The "Chilling Effect" of ICE Enforcement: Evidence from High-Frequency Mobility and Spending Data in the Los Angeles Region

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January 26, 2026

PRELIMINARY DRAFT

Abstract

Anecdotal accounts from immigrant communities and media reports describe sharp reductions in everyday activity following high-profile immigration enforcement actions, as residents avoid public spaces, workplaces, and local businesses out of fear of detention. Despite the prominence of these narratives, systematic evidence on whether enforcement measurably disrupts local economic life remains limited. This paper uses high-frequency cellphone-based foot-traffic data and transaction-based consumer spending data to examine how intensified Immigration and Customs Enforcement (ICE) activity affected local economic activity in the Los Angeles–Orange County metropolitan area. Exploiting the timing of a major ICE operation announced in mid-May 2025 and variation in exposure to immigrant communities, we estimate dynamic difference-in-differences models tracing behavioral responses before and after the enforcement escalation. Our primary analysis compares census block groups with high versus low shares of Latin American–born residents, and we complement this approach with an alternative identification strategy that defines exposure at the point-of-interest level based on the residential composition of businesses' customers. Across both strategies, we find no evidence of differential pre-trends and large, persistent post-announcement declines in economic activity. In immigrant-heavy neighborhoods, weekly foot traffic falls by 5–8 percent in the two months following the announcement (8–10 percent in retail districts), while consumer spending declines by roughly 20–25 percent. These effects are robust across specifications, absent in placebo tests, and imply over $625 million in lost sales and $59 million in foregone sales tax revenue in the region. Together, the results provide direct evidence that immigration enforcement generates economically meaningful spillovers on local commercial activity that extend well beyond the individuals directly targeted by enforcement actions.

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1 Introduction

Immigration enforcement has intensified dramatically over the past two decades, transforming from a border-focused activity into a pervasive and highly visible presence throughout U.S. communities. Following the inauguration of President Trump in January 2025, ICE enforcement operations escalated sharply: arrests of immigrants without criminal records increased by more than 1,000 percent compared to the previous administration, workplace raids became routine rather than exceptional, and enforcement activity extended into churches, schools, and hospitals, spaces previously considered "sensitive locations" where ICE presence was limited (Factchequeado 2025). The visibility and unpredictability of these operations have fundamentally altered the everyday lives of millions of immigrants and their families. Scholars describe this shift as creating a climate of persistent surveillance and fear that extends far beyond those directly targeted for enforcement, reshaping how entire communities navigate public space, engage with institutions, and participate in economic life (Asad 2023).

The reach of enforcement has grown particularly salient under recent administrations. The Trump administration's immigration policies, first implemented between 2017 and 2021 and resumed in 2025, prioritized aggressive interior enforcement, including large-scale workplace raids and neighborhood sweeps. These operations generate extensive media coverage and public visibility, amplifying their psychological impact well beyond the individuals directly detained. When ICE conducts a raid at a single workplace or neighborhood, the resulting fear and behavioral changes ripple outward through entire communities, affecting not only undocumented immigrants but also legal residents and mixed-status families. In addition, recent events from Minneapolis, MN suggest an explicit increase in the level of violence used by ICE agents on potential detainees and protesters alike.[1]

A substantial scholarly literature documents how the threat of enforcement alters labor supply, school attendance, health care utilization, and family routines, even among legally present immigrants and mixed-status households (Watson 2014; Asad 2023). Yet much of what is known about these "chilling effects" comes either from administrative outcomes that are only indirectly related to everyday life or from surveys and qualitative accounts that capture fear and avoidance but cannot directly observe changes in local economic activity or commercial behavior.

Recent national reporting suggests that these chilling effects are now unfolding at a scale that is visible in everyday public life. A December 2025 investigation by *The New York Times* describes how the Trump administration's immigration crackdown is being felt not only at

[1]Meg Anderson, "Tackles, projectiles and gunfire: Many fear ICE tactics are growing more violent," *NPR*, October 13, 2025, https://www.npr.org/2025/10/13/nx-s1-5566785/ice-dhs-immigration-tactics-more-violent.

the border or in detention facilities, but in grocery stores, churches, schools, restaurants, and neighborhood festivals across the country. In immigrant-heavy communities, the paper reports that parents pull children out of school when they hear about detentions, church pews are emptier, soccer leagues struggle to field teams, and small businesses cancel events because customers are afraid to leave their homes. Restaurant owners, daycare operators, hospital administrators, and local officials repeatedly describe the same pattern: enforcement activity directed at a subset of residents produces a generalized retreat from public and commercial life that spills over into the broader local economy (DePillis and Robertson 2025).

These accounts are powerful, but they also raise a fundamental empirical question: *Can we actually measure this chilling effect in real time, and can we quantify its magnitude?* Stories of empty grocery aisles, quiet churches, and canceled concerts imply real economic losses for neighborhood businesses, but without systematic data it is impossible to know whether these effects are small and fleeting or large and persistent. Moreover, anecdotal evidence cannot distinguish between fear-driven behavioral change and other forces that might coincidentally affect local activity, such as seasonal patterns, inflation, or broader economic cycles.

This paper seeks to provide direct quantitative evidence on the local economic consequences of immigration enforcement by measuring how intensified Immigration and Customs Enforcement (ICE) activity affects both foot traffic and consumer spending. We focus on a major ICE enforcement operation announced in mid-May 2025 and study how activity changes before and after the announcement using high-frequency cellphone mobility data and transaction-based spending data. Our primary identification strategy compares census block groups with high versus low shares of Latin American–born residents, capturing differential exposure to enforcement-related fear at the neighborhood level.

We complement this neighborhood-based approach with an alternative identification strategy that defines exposure at the point-of-interest level based on the residential locations of businesses' customers. By identifying establishments that disproportionately serve patrons from immigrant communities—regardless of where those establishments are located—this strategy allows us to test whether enforcement-related fear translates into reduced economic activity specifically at businesses most reliant on immigrant customers. Together, these complementary approaches allow us to assess whether the chilling effects of enforcement operate through neighborhood context, customer composition, or both.

Beyond documenting changes in mobility and spending, we translate our estimated treatment effects into measures of economic and fiscal impact. Using the spending responses from our preferred specifications, we project aggregate revenue losses for neighborhood businesses across the Los Angeles–Orange County metropolitan area and estimate the associated

reductions in local sales tax collections. These projections provide a direct link between enforcement-induced behavioral change and the fiscal consequences borne by state and local governments.

We focus on the Los Angeles–Orange County metropolitan area because it contains some of the nation's largest and most economically important immigrant communities, including cities such as Los Angeles, Anaheim, and Santa Ana, and because the scale of immigration enforcement activity in the region has been highly visible. In 2025, federal immigration raids across Los Angeles County drew large protests, confrontations between demonstrators and agents, and broad media attention to the disruption and anxiety these operations generated in immigrant neighborhoods (Los Angeles Times 2025). The size and diversity of the region also yield a sufficiently large and heterogeneous sample to support detailed statistical analysis of both neighborhood-level and establishment-level responses to enforcement activity, as well as credible projections of regional economic and fiscal impacts.

2 Literature Review

A substantial body of research documents that immigration enforcement affects behavior not only among those directly targeted but throughout immigrant communities and even among legal residents and U.S. citizens in mixed-status families. This literature identifies what Watson (2014) termed "chilling effects,"the tendency for enforcement to deter participation in beneficial programs, services, and everyday activities among populations that perceive themselves to be at risk. The key insight is that enforcement operates not only through direct detention and deportation but through generalized fear and risk perception that alters everyday decision-making.

