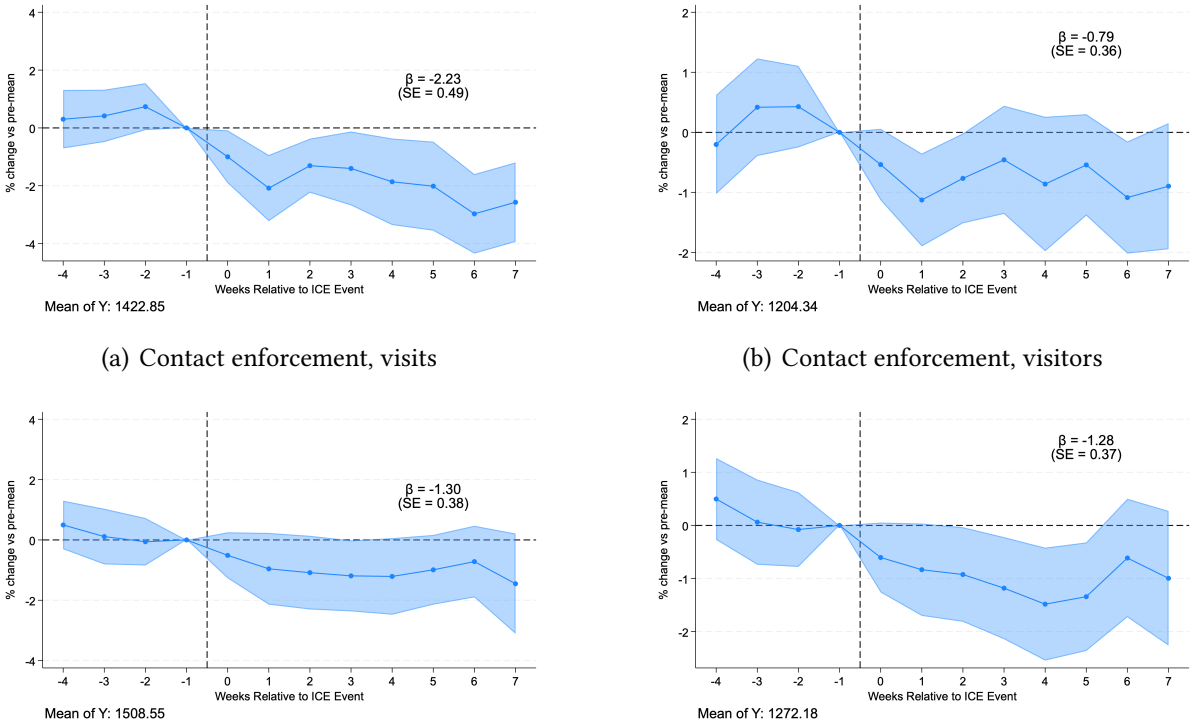


(b) Unique visitors (weekly)

Figure 2: Baseline Weekly Event-Study Estimates

Notes: This figure reports weekly event-study estimates from equation (2), expressed as percent changes. Treated POIs are those within 0.5 miles of the event location, while control POIs are between 0.5 and 3 miles. Coefficients are normalized to zero in event week −1. Panel (a) uses total visits (weekly), and Panel (b) uses unique visitors (weekly). All specifications include POI×stack, week×stack, and pre-period visit quartile×week fixed effects. Outcomes are normalized by the pre-treatment mean among treated POIs, so coefficients represent percent changes. Standard errors are clustered at the census-tract level. Shaded areas denote 95% confidence intervals. "Mean of Y" reports the pre-event mean for treated POIs.

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(a) Contact enforcement, visits

(b) Contact enforcement, visitors

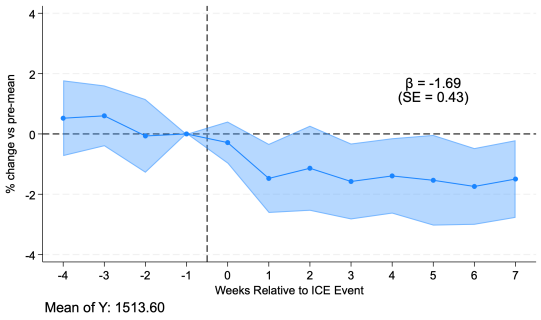
(c) Non-contact enforcement, visits

(d) Non-contact enforcement, visitors

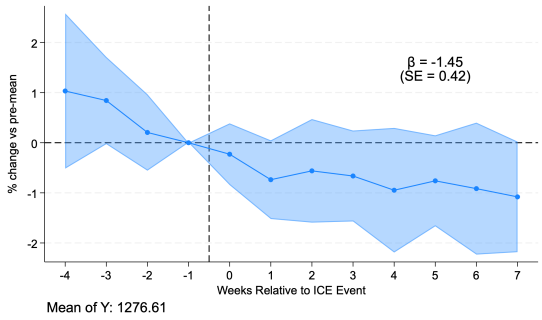
Figure 3: Event-Study Estimates by Operation Type: Contact versus Non-Contact Enforcement

Notes: This figure estimates the baseline event-study specification separately by operation type, expressed as percent changes. Contact enforcement, shown in panels (a) and (b), includes events such as arrests, raids, use of force, and active questioning. Non-contact enforcement, shown in panels (c) and (d), includes events such as passive presence, surveillance patrols, and stationary observation. Panels (a) and (c) use total visits (weekly), while panels (b) and (d) use unique visitors (weekly). Treated POIs are those within 0.5 miles of the event location, while control POIs are between 0.5 and 3 miles. Coefficients are normalized to zero in event week -1. All specifications include POI$\times$stack, week$\times$stack, and pre-period visit quartile$\times$week fixed effects. Outcomes are normalized by the pre-treatment mean within each subsample, so coefficients represent percent changes. Standard errors are clustered at the census-tract level. Shaded areas denote 95% confidence intervals. "Mean of Y" reports the subsample-specific pre-event mean.

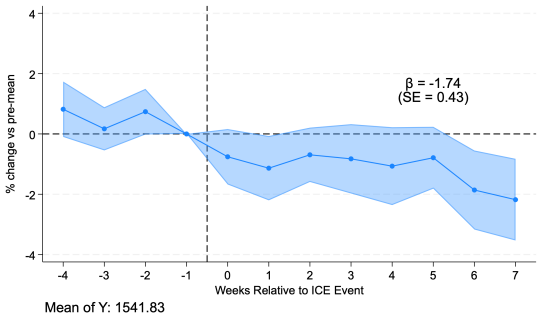
[Back to page 25]



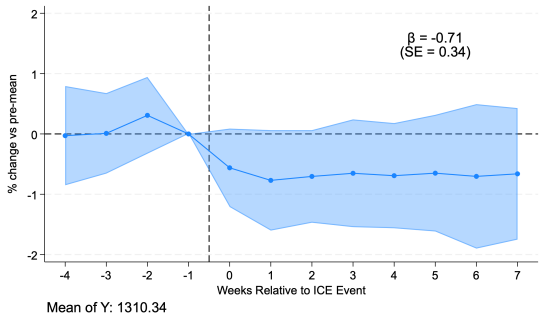
(a) High-force enforcement, visits



(b) High-force enforcement, visitors



(c) Low-force enforcement, visits

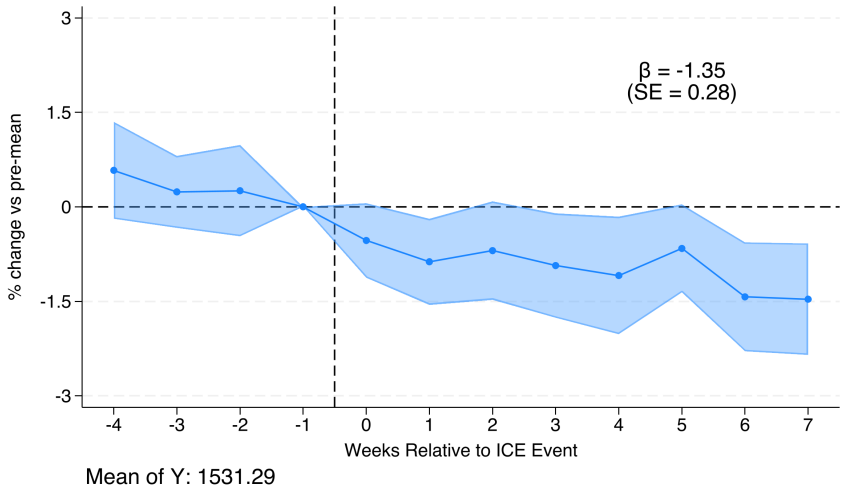


(d) Low-force enforcement, visitors

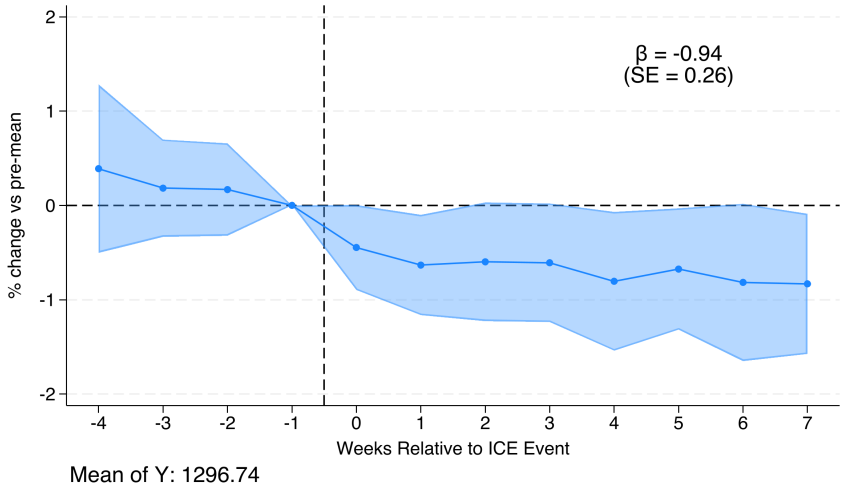
Figure 4: Event-Study Estimates by Force Scale

Notes: This figure estimates the baseline event-study specification separately by operation scale, expressed as percent changes. High-force operations, shown in panels (a) and (b), are events involving more than seven officials, while low-force operations, shown in panels (c) and (d), are events involving seven or fewer officials. Panels (a) and (c) use total visits (weekly), while panels (b) and (d) use unique visitors (weekly). Treated POIs are those within 0.5 miles of the event location, while control POIs are between 0.5 and 3 miles. Coefficients are normalized to zero in event week -1. All specifications include POI$\times$stack, week$\times$stack, and pre-period visit quartile$\times$week fixed effects. Outcomes are normalized by the pre-treatment mean within each subsample, so coefficients represent percent changes. Standard errors are clustered at the census-tract level. Shaded areas denote 95% confidence intervals. "Mean of Y" reports the subsample-specific pre-event mean.

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(a) Total visits (weekly)

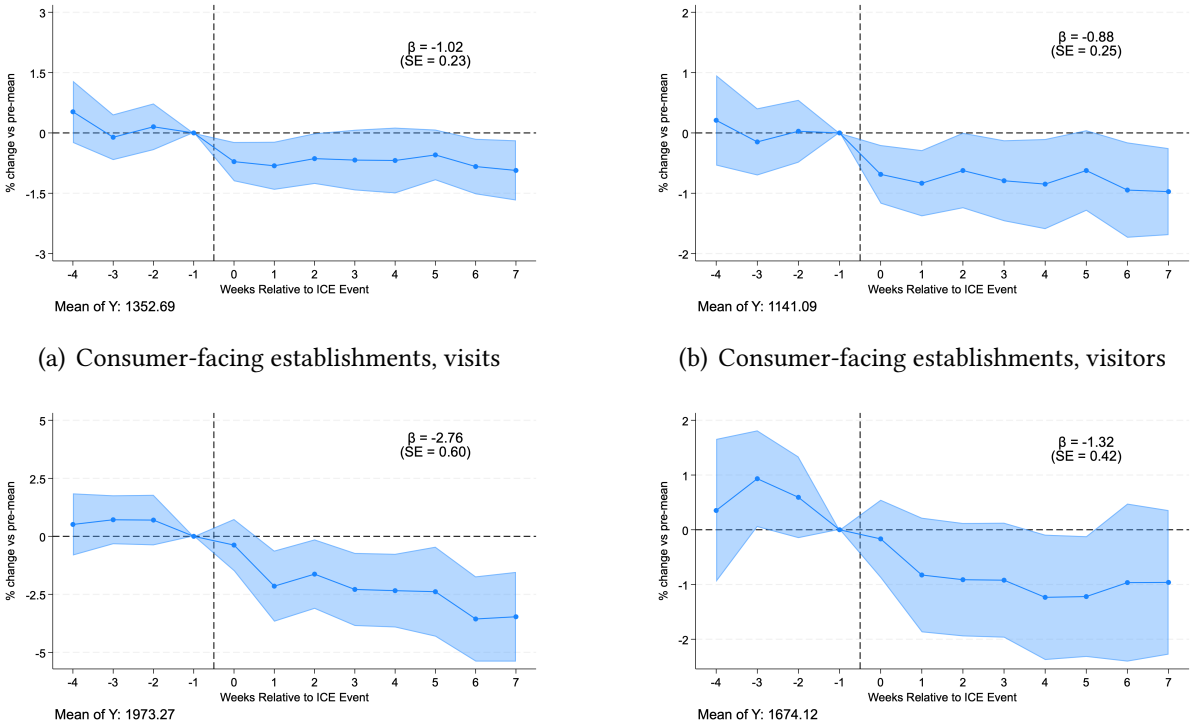


(b) Unique visitors (weekly)

Figure 5: Event-Study Estimates for Workplace POIs

Notes: This figure estimates the baseline event-study specification, restricting the sample to workplace POIs, defined as consumer-facing and worker-intensive establishments, with outcomes expressed as percent changes. Panel (a) uses total visits (weekly), while Panel (b) uses unique visitors (weekly). Treated POIs are those within 0.5 miles of the event location, while control POIs are between 0.5 and 3 miles. Coefficients are normalized to zero in event week -1. All specifications include POI$\times$stack, week$\times$stack, and pre-period visit quartile$\times$week fixed effects. Outcomes are normalized by the pre-treatment mean among treated workplace POIs, so coefficients represent percent changes. Standard errors are clustered at the census-tract level. Shaded areas denote 95% confidence intervals. "Mean of Y" reports the pre-event mean for treated workplace POIs.

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(a) Consumer-facing establishments, visits

(b) Consumer-facing establishments, visitors

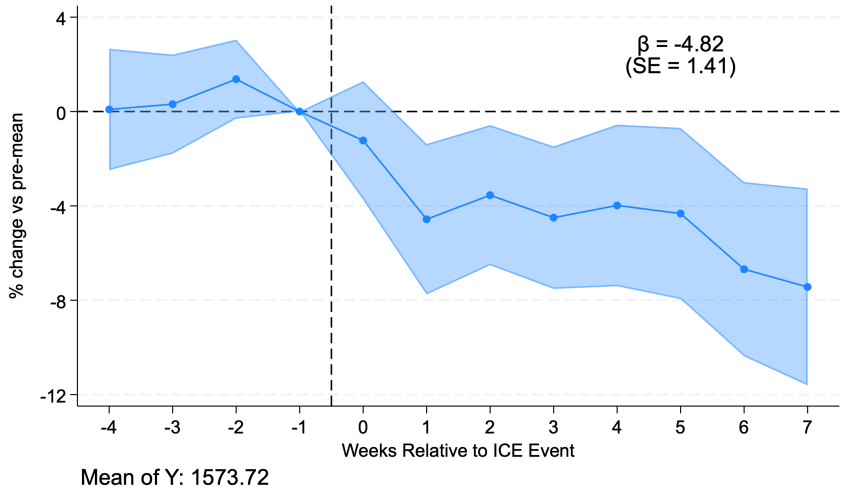
(c) Worker-intensive establishments, visits

(d) Worker-intensive establishments, visitors

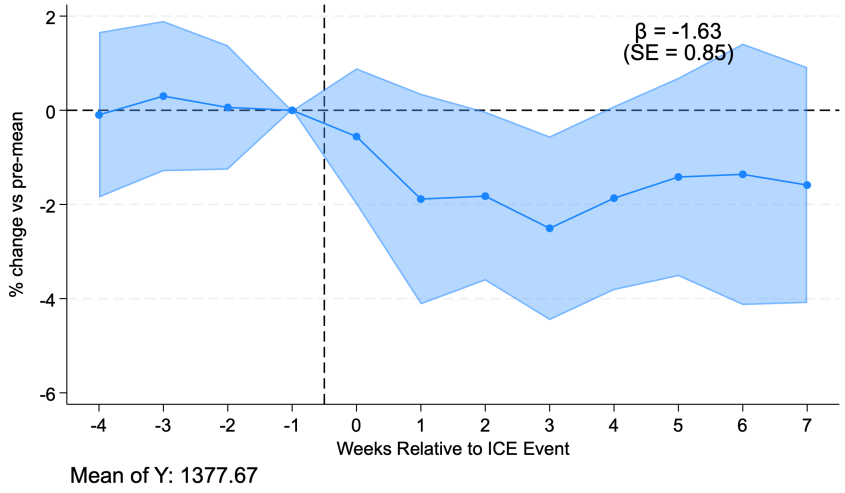
Figure 6: Event-Study Estimates by Establishment Type: Consumer-Facing vs. Worker-Intensive

Notes: This figure estimates the baseline event-study specification separately by establishment function, as defined by NAICS codes, with outcomes expressed as percent changes. Consumer-facing establishments, shown in panels (a) and (b), include sectors such as retail, groceries, restaurants, personal care, and accommodation. Worker-intensive establishments, shown in panels (c) and (d), include sectors such as manufacturing, construction, agriculture, utilities, warehousing and trucking, and administrative support. Panels (a) and (c) use total visits (weekly), while panels (b) and (d) use unique visitors (weekly). Treated POIs are those within 0.5 miles of the event location, while control POIs are between 0.5 and 3 miles. Coefficients are normalized to zero in event week -1. All specifications include POI$\times$stack, week$\times$stack, and pre-period visit quartile$\times$week fixed effects. Outcomes are normalized by the pre-treatment mean within each subsample, so coefficients represent percent changes. Standard errors are clustered at the census-tract level. Shaded areas denote 95% confidence intervals. "Mean of Y" reports the subsample-specific pre-event mean.