The most extensively documented chilling effects concern healthcare seeking. Healthcare facilities have important similarities with retail establishments and neighborhood commercial districts since they are both require prolonged presence in public spaces and individuals must weigh the benefits of participation against perceived enforcement risks. If enforcement-induced fear is powerful enough to deter immigrants from seeking essential medical care, where the stakes are health and well-being, it should generate even stronger effects on discretionary commercial activities, making healthcare utilization studies a valuable conceptual template for understanding how enforcement alters everyday economic activity.

Watson (2014) provides evidence that increased federal immigration enforcement reduces Medicaid participation among children of non-citizens, even when the children themselves are U.S. citizens facing no eligibility barriers. Exploiting spatial and temporal variation in INS enforcement activity across 25 district clusters from 1993-2002, Watson finds that a one-

log-point increase in enforcement reduces Medicaid participation by 9.2 percentage points for low-income children of non-citizens. Her analysis demonstrates that up to 75 percent of the decline in non-citizen Medicaid participation around the time of welfare reform can be attributed to the contemporaneous spike in immigration enforcement rather than to welfare reform itself. Subsequent research has confirmed and extended these findings across multiple dimensions of healthcare access. Herring and Barnow (2025) find that intensified enforcement under Secure Communities led to a 16.9 percent decline in healthcare office visits among likely authorized Hispanic immigrants aged 50+ relative to non-Hispanic U.S.-born respondents. These declines are not driven by loss of insurance coverage but rather by fear and perceived risk, both chilling effects stemming from ambiguous immigration law and network effects from fear of exposing undocumented family members. The effects are even larger among individuals with worse health status (18 percent decline among those without college education) and those with chronic conditions, precisely the populations for whom foregone healthcare imposes the greatest long-run costs.

Recent qualitative and ethnographic research emphasizes the everyday strategies through which immigrant households manage surveillance and enforcement risk. Asad (2023) documents how Latino immigrant families alter mobility patterns, avoid public spaces, and withdraw from institutions in response to enforcement threats, highlighting enforcement as a persistent background condition shaping daily life. This work underscores that immigration enforcement operates not only through formal legal channels, but also through fear, uncertainty, and anticipatory behavior.

This literature provides the theoretical foundation for our analysis. If enforcement-induced fear is powerful enough to deter immigrants from seeking essential medical care for themselves and their children, it should generate even stronger effects on discretionary activities like shopping, dining out, and other forms of commercial engagement that involve sustained presence in public spaces. Our contribution is to directly measure these effects on everyday economic activity using high-frequency data that captures behavioral responses in real time.

The proliferation of high-frequency mobility and transaction data since 2019 has transformed researchers' ability to measure behavioral responses to perceived threats in near-real time. The COVID-19 pandemic accelerated adoption of these data sources, particularly smartphone location data (SafeGraph/Advan) and credit card transaction records, as researchers sought to understand how individuals altered movement patterns and spending behavior in response to both policy mandates and voluntary risk avoidance. This literature offers both methodological tools and conceptual parallels for understanding immigration enforcement impacts. Like pandemic risk and policy compliance, enforcement operates through

anticipation and fear as much as through formal action, producing voluntary withdrawal from public and commercial spaces.

A central finding across COVID studies is that perceived threat drives behavior independent of and often prior to formal policy restrictions. Goolsbee and Syverson (2021) establish this framework most directly, using cellphone-based foot traffic data covering over two million businesses to disentangle government shutdown orders from voluntary responses. Exploiting variation across jurisdictional boundaries within the same commuting zones, they find that legal restrictions explain only a small fraction of the roughly 60 percent decline in consumer visits during early 2020. Instead, most of the contraction reflects voluntary decisions to avoid public spaces in response to fear of infection, with traffic declines tracking local COVID death counts rather than policy timing. Dave et al. (2020) document similar patterns: shelter-in-place orders accounted for only part of the observed mobility decline, with substantial reductions occurring prior to formal mandates as individuals responded to perceived health risks. Gupta et al. (2021) extend these findings by showing that informational shocks such as first case announcements and emergency declarations produced significant mobility reductions even in the absence of stringent mandates, accounting for a large share of behavioral change.

The durability of these behavioral responses is particularly relevant for understanding enforcement effects. Leong et al. (2023) analyze downtown visitation patterns across 62 North American cities, documenting highly uneven recovery trajectories that persist long after COVID restrictions ended. Downtowns dominated by office employment experienced persistently depressed visitation, while those with more diverse economic structures recovered more quickly. Notably, the duration of formal restrictions explains relatively little of the variation in recovery, suggesting that lasting behavioral changes including remote work and continued avoidance of dense public spaces shape urban activity patterns even after acute threats recede.

Transaction data reveal how fear-driven behavioral changes translate into economic losses. Baker et al. (2020) provide early evidence that households rapidly altered consumption during the pandemic onset, documenting a sharp initial spending surge driven by stockpiling followed by a sudden 25–30 percent contraction as perceived health risks intensified. Critically, spending declined even in places without immediate shelter-in-place orders, with effects concentrated in discretionary, in-person sectors such as dining, travel, and entertainment. Chetty et al. (2023) scale this analysis using a comprehensive database combining private-sector transaction, payroll, and employment data, demonstrating that consumer spending reductions, particularly by high-income households, were the primary driver of job losses among low-wage workers and small businesses in dense, affluent urban areas. Their find-

ings show that spending declines preceded formal policy interventions and persisted even as restrictions lifted, establishing that fear-driven demand shocks can propagate through local economies and generate persistent employment and revenue losses.

The parallel to immigration enforcement is direct. Just as COVID fear deterred restaurant visits and retail trips independent of legal mandates, enforcement-induced fear may deter commercial activity in immigrant neighborhoods independent of individuals' actual legal status or deportation risk. The combination of mobility and transaction data is particularly powerful: mobility patterns reveal spatial avoidance behavior, while spending data quantify the economic consequences of that avoidance.

3 Data

3.1 Advan Mobility Data

We measure foot traffic using anonymized aggregated mobile phone mobility data provided by Dewey through its Advan platform. These data are derived from opt-in mobile devices and record visits to a universe of points of interest (POIs), including retail establishments, restaurants, and service providers. A visit is defined as a device entering the geofenced boundary of a POI and remaining for a minimum dwell threshold determined by Advan's proprietary algorithms.

The raw data are aggregated to the census block group–week level in order to link mobility data to neighborhood demographic characteristics. Our primary outcomes are the logarithm of total weekly visitors and visits to POIs located within each block group. Advan applies multiple filters to reduce measurement error, including the exclusion of low-quality pings, correction for device sampling bias, and normalization procedures to account for changes in the active device panel over time. Because our empirical strategy relies on within–block group comparisons over narrow event windows and includes fixed effects that absorb time-invariant differences across neighborhoods, remaining differences in baseline device coverage are differenced out.

3.2 SafeGraph Spend Patterns Data

To measure actual economic transactions, we use SafeGraph's Spend Patterns dataset, which aggregates anonymized credit and debit card transactions from a large opt-in consumer panel. SpendGraph reports daily spending by location, which we aggregate to weekly totals at the census block group level using spatial joins between POI coordinates and 2010 Census block group boundaries using ArcGIS Pro.

SafeGraph captures the dollar value of consumer purchases at millions of retail and service establishments. Because the panel is constructed from participating financial institutions and opt-in consumers, it does not represent the full universe of transactions. In particular, populations that rely more heavily on cash, informal financial services, or nontraditional banking arrangements—including some immigrant and undocumented households—are likely to be underrepresented in the data. As a result, SpendGraph measures changes in spending among the observed consumer panel rather than total population spending.

This limitation is especially relevant in the context of immigration enforcement. Undocumented immigrants and mixed-status households may be both more likely to avoid formal financial institutions and more likely to alter consumption behavior in response to enforcement-related fear. To the extent that these populations are underrepresented in transaction-based data, our estimates may understate the true magnitude of the spending response to immigration enforcement. In this sense, the spending results should be interpreted as a conservative lower bound on the total economic contraction experienced in immigrant-heavy neighborhoods.

Importantly, our difference-in-differences design focuses on changes over time within neighborhoods rather than on cross-sectional spending levels. As long as participation in the SpendGraph panel does not change discontinuously at the time of the ICE enforcement announcement in a way that differs systematically between high- and low-exposure neighborhoods, relative changes in spending remain informative about behavioral responses to enforcement. The absence of any detectable response in placebo tests further supports the interpretation that the observed spending declines reflect enforcement-related behavioral change rather than shifts in sample composition or data coverage.

Finally, because transaction-based spending measures can be sensitive to the presence or absence of a small number of establishments, we restrict the SpendGraph analysis to census block groups containing at least 10 Points of Interest (POIs). This restriction concentrates the analysis on commercially active neighborhoods and reduces spurious volatility driven by idiosyncratic activity in sparsely populated retail areas. Although this screen removes 64.7 percent of block groups in the Los Angeles–Orange County metropolitan area, it substantially improves the signal-to-noise ratio of the spending data and aligns the analysis with locations where consumer spending meaningfully reflects neighborhood economic activity.

4 Identification Strategy

Our analysis exploits a sharp escalation in federal immigration enforcement activity that was publicly announced by U.S. Immigration and Customs Enforcement (ICE) on May 14,

2025, when the agency reported arresting 239 individuals in a multi-day operation in the Los Angeles area.[2] In the weeks that followed, enforcement actions and their social consequences were widely reported across Southern California. In early June, protests erupted in response to ICE raids targeting immigrant communities, drawing hundreds of demonstrators into the streets of downtown Los Angeles and prompting confrontations with federal agents and calls for curfews.[3] In reaction to sustained protests, the Trump administration deployed thousands of National Guard troops and several hundred U.S. Marines to Los Angeles to assist with crowd control and protect federal facilities—an unprecedented mobilization of military forces in response to domestic immigration protests.[4] "Hundreds of Marines Deployed to Los Angeles Amid Protests Over Immigration Raids." These developments, and their extensive media coverage, ensured that enforcement activity remained salient throughout late May and June 2025 across both Los Angeles and Orange Counties, contributing to a climate of heightened fear and uncertainty among immigrant and mixed-status communities. Although ICE does not release detailed information on the precise locations or timing of individual arrests, the announcement itself was highly salient and widely covered by local and national media, making it a natural focal point for measuring behavioral responses to enforcement.

Importantly, the May 14 announcement did not mark a one-day event but the beginning of an extended period of visible enforcement activity. Reporting by the *Los Angeles Times* documents that ICE operations continued throughout late May and early June, including repeated workplace and neighborhood sweeps across Los Angeles and Orange Counties, and triggered large-scale protests and community mobilization.[5] Tensions escalated dramatically on June 6, when clashes between demonstrators and federal authorities led President Trump to federalize the California National Guard and deploy U.S. Marines to the region—an unprecedented intervention in a domestic immigration enforcement context. These events received intense media attention and ensured that enforcement activity, and the risks associated with it, were highly visible to residents throughout the metropolitan area.

This sustained and widely publicized enforcement campaign provides the basis for our difference-in-differences design. We treat the May 14 announcement as the moment at which perceived enforcement risk sharply increased, while recognizing that the actual enforcement

[2]U.S. Immigration and Customs Enforcement (2025), ICE press release, May 14, 2025.

[3]Los Angeles Times (2025), "ICE raids spark protests and clashes in downtown Los Angeles," *Los Angeles Times*, June 6, 2025, https://www.latimes.com/california/story/2025-06-06/la-me-ice-raids-protests-color-scene.

[4]Los Angeles Times (2025), "National Guard arrives in L.A. as fallout from immigration raids continues," *Los Angeles Times*, June 8, 2025, https://www.latimes.com/california/story/2025-06-08/national-guard-arrives-l-a-immigration-raids.

[5]Los Angeles Times (2025), "L.A. immigration raid protests prompt curfew, clashes," *Los Angeles Times*, June 10, 2025, https://www.latimes.com/california/story/2025-06-10/la-me-immigration-protests-curfew.

and its social consequences unfolded over the subsequent weeks. Our distributed-lag specifications are therefore designed to capture both the immediate and evolving behavioral responses to this enforcement climate.

Because we cannot directly observe where ICE agents conducted arrests, we proxy for exposure to enforcement by exploiting systematic differences in how enforcement is perceived across neighborhoods. Specifically, we classify census block groups by the share of residents born in Latin America, using data from the 2019-2023 5-year American Community Survey (ACS).[6] Block groups in the top decile of this distribution are treated as high-exposure neighborhoods, while those in the bottom decile serve as a comparison group.

This contrast has a clear theoretical basis. Immigration enforcement is not only a legal risk but a social and psychological one: fear, avoidance, and withdrawal from public space are most likely to arise in communities where residents, their family members, coworkers, or neighbors are themselves at risk of detention or deportation. In heavily Latin American–born neighborhoods, enforcement actions—even when not occurring on a specific block—are more likely to be interpreted as personally relevant and threatening, generating broad chilling effects on everyday mobility and consumption. By contrast, in neighborhoods with very small Latin American immigrant populations—such as places like Beverly Hills or Newport Beach—we would not expect ICE operations in the region to meaningfully alter perceived personal risk or day-to-day behavior, even when enforcement is occurring elsewhere in the metropolitan area.

Our identification strategy therefore compares changes in foot traffic and consumer spending in neighborhoods where enforcement should be highly salient and fear-inducing to changes in neighborhoods where enforcement should be largely irrelevant to residents' daily lives. This approach follows prior work emphasizing that immigration enforcement operates through perceived risk and spillovers, not just through direct contact with authorities (Watson 2014; Asad 2023), and allows us to isolate the causal impact of enforcement on local economic activity.

4.1 Distributed-Lag Difference-in-Differences with Calendar-Time Fixed Effects

We estimate a distributed-lag difference-in-differences model that absorbs common shocks tied to specific calendar weeks (such as holidays, seasonal patterns, macroeconomic conditions, or regional events) by including calendar-week fixed effects.

[6]Foreign Born status is available at the tract-level rather than the block group level. As such we allocate the share foreign born observed at the tract level to each block group within the tract

Specifically, we construct a treatment pulse equal to one for high-exposure neighborhoods in the enforcement announcement week and zero otherwise, and estimate leads and lags of this pulse interacted with treatment status. The model can be written as:

$$Y_{it} = \alpha_i + \lambda_t + \sum_{\tau \neq -1} \gamma_\tau \left(\text{HighFB}_i \times 1[t - T_0 = \tau] \right) + \varepsilon_{it}. \tag{1}$$

where λ_t are calendar-week fixed effects. In practice, this specification is implemented using distributed leads and lags of the treatment pulse. This model identifies treatment effects using variation relative to calendar time rather than event time, allowing us to absorb shocks that are common to all neighborhoods in a given week (such as holidays, macroeconomic conditions, or regional events).

4.2 Retail-Active Block Groups and POI-Based Sample Restriction

To focus on economically meaningful locations, we also estimate the distributed-lag specification on a restricted sample of block groups containing at least 10 points of interest (POIs). This restriction isolates neighborhood retail and commercial clusters and excludes predominantly residential or industrial block groups that contain few or no consumer-facing establishments.

Block groups with very small numbers of POIs can generate mechanically volatile visit and spending measures, where idiosyncratic changes at a single establishment can dominate aggregate patterns. Restricting attention to block groups with at least 10 POIs therefore sharpens the economic interpretation of the estimates by focusing on places where foot traffic plausibly corresponds to local commercial activity.