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(a) Total visits (weekly)

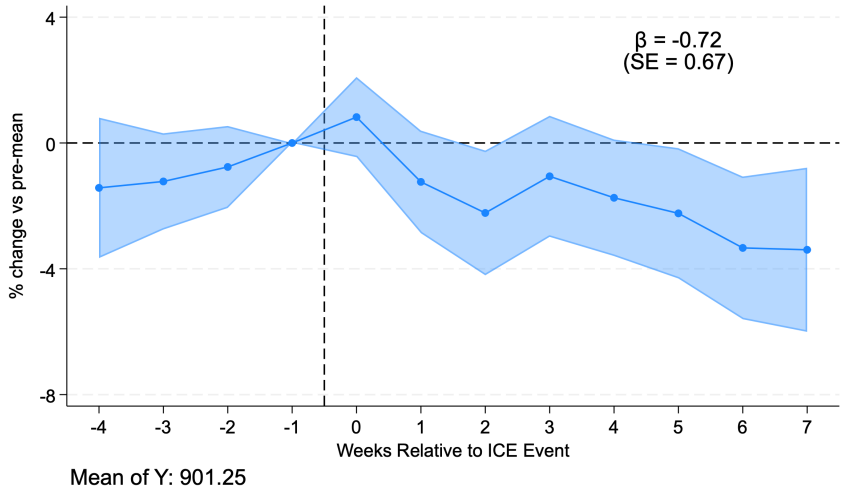


(b) Unique visitors (weekly)

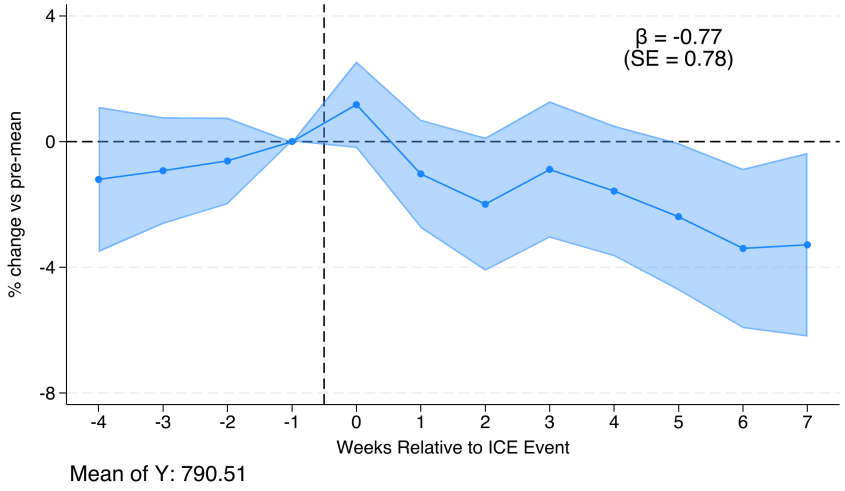
Figure 7: Event-Study Estimates for Education Facility POIs

Notes: This figure estimates the baseline event-study specification, restricting the sample to education facility POIs, including establishments such as K–12 schools, junior colleges, universities, and educational support services, with outcomes expressed as percent changes. Panel (a) uses total visits (weekly), while Panel (b) uses unique visitors (weekly). Treated POIs are those within 0.5 miles of the event location, while control POIs are between 0.5 and 3 miles. Coefficients are normalized to zero in event week −1. All specifications include POI×stack, week×stack, and pre-period visit quartile×week fixed effects. Outcomes are normalized by the pre-treatment mean among treated education facility POIs, so coefficients represent percent changes. Standard errors are clustered at the census-tract level. Shaded areas denote 95% confidence intervals. "Mean of Y" reports the pre-event mean for treated education facility POIs.

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(a) Total visits (weekly)

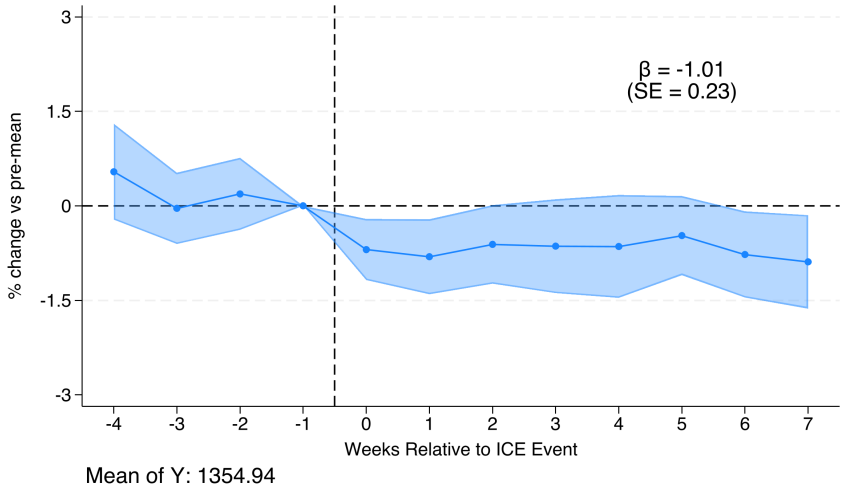


(b) Unique visitors (weekly)

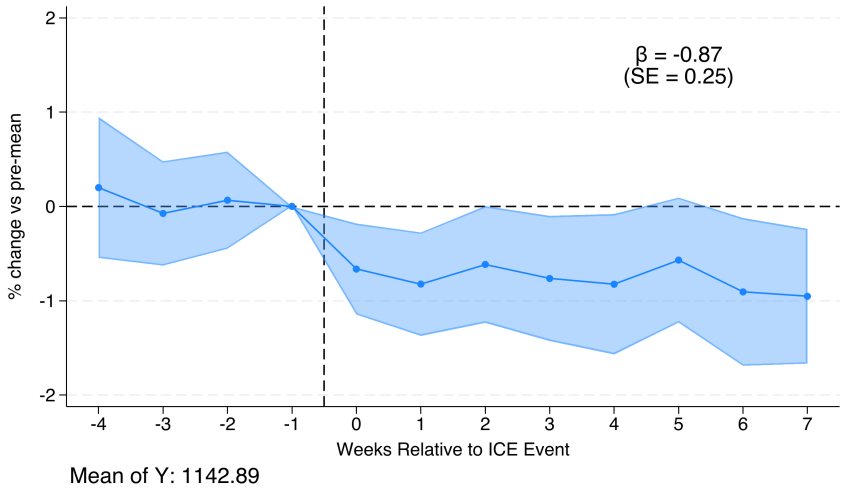
Figure 8: Event-Study Estimates for Healthcare Establishments

Notes: This figure estimates the baseline event-study specification, restricting the sample to healthcare establishments, including facilities such as clinics, pharmacies, urgent care centers, and other health-related services, with outcomes expressed as percent changes. Panel (a) uses total visits (weekly), while Panel (b) uses unique visitors (weekly). Treated POIs are those within 0.5 miles of the event location, while control POIs are between 0.5 and 3 miles. Coefficients are normalized to zero in event week -1. All specifications include POI×stack, week×stack, and pre-period visit quartile×week fixed effects. Outcomes are normalized by the pre-treatment mean among treated healthcare establishments, so coefficients represent percent changes. Standard errors are clustered at the census-tract level. Shaded areas denote 95% confidence intervals. "Mean of Y" reports the pre-event mean for treated healthcare establishments.

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(a) Total visits (weekly)

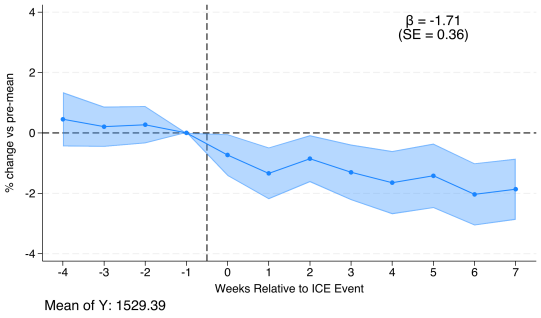


(b) Unique visitors (weekly)

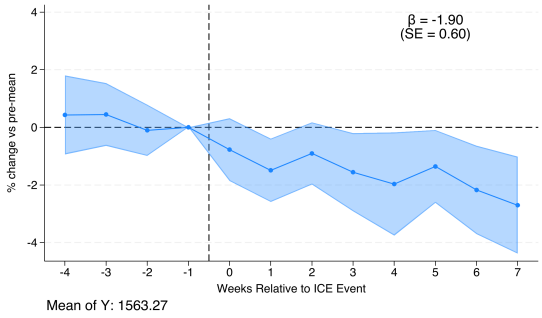
Figure 9: Event-Study Estimates for Commercial POIs

Notes: This figure estimates the baseline event-study specification, restricting the sample to commercial POIs, including establishments such as retail, restaurants, groceries, personal services, and other commerce-oriented activities, with outcomes expressed as percent changes. Panel (a) uses total visits (weekly), while Panel (b) uses unique visitors (weekly). Treated POIs are those within 0.5 miles of the event location, while control POIs are between 0.5 and 3 miles. Coefficients are normalized to zero in event week -1. All specifications include POI×stack, week×stack, and pre-period visit quartile×week fixed effects. Outcomes are normalized by the pre-treatment mean among treated commercial POIs, so coefficients represent percent changes. Standard errors are clustered at the census-tract level. Shaded areas denote 95% confidence intervals. "Mean of Y" reports the pre-event mean for treated commercial POIs.