Importantly, this restriction also aligns the foot-traffic sample with the SpendGraph spending data, which are meaningful only in retail-active block groups. Estimating foot-traffic effects on this same subsample therefore provides a direct robustness check on whether mobility declines translate into revenue losses.

4.3 Placebo Tests Using a Shifted Event Date

As a falsification exercise, we conduct placebo tests for distributed-lag specifications for total visitors and spending by shifting the enforcement date backward by exactly one year. Under this placebo assignment, we re-estimate the models using the same treatment and control groups but redefining T_0 to correspond to the same calendar week in the prior year (2024).

If the estimated effects in the main analysis are driven by genuine behavioral responses to the ICE enforcement announcement rather than seasonal patterns or slow-moving trends, we should observe no systematic treatment effects under the placebo timing. In practice, the placebo estimates show no evidence of divergence between high- and low-exposure neighborhoods either before or after the placebo event. Lead and lag coefficients are small and statistically indistinguishable from zero, and joint tests fail to reject the null of no post-event effect. These placebo results reinforce the causal interpretation of the main findings.

4.4 Alternative Identification Strategy: Customer-Based Exposure at the Point-of-Interest Level

While our primary identification strategy classifies exposure at the residential block group level, this approach may mis-measure the degree to which immigration enforcement risk is actually relevant for specific establishments. In particular, businesses located in high–foreign-born neighborhoods may serve a substantial number of customers who do not themselves reside in immigrant communities and, therefore, may be less likely to alter their behavior in response to enforcement activity. Conversely, establishments located outside high–foreign-born neighborhoods may nonetheless serve immigrant customers who travel from elsewhere and whose consumption patterns may be highly sensitive to enforcement-related fear. To address this potential source of misclassification, we implement an alternative identification strategy that defines exposure based on the residential locations of a business's customers rather than the demographic characteristics of the neighborhood in which the business is located.

This alternative approach also allows us to shift the unit of analysis to the point-of-interest (POI) level. Doing so substantially increases the number of observational units, aligns the analysis with the native level of measurement in the mobility data, and permits a more direct mapping between enforcement-related behavioral responses and individual establishments. More broadly, defining exposure using observed customer behavior rather than business location or industry classification allows us to identify immigrant-serving establishments in a revealed-preference sense, based on who actually patronizes them. We are unable to use this alternative identification strategy to measure spending responses as the SpendGraph data does not contain the home block group locations of tracked consumers.

Customer-based exposure measure. One of the unique features of the Advan mobility data is the ability to link visitors who enter a specific POI to their residential home geography at the block group level. Advan assigns each device a home census block group based on

the modal nighttime location of the device over an extended observation window, following standard approaches in the mobility literature.[7] Home locations are inferred using repeated overnight pings and are independent of visits observed during the analysis period. We use this data to link POIs to the home block groups of their visitors. Specifically, we calculate, for each POI, the share of visits originating from residential areas with high immigrant concentration. As in the block-group level identification, we use 2019–2023 ACS estimates to measure the tract-level share of residents born in Latin America, assigning these values to block groups within each tract. We then rank block groups by this measure and define "high immigrant" block groups as those in the top decile of the distribution.

For each POI j, we compute the share of its visitors who reside in high-immigrant block groups, averaging over all weeks in calendar year 2024:

$$\text{ShareHighFB}_j^{2024} = \frac{\text{Visits to POI } j \text{ from high-immigrant block groups in 2024}}{\text{Total visits to POI } j \text{ in 2024}}.$$

Because this measure is constructed entirely using pre-event data, it is predetermined with respect to the 2025 enforcement escalation and invariant over the analysis period.

We define "high-exposure" establishments as those with at least 90 percent of customers from high Latin American foreign born block groups of ($\text{ShareHighFB}_j^{2024} > 0.90$). Our control group consists of POIs for which fewer than 10 percent of visitors in 2024 originated from high-immigrant block groups. This classification strategy ensures that treatment status reflects sustained customer composition rather than short-run fluctuations or post-treatment sorting. We restrict the sample to only those POIs that had at least 100 visitors in the pre-period, as many small POIs have highly volatile visit counts or report zeros for some weeks.

Difference-in-differences design at the POI level. We then estimate a distributed-lag difference-in-differences model using a POI-week panel. Let y_{jt} denote log foot traffic to POI j in week t. Our estimating equation is:

$$y_{jt} = \alpha_j + \gamma_t + \sum_{\tau \neq -1} \beta_\tau \left(\text{HighExposure}_j \times \mathbf{1}[t = \tau] \right) + \varepsilon_{jt},$$

where α_j are POI fixed effects, γ_t are week fixed effects, and τ indexes event time relative to the May 14, 2025 enforcement announcement, with $\tau = -1$ omitted as the reference period. Standard errors are clustered at the POI level.

[7] See Advan Research documentation, "Weekly Patterns," which describes the assignment of device home locations based on repeated nighttime observations: `https://docs.deweydata.io/docs/advan-research-weekly-patterns`. This approach is consistent with standard methods for inferring residential locations from mobile device data; see Cobo et al. (2024), *EPJ Data Science*.

This specification compares changes in visitation at establishments that primarily serve customers from immigrant communities to changes at establishments whose customer base is largely drawn from low-immigrant areas, before and after the enforcement escalation. By defining exposure based on customer home locations rather than business location alone, this approach isolates behavioral responses among establishments for which immigration enforcement risk is most likely to be salient to patrons, and provides a complementary test of the mechanisms underlying our main results.

5 Results

5.1 Foot Traffic Responses

Figure 1 reports distributed-lag difference-in-differences estimates for the full sample of block groups. Pre-treatment leads are small and statistically insignificant, providing evidence against differential pre-trends. Post-treatment coefficients reveal a sustained decline in foot traffic in high-exposure neighborhoods beginning roughly two to three weeks after the announcement.

Figure 1: Distributed-Lag Difference-in-Differences Estimates of ICE Enforcement on Foot Traffic (All Block Groups)

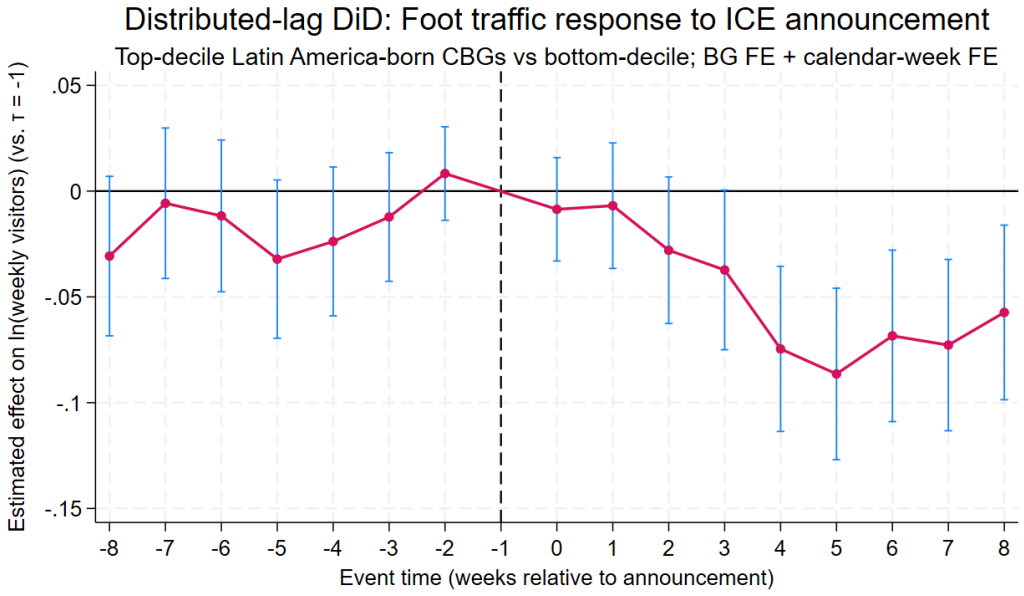
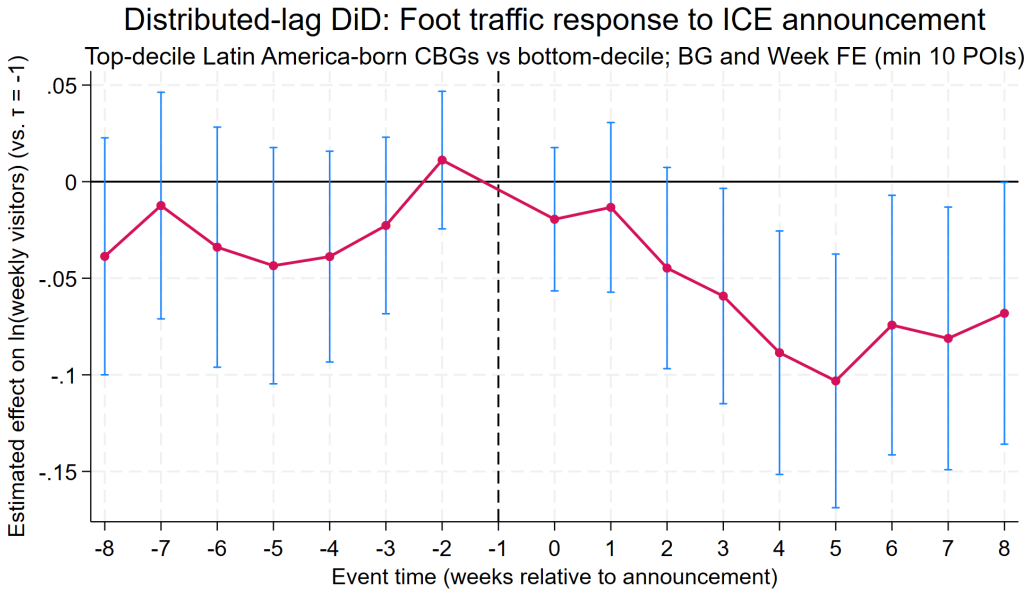


Figure 2 presents the same distributed-lag estimates, but restricted to block groups containing at least 10 points of interest. This subsample isolates retail-active neighborhoods

and corresponds to the sample used in the consumer spending analysis below. The resulting pattern is even sharper: following the ICE announcement, foot traffic in immigrant-heavy commercial corridors declines by roughly 8–10 percent within four to six weeks and remains depressed through the end of the event window.

Figure 2: Distributed-Lag Difference-in-Differences Estimates of ICE Enforcement on Foot Traffic (Block Groups with ≥10 POIs)



The stronger effects in Figure 2 indicate that the behavioral response to immigration enforcement is concentrated in neighborhood retail environments rather than in primarily residential or industrial areas. This finding also provides a critical link to the spending results: when people stop visiting commercial districts, businesses lose revenue.

Of particular policy relevance is the magnitude of the effect eight weeks after the announcement. In retail-active neighborhoods, foot traffic remains approximately 7–9 percent below its pre-announcement baseline, implying persistent reductions in customer flows well after the enforcement operation itself has ended.

5.2 Consumer Spending Responses

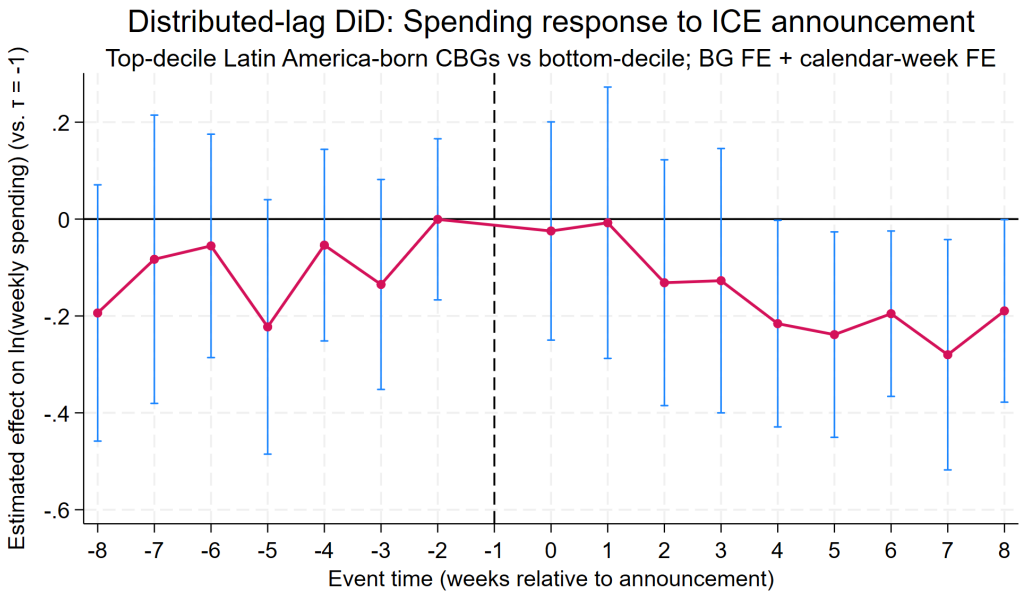
We next examine whether the decline in foot traffic is accompanied by corresponding changes in consumer spending using SafeGraph's Spend Patterns data. We estimate the same distributed-lag difference-in-differences specification used for mobility, but with the logarithm of weekly consumer spending in each census block group as the outcome.

Figure 3 reports the estimated dynamic treatment effects. As in the mobility results, there is no evidence of differential pre-trends between high- and low-exposure neighborhoods in the weeks preceding the ICE announcement. Following the announcement, however, consumer spending in immigrant-heavy neighborhoods declines sharply relative to the comparison group.

The magnitude of the spending response is substantial. By four to six weeks after the announcement, weekly spending in high-exposure neighborhoods is approximately 20–25 percent below its pre-announcement baseline. The persistence of the effect through at least eight weeks indicates that the behavioral response to enforcement is not merely a short-lived shock, but rather a sustained contraction in local economic activity.

The close correspondence between the spending and foot-traffic results implies that reduced mobility translates directly into lost revenue for neighborhood businesses, rather than reflecting substitution across locations or timing of purchases. Together, these findings suggest that immigration enforcement generates economically meaningful spillovers onto local commercial ecosystems.

Figure 3: Distributed-Lag Difference-in-Differences Estimates of ICE Enforcement on Consumer Spending



5.3 Placebo Tests

To further assess the causal interpretation of our results, we conduct placebo tests for both mobility and consumer spending by shifting the enforcement event date backward by exactly one year. We then re-estimate the same distributed-lag difference-in-differences models using this placebo event week, while keeping the treated and control neighborhoods unchanged.

If the main results reflect true behavioral responses to the ICE enforcement announcement rather than seasonal patterns, slow-moving trends, or spurious correlations, then no systematic effects should appear around the placebo date.

Figure 4 presents placebo estimates for overall foot traffic. The estimated coefficients fluctuate narrowly around zero both before and after the placebo event, with no sustained divergence between high- and low-exposure neighborhoods. There is no evidence of any systematic mobility response when no enforcement actually occurred.