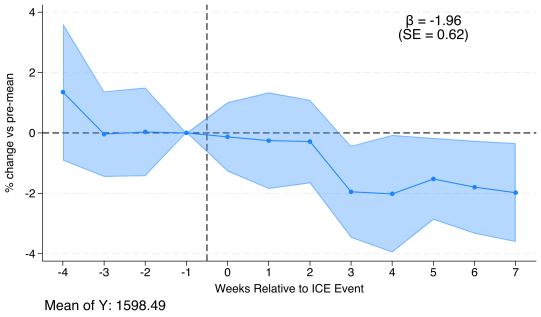
[Back to page 28]



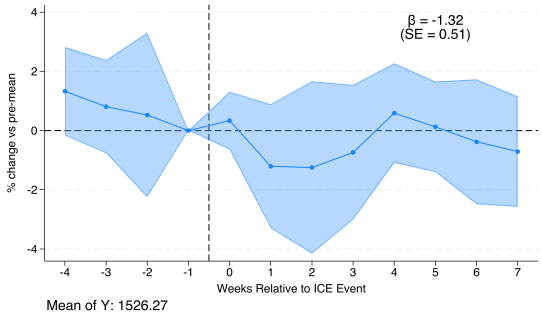
(a) Near workplace



(b) Near education facility



(c) Near healthcare facility

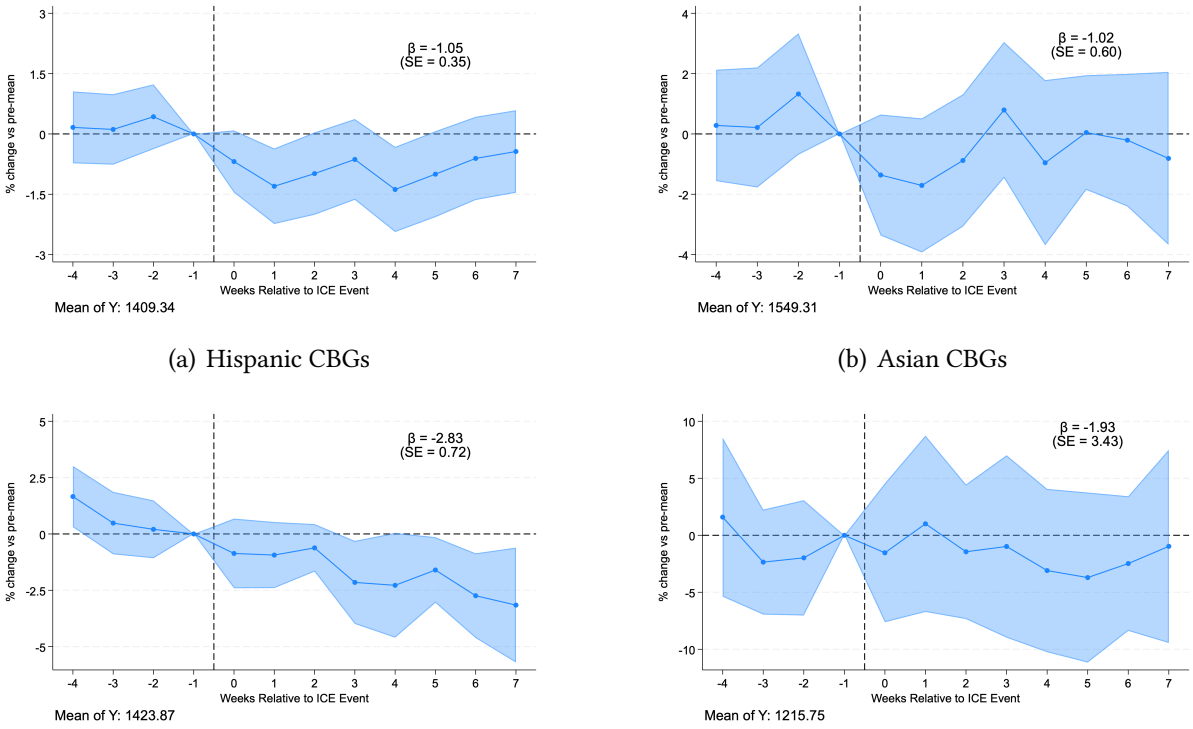


(d) No sensitive location

Figure 10: Event-Study Estimates by Proximity to Location Type

Notes: This figure estimates the baseline event-study specification separately for ICE events partitioned by the presence of a given location type within 0.1 mile of the raid's geocoordinates. Panel (a) includes raids with at least one workplace POI within 0.1 mile; panel (b) includes raids with at least one education facility within 0.1 mile; panel (c) includes raids with at least one healthcare establishment within 0.1 mile; panel (d) includes raids with no school, healthcare, or workplace POI within 0.1 mile. Groups are non-mutually exclusive for panels (a)–(c). This proximity-based classification addresses two limitations of a nearest-POI assignment: the Advan foot-traffic dataset covers only a sample of commercial and institutional establishments, so the nearest POI in the data may not be the nearest establishment in the full population; and assigning each raid to a single nearest type introduces measurement error that attenuates category-specific estimates. The outcome in all panels is total visits (weekly) across all POI types within the 0.5-mile treatment radius, normalized by the pre-treatment mean. Treated POIs are those within 0.5 miles of the event location, while control POIs are between 0.5 and 3 miles. Coefficients are normalized to zero in event week -1. All specifications include POI$\times$stack, week$\times$stack, and pre-period visit quartile$\times$week fixed effects. Standard errors are clustered at the census-tract level. Appendix Figure A5 reports the corresponding estimates for unique visitors. Shaded areas denote 95% confidence intervals. "Mean of Y" reports the subsample-specific pre-event mean.

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(a) Hispanic CBGs

(b) Asian CBGs

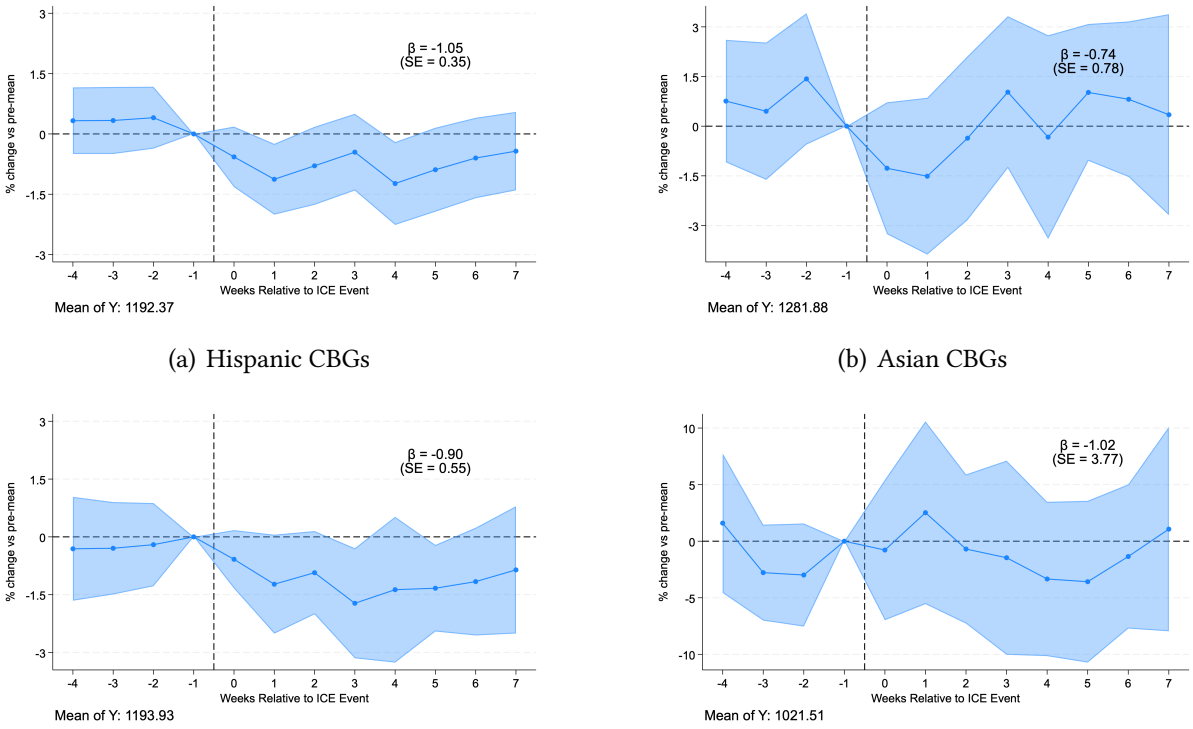
(c) White CBGs

(d) Black CBGs

Figure 11: Event-Study Estimates of Visits by the Racial Composition of Visitors' Home Census Block Groups

Notes: This figure estimates the baseline event-study specification using total visits (weekly), decomposed by the racial composition of visitors' home census block groups (CBGs), with outcomes expressed as percent changes. CBGs are classified using ACS five-year estimates: a CBG is assigned to the Hispanic, Asian, White (non-Hispanic), or Black (non-Hispanic) category if that group's population share exceeds 0.5; CBGs that do not exceed the 0.5 threshold for any group are classified as mixed and excluded. For each POI-week cell, total visits are aggregated separately from CBGs in each racial category. Panels (a)–(d) correspond to visits from Hispanic-majority, Asian-majority, White-majority, and Black-majority CBGs, respectively. Treated POIs are those within 0.5 miles of the event location, while control POIs are between 0.5 and 3 miles. Coefficients are normalized to zero in event week -1. All specifications include POI$\times$stack, week$\times$stack, and pre-period visit quartile$\times$week fixed effects. Outcomes are normalized by the pre-treatment mean within each subsample, so coefficients represent percent changes. Standard errors are clustered at the census-tract level. Shaded areas denote 95% confidence intervals.

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(a) Hispanic CBGs

(b) Asian CBGs

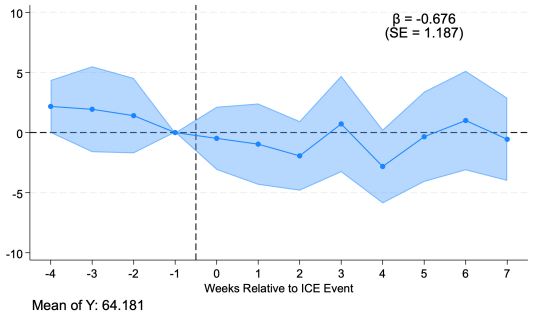
(c) White CBGs

(d) Black CBGs

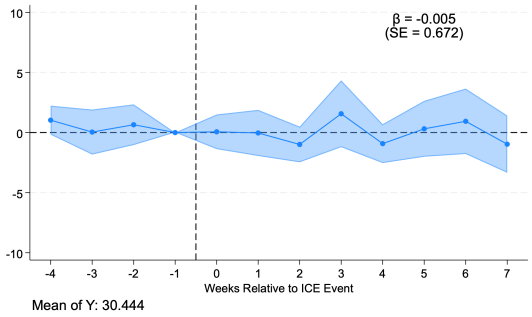
Figure 12: Event-Study Estimates of Unique Visitors by the Racial Composition of Visitors' Home Census Block Groups

Notes: This figure estimates the baseline event-study specification using unique visitors (weekly), decomposed by the racial composition of visitors' home census block groups (CBGs), with outcomes expressed as percent changes. CBGs are classified using ACS five-year estimates: a CBG is assigned to the Hispanic, Asian, White (non-Hispanic), or Black (non-Hispanic) category if that group's population share exceeds 0.5; CBGs that do not exceed the 0.5 threshold for any group are classified as mixed and excluded. For each POI-week cell, total visits are aggregated separately from CBGs in each racial category. Panels (a)–(d) correspond to visits from Hispanic-majority, Asian-majority, White-majority, and Black-majority CBGs, respectively. Treated POIs are those within 0.5 miles of the event location, while control POIs are between 0.5 and 3 miles. Coefficients are normalized to zero in event week -1. All specifications include POI×stack, week×stack, and pre-period visit quartile×week fixed effects. Outcomes are normalized by the pre-treatment mean within each subsample, so coefficients represent percent changes. Standard errors are clustered at the census-tract level. Shaded areas denote 95% confidence intervals.

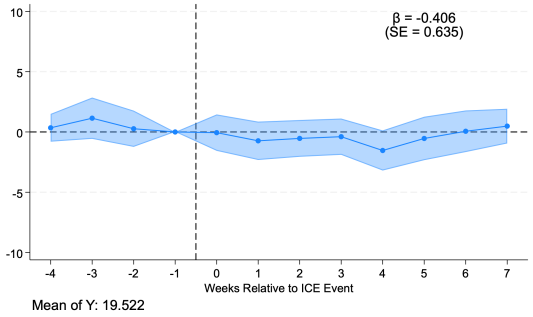
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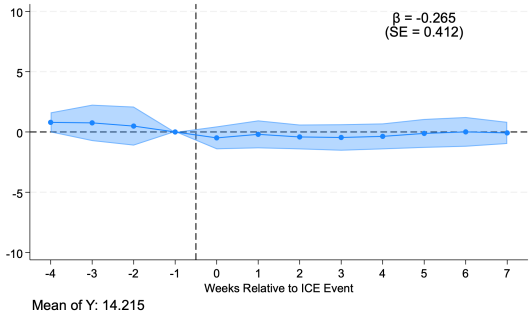
(a) Total postings



(b) High-skill postings



(c) Low-skill postings

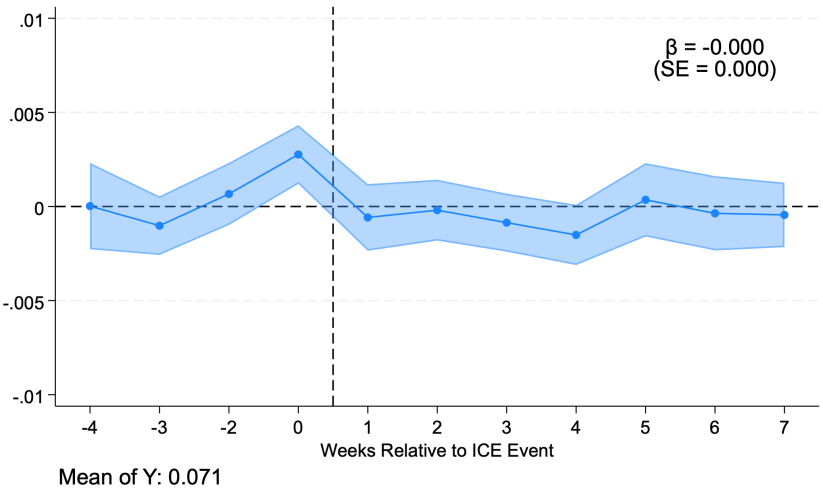


(d) Other postings

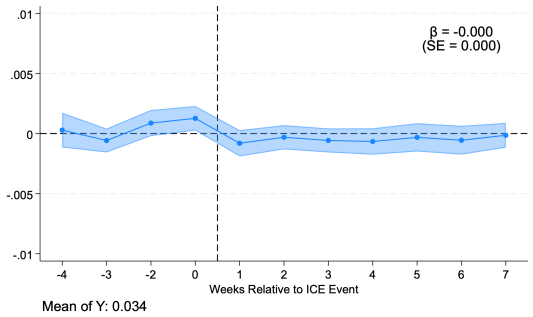
Figure 13: Event-Study Estimates for Job Postings

Notes: This figure reports event-study estimates from stacked city-week regressions. For each treated city, defined by the first observed ICE raid week, the stack includes the treated city and its propensity-score-matched control within the same county. Event time is defined relative to the treated city's first raid week and binned to $[-5, 7]$, with endpoints pooled. Coefficients are normalized to zero in week -1. The outcome is weekly job postings: total (panel (a)), high-skill (panel (b)), low-skill (panel (c)), and other (panel (d)). Skill classification follows O*NET Job Zone assignments, where Zones 4–5 are high-skill, Zones 1–2 are low-skill, and Zone 3 and unmatched occupations are classified as other. All specifications include city fixed effects and county$\times$week fixed effects. Standard errors are clustered at the city level. Shaded areas denote 95% confidence intervals.

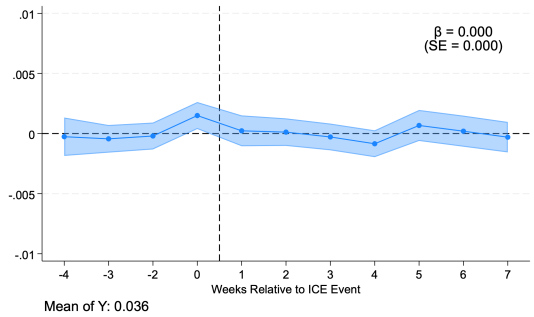
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(a) Total crime



(b) Violent crime



(c) Non-violent crime

Figure 14: Effect of ICE Enforcement on Local Crime Rates

Notes: This figure estimates the effect of ICE enforcement events on crime incident counts in the surrounding neighborhood using geocoded crime incident data and a hyperlocal stacked event-study design. Census blocks are assigned based on the location of their centroids: blocks within 0.5 miles of the event location are defined as treated, while blocks between 0.5 and 3 miles serve as controls. The outcome is weekly crime incident counts aggregated to the census block-week level. Panel (a) uses total crime, Panel (b) uses violent crime (including assault, robbery, and homicide), and Panel (c) uses non-violent crime (including property crime, vandalism, and drug offenses). All specifications include block×stack, week×census-tract, and week×crime-category fixed effects. Standard errors are clustered at the census-tract level. Shaded areas denote 95% confidence intervals.

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Tables

Table 1: Summary Statistics

	Mean	SD	N
Panel A: ICE Enforcement Events			10,018
Contact enforcement	0.597	0.491	
Number of officers	4.0	5.1	
Sensitive location within 0.1 mile[†]			
Education facility	0.011	0.105	
Healthcare establishment	0.017	0.130	
Workplace	0.083	0.276	
None of the above	0.917	0.276	
Panel B: Stack Panel Sample (POI× Week)			12,412,990
Weekly visits	1454.1	4489.0	
Unique weekly visitors	1211.7	4423.9	
Distance to event (miles)	1.773	0.876	
Treated ($\leq$0.5 miles)	0.089	0.285	
POI type indicators			
School POI	0.040	0.197	
Healthcare POI	0.026	0.159	
Any workplace POI	0.937	0.244	
Consumer-facing	0.688	0.463	
Worker-dominated	0.271	0.444	

Notes: Panel A describes the 10,018 ICE enforcement events in the analysis sample, spanning January 2025 to January 2026 across California, Illinois, Minnesota, New York, Oregon, and Texas. Contact enforcement includes events such as arrests, raids, checkpoints, knocking, and questioning, while non-contact enforcement includes events such as loitering, errands, patrolling, and staging. Panel B describes the stacked analysis panel, consisting of all POI×week cells within a 3-mile radius and a $[-4, 7]$ week window around each event. The sample is restricted to POIs that are never treated or first treated in the current stack. The treated indicator equals one if the POI is within 0.5 miles of the event location. POI type indicators are constructed using NAICS codes and establishment-name text.

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Table 2: Balance: Visits at Week -1 by Treatment Status

	Mean Visits		Mean Diff.	Cond. Mean Diff.
Sample	Control (0.5–3 mi)	Treated ($\leq$0.5 mi)	(Uncond. Diff.)	(Stack FEs)
Full sample	1449.0	1578.7	129.6***	25.9
			(-7.51)	(20.66)
Education facility POIs	2508.4	2565.9	57.5	178.5
			(-0.38)	(255.17)
Healthcare POIs	834.6	954.4	119.8***	58.2
			(-4.10)	(36.70)
Any workplace POI	1422.6	1582.5	159.8***	35.3*
			(-9.27)	(21.47)
Consumer-facing establishments	1306.4	1399.0	92.6***	9.9
			(-6.67)	(18.69)
Worker-dominated establishments	1686.1	2097.2	411.1***	129.0***
			(-8.36)	(49.92)
Immigrant-intensive workplaces	1713.3	2304.0	590.7***	134.5**
			(-6.73)	(67.37)
Native-intensive workplaces	1655.4	1926.3	270.9***	121.5*
			(-5.74)	(68.63)