Figure 4: Placebo Distributed-Lag Difference-in-Differences: Foot Traffic (Event Shifted Back One Year)

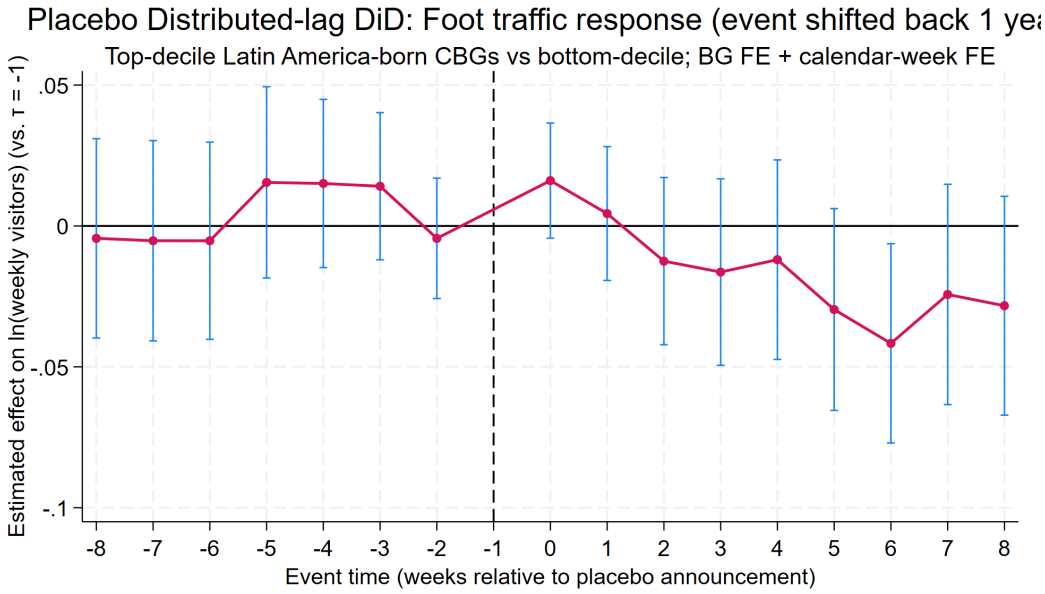
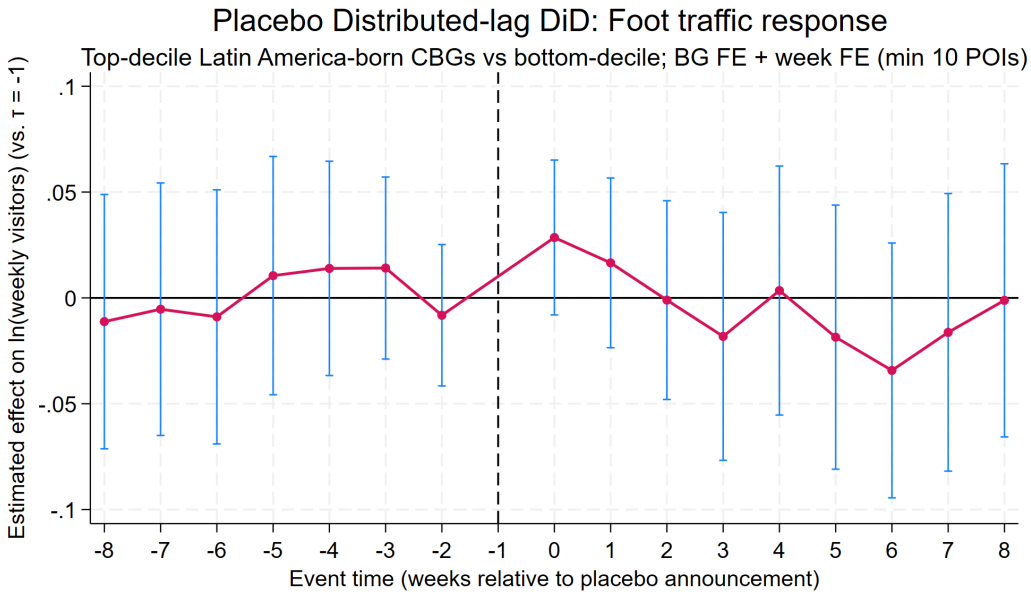


Figure 5 reports the placebo estimates for foot traffic in neighborhood retail block groups, defined as census block groups containing at least 10 points of interest. This sample isolates locations where foot traffic directly reflects neighborhood commercial activity and corresponds to the sample used in the spending analysis. As in the overall mobility placebo, coefficients remain small and statistically indistinguishable from zero throughout the event window, with no sign of a spurious treatment effect at the placebo date.

Figure 6 presents the corresponding placebo estimates for consumer spending. Spending

Figure 5: Placebo Distributed-Lag Difference-in-Differences: Foot Traffic in Retail Block Groups (Min. 10 POIs)



responses likewise exhibit no systematic change following the placebo announcement: estimates are centered close to zero, noisy, and display no persistent divergence between high- and low-exposure neighborhoods.

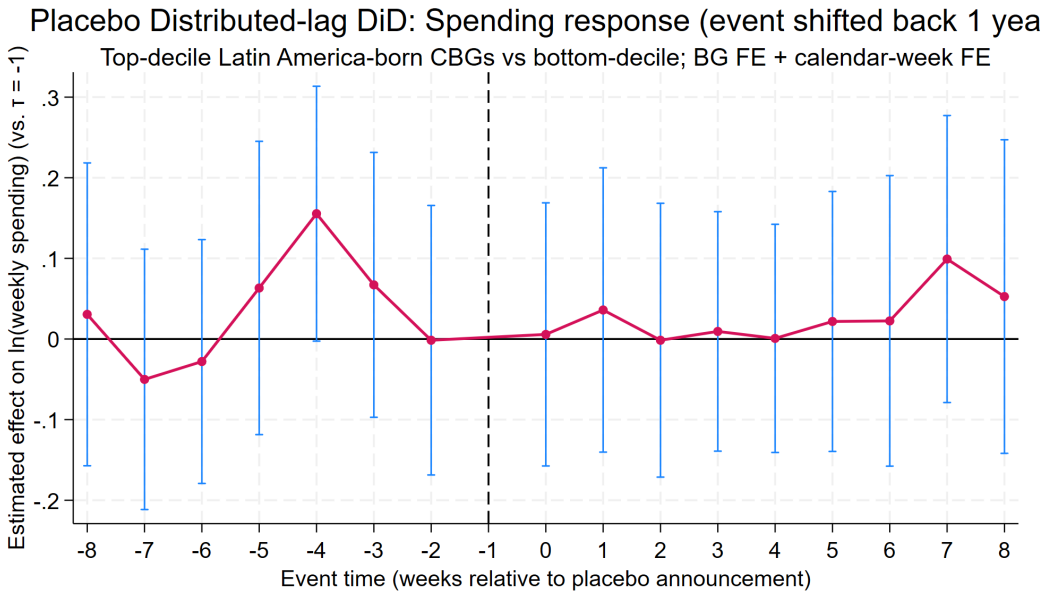
Formal joint tests reinforce the visual evidence. For foot traffic, a joint F-test of all post-placebo coefficients fails to reject the null of no effect, indicating no systematic change in visits following the placebo event. For consumer spending, the joint post-event test likewise yields no evidence of an effect (Prob ¿F.$p = 0.96$). These results rule out the possibility that the main findings are driven by seasonality, long-run trends, or coincident calendar-week shocks.

Together, the placebo tests provide strong falsification evidence in support of a causal interpretation: when ICE enforcement did not occur, neither foot traffic nor consumer spending exhibited any systematic response.