Notes: This table compares weekly visits in week -1 (the reference period) between treated and control POIs across sample subgroups. Treated POIs are those within 0.5 miles of the enforcement event, while control POIs are between 0.5 and 3 miles. The mean-difference column reports the raw treated-minus-control difference, with t-statistics in parentheses. The conditional mean difference column reports the coefficient on the treated indicator from regressions that absorb stack fixed effects, with standard errors clustered at the census-tract level. POI type indicators follow the NAICS-based definitions used in the main analysis. Workplace POIs include both consumer-facing and worker-intensive establishments. Immigrant-intensive workplaces include sectors such as manufacturing, construction, agriculture, restaurants, accommodation, and warehousing, while native-intensive workplaces include the remaining establishments within worker-intensive POIs. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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Table 3: Effect of ICE Enforcement on Weekly Foot Traffic

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Alternative Fixed Effects			Alternative Sample		
	Baseline	Parsimonious	+Ind2×Wk	+Ind4×Wk	Excl. Entrants	Excl. Exiters	Balanced
Panel A: Total weekly visits (pre-event mean: 1555.5 visits/wk)							
Average Treatment Effect	-1.69^{***}	-1.60^{***}	-1.58^{***}	-1.31^{***}	-1.69^{***}	-1.69^{***}	-1.72^{***}
	(0.30)	(0.30)	(0.30)	(0.28)	(0.30)	(0.30)	(0.30)
Obs.	12,374,764	12,374,764	12,374,691	12,372,896	12,370,177	12,092,498	11,733,715
Panel B: Weekly unique visitors (pre-event mean: 1331.0 visitors/wk)							
Average Treatment Effect	-1.04^{***}	-0.96^{***}	-0.92^{***}	-0.78^{***}	-1.04^{***}	-1.04^{***}	-1.06^{***}
	(0.26)	(0.25)	(0.25)	(0.24)	(0.26)	(0.26)	(0.26)
Obs.	12,374,764	12,374,764	12,374,691	12,372,896	12,370,177	12,092,498	11,733,715
POI Fixed Effects	✓	✓	✓	✓	✓	✓	✓
Tract × Time FEs	✓	✓	✓	✓	✓	✓	✓
Size Qrt.× Week FEs	✓		✓	✓	✓	✓	✓
2-dig. NAICS × Week FEs			✓				
4-dig. NAICS × Week FEs				✓			
Excl. New Entrants					✓		
Excl. Exiters						✓	
Balanced Panel							✓

Notes: Panel A reports estimates for total visits (weekly), while Panel B reports the corresponding estimates for unique visitors (weekly). Each cell reports the average treatment effect, expressed as a percentage change relative to the pre-event mean, from a stacked DiD regression over weeks -4 through $+7$ around each ICE enforcement event. Treated POIs are those within 0.5 miles of the event location, while control POIs are between 0.5 and 3 miles. Column (1) presents the baseline specification, which includes POI fixed effects, stack×week fixed effects, and baseline size quartile×week fixed effects. Column (2) presents a parsimonious specification that omits the size quartile×week fixed effects. Column (3) adds 2-digit NAICS×week fixed effects to absorb industry-specific time trends. Column (4) adds 4-digit NAICS×week fixed effects to control for more granular industry trends. Column (5) excludes treated POIs first observed at event time week 0 or later. Column (6) excludes POIs observed only in the pre-period. Column (7) restricts the sample to POIs present in all weeks from -4 through $+4$. Standard errors are clustered at the census-tract level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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Table 4: Social Costs of ICE Enforcement per Event

Channel (k)	Effect $\hat{\beta}^k$ (%)	Baseline $\bar{Y}^k$ (visits/wk)	Shadow value v^k ($/unit)	Social cost C_k ($/estab., 4 wk)
A. Labor supply				
Worker-intensive establishments	1.32	1,674.1	$600/wk	$53,197
B. Local business revenue				
Customer-facing establishments	1.02	1,352.7	$50/visit	$2,760
C. Education				
Education facility POIs	1.63	1,377.7	$1,556/wk	$139,712
D. Benchmark: government enforcement cost				
Per person arrested, detained, and removed (DHS, 2025)	—	—	—	$17,121

Notes: $C_k = \hat{\beta}^k \cdot \bar{Y}^k \cdot v^k \cdot T$, where $\hat{\beta}^k$ denotes the percentage decline in activity estimated from the DiD specification, $\bar{Y}^k$ is the pre-event baseline activity per week, v^k is the dollar value per foregone unit, and $T = 4$ weeks. The labor supply channel (Panel A) and the education channel (Panel C) use the unique visitor outcome, while the consumer revenue channel (Panel B) uses total visits (see Section 7.2 for details). Dollar values are defined as follows. For Panel A, we use the minimum wage floor of $15.00 per hour multiplied by 40 hours, yielding $600 per week. For Panel B, we use average per-trip expenditure at food and retail establishments from the BLS Consumer Expenditure Survey (2024). For Panel C, we compute $0.07 \times 50{,}000 \div 180 \times 5 = \$97$ per week, following Card (1999) and BLS QCEW (2024). Healthcare is excluded because Section 5.3 finds no statistically significant decline in visits to healthcare establishments. Panel B reports lost business revenue rather than a welfare measure (see Section 7.2). All estimates should be interpreted as partial-equilibrium lower bounds (see Section 7.3).

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Appendix

Panel A: ICEout website interface



Panel B: Example ICEout case report

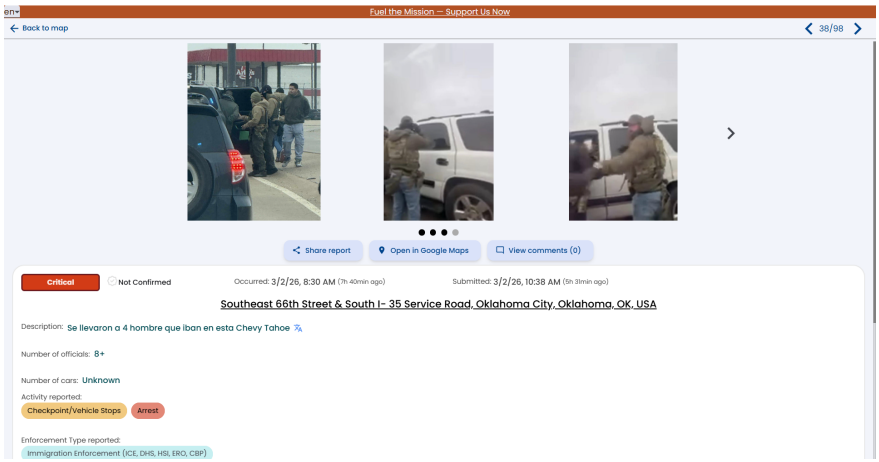
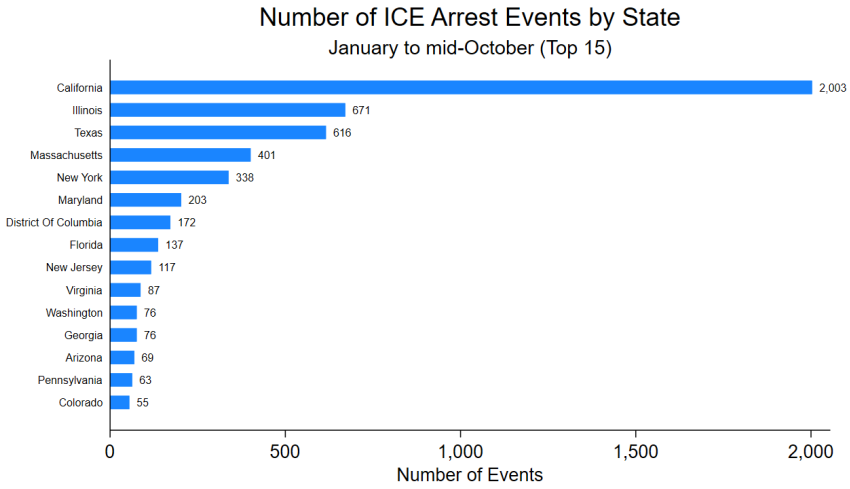


Figure A1: Illustration of ICEout Data Source

Notes: Panel A displays the ICEout website interface. Panel B shows an example case report with event details and supporting documentation.

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Panel A: ICE events by state



Panel B: Official arrests by state

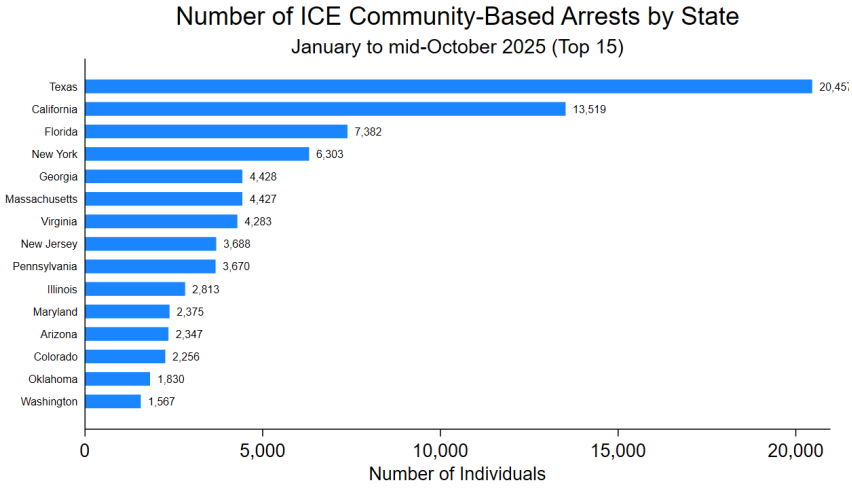
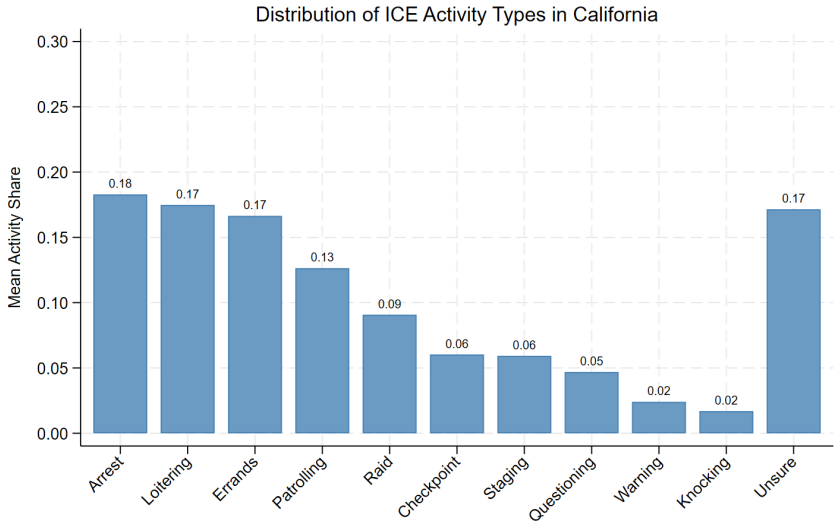


Figure A2: ICE Community-Based Arrests: Community Reports vs. Official Arrest Data

Notes: Panel A reports the number of ICE arrest events by state using ICEout data, which are compiled from community-reported enforcement activity. Panel B reports the number of ICE community-based arrests by state using official administrative data from the Deportation Data Project. While Panel A measures reported arrest events, Panel B measures the number of individuals arrested. The figure displays the top 15 states in each panel from January 21, 2025 to October 15, 2025, the period for which data are available from the Deportation Data Project.

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Panel A: Activity type distribution



Panel B: Weekly enforcement trend

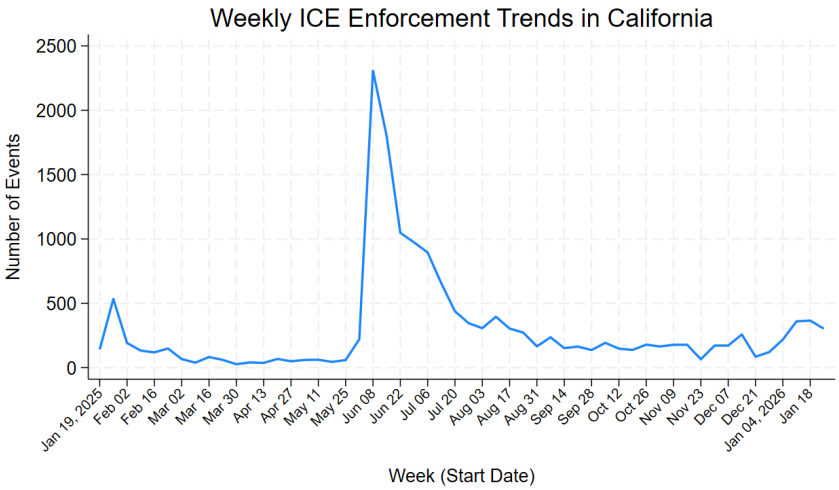
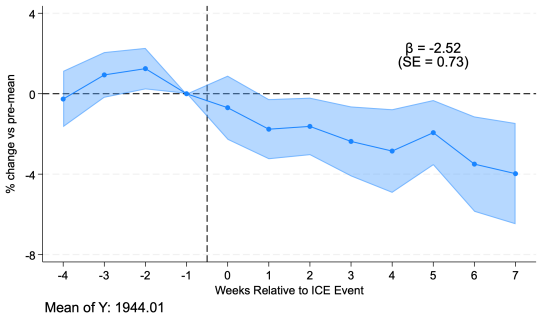


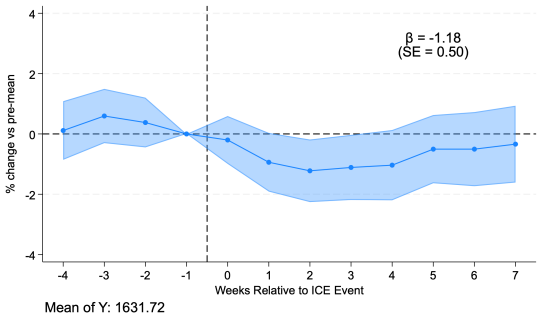
Figure A3: ICE Community Enforcement Activity in California, 2025–2026

Notes: This figure uses ICEout data to document community-based ICE enforcement activity in California from January 2025 to January 2026. Panel A reports the distribution of activity types, where each bar represents the share of events classified into a given category. Panel B plots the weekly number of reported enforcement events, aggregated by event date. ICEout events are compiled from community reports and may reflect variation in reporting intensity across time and across locations.

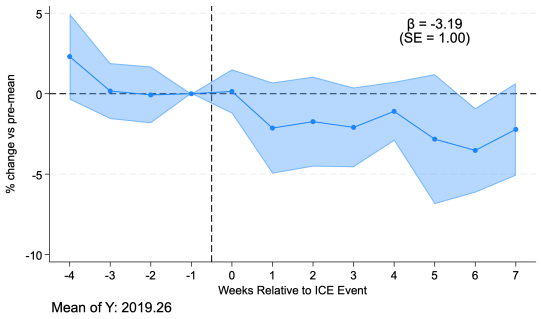
[Back to page 17]



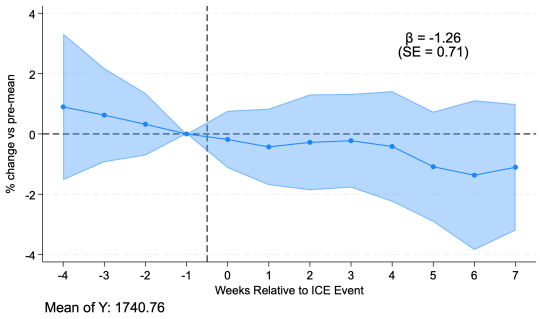
(a) Immigrant-intensive, visits



(b) Immigrant-intensive, visitors



(c) Native-intensive, visits

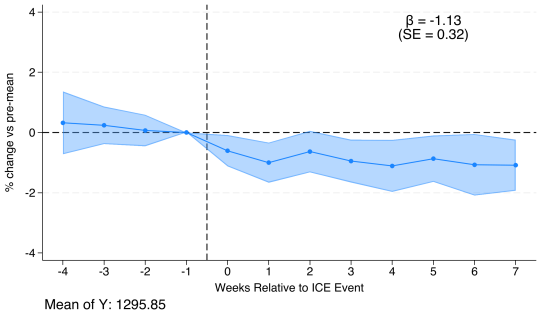


(d) Native-intensive, visitors

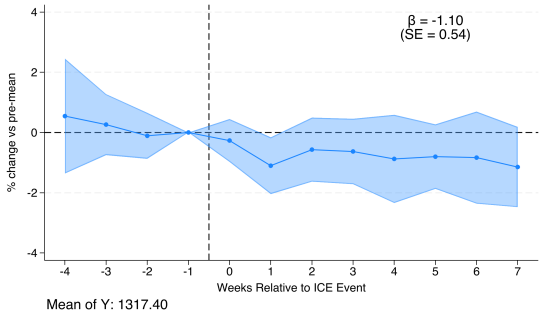
Figure A4: Event-Study Estimates by Immigrant Intensity of Workplace Industries

Notes: This figure estimates the baseline event-study specification separately for establishments in industries classified by immigrant workforce intensity, based on NAICS industry groups, with outcomes expressed as percent changes. Industries with a higher share of immigrant workers, shown in panels (a) and (b), include sectors such as food manufacturing, construction, agriculture, restaurants, accommodation, and warehousing. Industries with a higher share of native-born workers, shown in panels (c) and (d), include sectors such as finance, professional services, and information. Panels (a) and (c) use total visits (weekly), while panels (b) and (d) use unique visitors (weekly). These classifications are based on industry-level workforce composition and should be interpreted as proxies rather than precise measures of immigrant and native employment at the establishment level. Treated POIs are those within 0.5 miles of the event location, while control POIs are between 0.5 and 3 miles. Coefficients are normalized to zero in event week -1. All specifications include POI×stack, week×stack, and pre-period visit quartile×week fixed effects. Outcomes are normalized by the pre-treatment mean within each subsample, so coefficients represent percentage changes. Standard errors are clustered at the census-tract level. Shaded areas denote 95% confidence intervals. "Mean of Y" reports the subsample-specific pre-event mean.

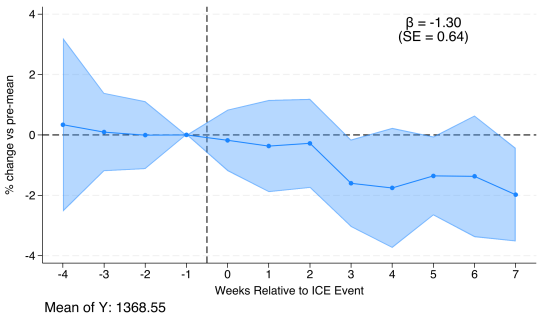
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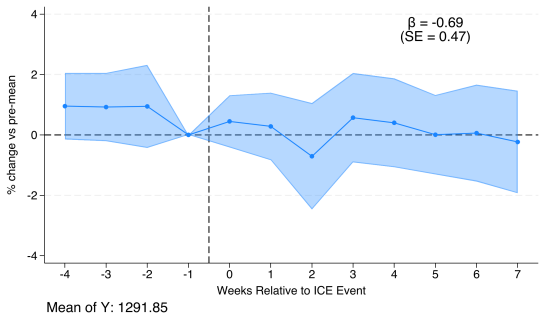
(a) Near workplace