5.4 Alternative Identification Strategy: POI-Level Evidence Based on Customer Home Locations

As a complementary test of our main results, we re-estimate the distributed-lag difference-in-differences model using the alternative identification strategy based on customer home locations and POI-level outcomes. Rather than classifying exposure based on the demographic characteristics of the neighborhood in which a business is located, this approach

Figure 6: Placebo Distributed-Lag Difference-in-Differences: Consumer Spending (Event Shifted Back One Year)



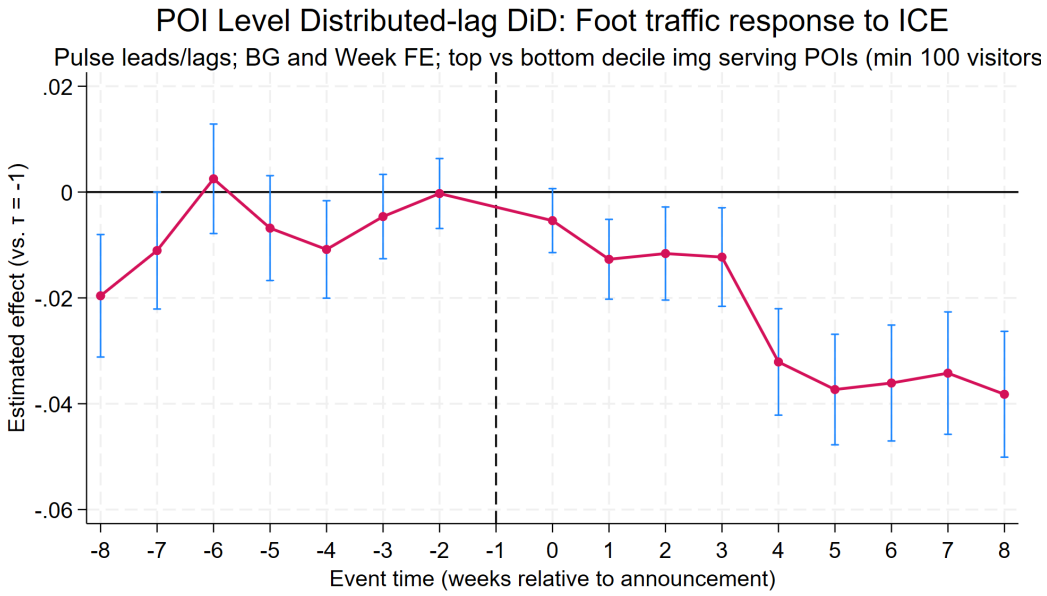
defines treatment using the residential origins of a POI's customers, focusing directly on establishments that disproportionately serve patrons from immigrant communities.

Figure 7 reports the corresponding distributed-lag estimates from this POI-level specification, comparing establishments in the top decile of immigrant customer exposure to those with fewer than 10 percent of visitors originating from high-immigrant residential areas. As in the block-group analyses, pre-treatment coefficients are small and statistically indistinguishable from zero, providing no evidence of differential pre-trends between high- and low-exposure establishments prior to the announcement.

Following the enforcement announcement, foot traffic at high-exposure POIs declines relative to the control group, with effects emerging gradually over the subsequent weeks. The magnitude of the response increases over time, reaching declines on the order of 3–4 percent by approximately four to six weeks after the announcement, and remaining persistently negative through the end of the event window. The estimated confidence intervals in this specification are notably tighter than in the block-group analyses, reflecting the substantially larger number of observational units and the use of POI-level outcomes, while the overall dynamic pattern closely mirrors the neighborhood-level results.

Importantly, these findings indicate that the behavioral response to immigration enforcement is not driven solely by the demographic composition of commercial neighborhoods, but is also evident when exposure is defined by who businesses actually serve. Establish-

Figure 7: POI-Level Distributed-Lag Difference-in-Differences Estimates of ICE Enforcement on Foot Traffic



ments that rely more heavily on customers residing in immigrant communities experience sharper and more persistent declines in visitation following enforcement activity, even when located outside immigrant-dense neighborhoods. This pattern reinforces the interpretation that immigration enforcement operates through fear and avoidance among immigrant households, reducing everyday consumption and mobility in ways that propagate through local commercial networks.

Taken together with the block-group results and the placebo tests, the POI-level evidence strengthens the causal interpretation of our findings and underscores the importance of customer composition—rather than business location alone—in mediating the economic spillovers of immigration enforcement.

6 Policy and Economic Implications

The declines in foot traffic and spending documented here imply meaningful economic losses for neighborhood-serving businesses in immigrant communities. When aggregated across thousands of establishments and multiple weeks, even modest percentage declines translate into large dollar losses.

These revenue reductions also have fiscal consequences. In California, local governments receive a share of sales tax revenue from taxable retail activity. Sustained enforcement-driven reductions in spending therefore reduce municipal revenues that support public services, including policing, schools, and infrastructure.

To translate our difference-in-differences estimates into policy-relevant economic magnitudes, we combine the estimated percentage reductions in consumer spending with observed baseline spending levels and administrative data on taxable sales. This allows us to quantify the revenue losses experienced by neighborhood businesses and the corresponding fiscal impacts for local governments in Los Angeles and Orange Counties.

6.1 Baseline Spending in Treated Retail Neighborhoods

We begin by computing pre-enforcement baseline spending using SafeGraph's Spend Patterns data. To focus on neighborhood retail districts rather than primarily residential areas, we restrict the analysis to census block groups (BGs) containing at least 10 points of interest (POIs). This restriction yields 197 treated retail BGs in Los Angeles County and 22 in Orange County, for a total of 219 treated retail BGs metro-wide.

Within this treated retail universe, average pre-period weekly spending equals $2.30 million per block group in Los Angeles County and $2.13 million in Orange County. At the establishment level, this corresponds to approximately $100,198 and $92,626 per POI per week, respectively. These values represent typical levels of neighborhood-serving retail activity prior to the ICE enforcement announcement.

6.2 Post-Enforcement Spending Losses from DiD Estimates

Our distributed-lag difference-in-differences model implies sustained declines in consumer spending in immigrant-heavy neighborhoods following the May 14, 2025 ICE enforcement announcement. Summing the estimated post-treatment coefficients over the eight-week post period yields cumulative spending declines on the order of 20–25 percent.

Applying these percentage reductions to the observed baseline spending levels implies average eight-week losses of $2.88 million per treated retail block group in Los Angeles

County and $2.66 million in Orange County. At the establishment level, this corresponds to losses of approximately $125,147 and $115,690 per POI, respectively.

6.3 Scaling to the Full Retail Economy

SafeGraph's Spend Patterns data capture spending by a large but incomplete panel of consumers. To translate these panel-based amounts into economy-wide retail sales, we scale up using administrative data on taxable sales from the California Department of Tax and Fee Administration (CDTFA).

For each county, we compute an inflation factor equal to the ratio of total taxable sales to total SpendGraph transactions:

$$\text{Inflation Factor}_c = \frac{\text{Total Taxable Sales}_c}{\text{Total SpendGraph Spending}_c}.$$

Using 2023–2024 county totals, these factors equal 328 for Los Angeles County and 303 for Orange County. These multipliers convert SpendGraph dollars into population-level retail sales and are applied to all spending-based impact calculations. Thus each dollar in the Spend Patterns dataset represents $328 in total spending for LA County.$

6.4 Aggregate Economic and Fiscal Impacts

Multiplying the average block-group losses by the number of treated retail BGs and applying the county-specific inflation factors yields the total economic impacts reported in Table **??**. Over the eight-week post-enforcement period, we estimate:

- $567 million in lost retail sales in Los Angeles County,
- $58.5 million in Orange County, and
- $626 million metro-wide.

We then translate these revenue losses into fiscal impacts using each county's statutory sales tax rate (9.75 percent in Los Angeles County and 7.75 percent in Orange County). This yields estimated losses in sales tax revenue of $55.3 million in Los Angeles County and $4.5 million in Orange County over just eight weeks, for a combined metro-wide fiscal loss of nearly $60 million.