(b) Near education facility



(c) Near healthcare facility

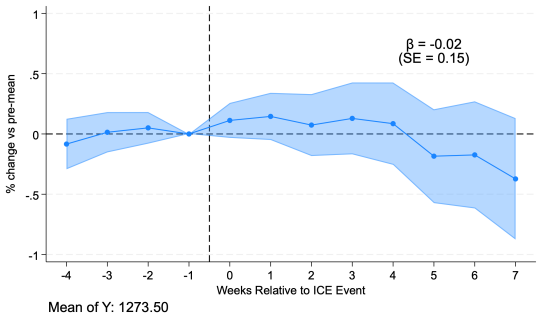


(d) No sensitive location

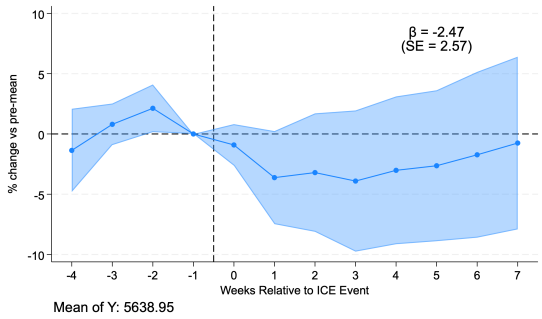
Figure A5: Event-Study Estimates by Proximity to Location Type, Unique Visitors

Notes: This figure replicates Figure 10 using weekly unique visitors as the outcome. Panels (a)–(d) correspond to raids with at least one workplace, education facility, healthcare establishment, or no sensitive location within 0.1 mile, respectively. See Figure 10 notes for location-type classification criteria and the rationale for the proximity-based definition. Treatment and control definitions, event-time normalization, fixed effects, and standard error clustering follow Figure 2. Shaded areas represent 95% confidence intervals. "Mean of Y" is the subsample-specific pre-event mean.

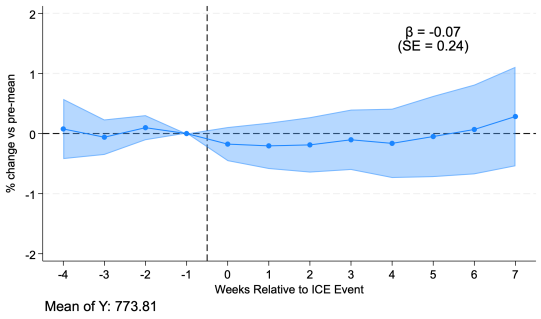
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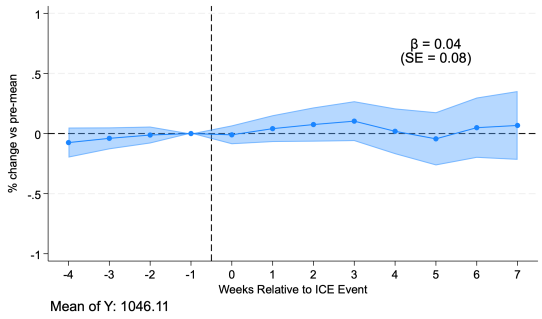
(a) Workplace POIs



(b) Education facility POIs



(c) Healthcare POIs

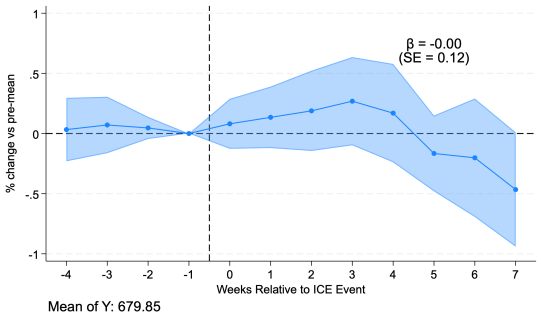


(d) Commercial POIs

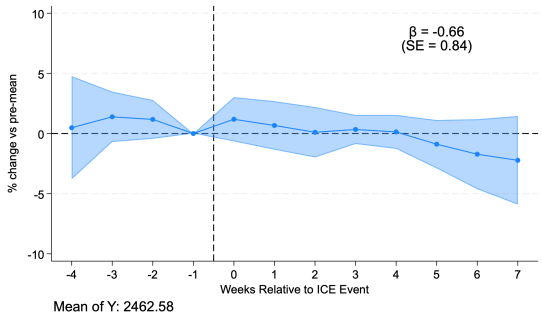
Figure A6: Geographic Substitution: Spillovers to Untreated Same-Type POIs in the Same County, Visits

Notes: This figure tests for geographic substitution by estimating post-enforcement changes in total visits (weekly) at untreated POIs of the same type located beyond 3 miles of the event location but within the same county. A positive post-event coefficient is consistent with geographic displacement, while a null or negative coefficient indicates genuine withdrawal. The sample in each panel consists of same-type POIs located outside the 3-mile estimation window but within the same county as the event. All specifications include POI×stack and county×week fixed effects. Standard errors are clustered at the census-tract level. Shaded areas represent 95% confidence intervals.

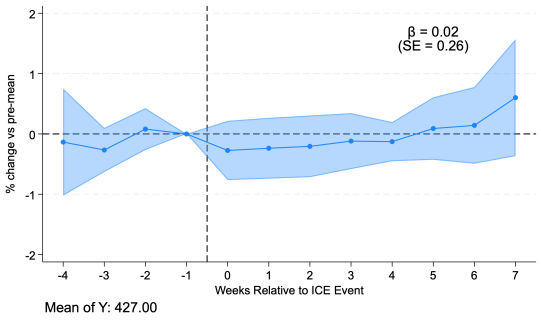
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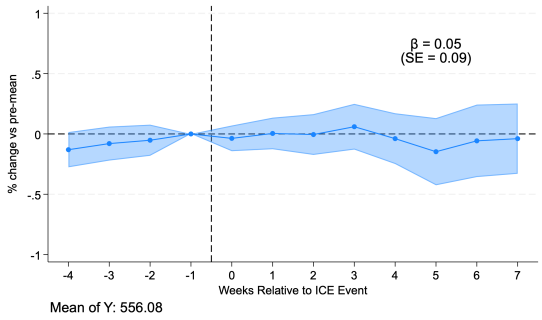
(a) Workplace POIs



(b) Education facility POIs



(c) Healthcare POIs



(d) Commercial POIs

Figure A7: Geographic Substitution: Spillovers to Untreated Same-Type POIs in the Same County, Unique Visitors

Notes: This figure replicates Appendix Figure A6 using unique visitors (weekly) as the outcome. A positive post-event coefficient is consistent with geographic displacement, while a null or negative coefficient indicates genuine withdrawal. The sample in each panel consists of same-type POIs located outside the 3-mile estimation window but within the same county as the event. All specifications include POI×stack and county×week fixed effects. Standard errors are clustered at the census-tract level. Shaded areas represent 95% confidence intervals.

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Table A1: Covariate Balance Before and After Propensity Score Matching

	(1) Treated	(2) Controls (All)	(3) Controls (Matched)	(4) SMD (Pre)	(5) SMD (Post)
Panel A: Means (standard deviations in parentheses)					
Log Population	10.598	8.718	10.405	1.164	0.196
	(1.094)	(2.004)	(0.849)		
Median HH Income ($000s)	95.913	111.965	101.685	−0.401	−0.191
	(28.972)	(48.640)	(31.340)		
Poverty Rate	0.104	0.103	0.092	0.015	0.277
	(0.047)	(0.098)	(0.043)		
Hispanic Share	0.383	0.222	0.320	0.719	0.284
	(0.239)	(0.206)	(0.204)		
Foreign-Born Share	0.257	0.157	0.272	0.914	−0.129
	(0.106)	(0.113)	(0.132)		
Unemployment Rate	0.059	0.062	0.054	−0.057	0.290
	(0.020)	(0.056)	(0.019)		
College Share	0.343	0.393	0.388	−0.249	−0.266
	(0.180)	(0.220)	(0.161)		
N	183	656	183		

Notes: This table reports covariate means and standardized mean differences (SMD) for treated and control cities, before and after propensity score matching. Column (1) reports statistics for all treated cities (cities that experienced at least one ICE enforcement event during the sample period). Column (2) reports statistics for all never-treated cities in the same county as any treated city prior to matching. Column (3) reports statistics for the matched control observation selected for each treated city; when the same control city is reused across multiple treated cities, it enters column (3) with replacement. The propensity score is estimated by logistic regression of treatment status on 2022 American Community Survey city-level covariates. Matching is 1:1 nearest-neighbor on the logit propensity score within the same county, subject to a caliper of $0.25 \times \mathrm{SD}(\mathrm{logit}\,\hat{p})$. Treated cities with no within-caliper match are excluded to maintain common support. The standardized mean difference in columns (4) and (5) is defined as $(\bar{X}_T - \bar{X}_C) \,/\, \sqrt{(s_T^2 + s_C^2)/2}$, where subscripts T and C denote treated and control groups, respectively.

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Table A2: Effect of Weekly ICE Raid Count on Job Postings

	(1) Total	(2) High-Skill	(3) Low-Skill	(4) Other
Weekly Raid Count	−0.0003	−0.0230	0.0199	0.0045
	(0.0132)	(0.0190)	(0.0136)	(0.0193)
Observations	7,308	7,273	7,273	7,167
City FEs	✓	✓	✓	✓
County × Week FEs	✓	✓	✓	✓

Notes: Each column reports OLS estimates of the association between contemporaneous weekly ICE raid counts and weekly job postings in the matched stacked city-week sample. Columns (1)–(4) correspond to total, high-skill, low-skill, and other postings. Skill categories follow O*NET Job Zone assignments, as described in the notes to Figure 13. The sample pairs each treated city with its propensity-score-matched control within the same county, as detailed in Appendix Table A1. All specifications include city and county×week fixed effects. Unlike the event-study specification, this approach does not allow for a pre-period placebo test and should be interpreted as a descriptive conditional correlation rather than a causal effect. Standard errors are clustered at the city level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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