These figures indicate that immigration enforcement generates not only substantial private-sector revenue losses for neighborhood businesses, but also large negative spillovers onto local public finances.

Table 1: Estimated Economic and Fiscal Impacts of ICE Enforcement in Treated Retail Block Groups

	Los Angeles County	**Orange County**	**Metro Area**
Pre-enforcement baseline			
Average weekly spending per treated BG	$2,304,594	$2,130,428	$2,217,511
Average weekly spending per POI	100,198	92,626	96,412
Eight-week post-enforcement losses (per unit)			
Average loss per treated BG	($2,878,437)	($2,660,905)	($2,769,671)
Average loss per POI	(125,147)	(115,690)	(120,419)
Treated retail universe			
Number of treated retail BGs	197	22	219
Aggregate impacts over eight weeks			
Total lost sales	($567,052,179)	($58,539,905)	($625,592,085)
Lost sales tax revenue	($55,287,588)	($4,536,843)	($59,824,430)

Note: Author's analysis of SafeGraph Spend Patterns data and county-level taxable sales data from the California Department of Tax and Fee Administration (CDTFA), `https://cdtfa.ca.gov/DataPortal/dataset.htm?url=TaxSalesByCounty`. Dollar values reflect inflation-adjusted scaling from SpendGraph panel totals to countywide taxable sales using county-specific inflation factors.

Because California's sales tax collections are split between the state and local governments, these revenue losses are shared across levels of government: in Los Angeles County, roughly 40 percent of foregone sales tax accrues to the State of California and about 60 percent to local governments, while in Orange County—where district taxes are lower—over half of the loss falls on the state and the remainder on local governments. Our estimates of fiscal impacts for local governments in Los Angeles County and Orange County are therefore conservative, since many cities levy additional district taxes that increase the local share of sales tax above the countywide averages used here.

7 Conclusion

This paper provides new evidence that immigration enforcement reverberates through local economies by reshaping everyday mobility and consumption behavior. Using two independent high-frequency datasets, we show that a major ICE enforcement announcement was followed by a sustained decline in both foot traffic and consumer spending in immigrant-heavy areas of Los Angeles and Orange County. These effects emerge gradually, persist well beyond the initial enforcement period, and are concentrated in neighborhood commercial environments.

Our primary results rely on a block-group–level identification strategy that exploits variation in the residential concentration of Latin American–born populations. We complement this approach with an alternative identification strategy that defines exposure based on the residential origins of a business's customers and estimates effects at the point-of-interest level. This customer-based, POI-level analysis yields qualitatively similar dynamic responses, with tighter confidence intervals reflecting the larger number of observational units and the native level of measurement in the mobility data. Together, these findings indicate that the economic impacts of enforcement are not driven solely by where businesses are located, but by whom they serve.

Across specifications, placebo tests show no evidence of spurious pre-trends or coincident seasonal effects, strengthening the causal interpretation of the results. The consistency of the findings across outcomes, units of analysis, and identification strategies highlights a key mechanism: immigration enforcement generates widespread fear and avoidance that alters routine economic behavior, producing spillover effects that extend well beyond the individuals directly targeted by enforcement actions.

From a policy perspective, these results underscore that immigration enforcement carries substantial economic costs for local communities. Reduced foot traffic and consumer spending translate into lost business revenue, diminished sales tax collections, and weaker

local fiscal capacity. The Trump Administration's ICE enforcement policies therefore impose significant economic burdens on state and local governments that compound the very real human, emotional, and psychological harms experienced by immigrant and mixed-status communities. These broader economic spillovers should be central to debates over the design and implementation of immigration enforcement policy.

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Table 2: Distributed-Lag Difference-in-Differences Estimates of ICE Enforcement on Foot Traffic and Consumer Spending: Main and Placebo Results

	Main (All BGs)			Main (Min. 10 POIs)			Placebo (Min. 10 POIs)		
Event time τ	ln(visitor)	ln(visits)	ln(spending)	ln(visitor)	ln(visits)	ln(spending)	ln(visitor)	ln(visits)	ln(spending)
Min. 10 POIs	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
	-0.033*	-0.035***	-0.041	-0.039	-0.050**	-0.194	-0.011	-0.024	0.031
-8	(0.019)	(0.013)	(0.100)	(0.031)	(0.020)	(0.135)	(0.031)	(0.020)	(0.096)
	-0.008	-0.021*	-0.143	-0.012	-0.036**	-0.083	-0.005	-0.034*	-0.050
-7	(0.018)	(0.011)	(0.100)	(0.030)	(0.017)	(0.152)	(0.030)	(0.021)	(0.082)
	-0.014	-0.024**	-0.135	-0.034	-0.046**	-0.055	-0.009	-0.028	-0.028
-6	(0.018)	(0.012)	(0.114)	(0.032)	(0.020)	(0.118)	(0.031)	(0.021)	(0.077)
	-0.034*	-0.037***	-0.066	-0.043	-0.054***	-0.223*	0.011	-0.006	0.063
-5	(0.019)	(0.013)	(0.103)	(0.031)	(0.020)	(0.134)	(0.029)	(0.020)	(0.093)
	-0.026	-0.032***	-0.113	-0.039	-0.049***	-0.054	0.014	-0.005	0.155*
-4	(0.018)	(0.012)	(0.105)	(0.028)	(0.018)	(0.101)	(0.026)	(0.019)	(0.081)
	-0.013	-0.020**	0.022	-0.023	-0.036**	-0.135	0.014	-0.003	0.067
-3	(0.015)	(0.010)	(0.108)	(0.023)	(0.015)	(0.111)	(0.022)	(0.016)	(0.084)
	0.007	-0.002	-0.142	0.011	-0.008	-0.001	-0.008	-0.005	-0.002
-2	(0.011)	(0.007)	(0.100)	(0.018)	(0.011)	(0.085)	(0.017)	(0.014)	(0.085)
	-0.012	-0.023**	-0.151	-0.019	-0.037***	-0.025	0.029	0.028**	0.006
0	(0.013)	(0.009)	(0.106)	(0.019)	(0.012)	(0.115)	(0.019)	(0.014)	(0.083)
	-0.009	-0.025**	-0.069	-0.013	-0.038***	-0.008	0.017	0.005	0.036
1	(0.016)	(0.011)	(0.104)	(0.022)	(0.014)	(0.143)	(0.020)	(0.014)	(0.090)
	-0.032*	-0.039***	-0.111	-0.045*	-0.055***	-0.131	-0.001	-0.003	-0.001
2	(0.018)	(0.012)	(0.106)	(0.027)	(0.018)	(0.129)	(0.024)	(0.017)	(0.087)
	-0.043**	-0.048***	-0.037	-0.059**	-0.068***	-0.127	-0.018	-0.012	0.009
3	(0.020)	(0.013)	(0.112)	(0.028)	(0.018)	(0.139)	(0.030)	(0.020)	(0.076)
	-0.080***	-0.085***	-0.196**	-0.089***	-0.092***	-0.216**	0.003	-0.003	0.001
4	(0.020)	(0.013)	(0.099)	(0.032)	(0.020)	(0.109)	(0.030)	(0.021)	(0.072)
	-0.092***	-0.096***	0.058	-0.103***	-0.110***	-0.239**	-0.019	-0.014	0.022
5	(0.021)	(0.015)	(0.106)	(0.033)	(0.022)	(0.108)	(0.032)	(0.023)	(0.082)
	-0.074***	-0.076***	-0.155	-0.074**	-0.080***	-0.195**	-0.034	-0.025	0.022
6	(0.021)	(0.014)	(0.108)	(0.034)	(0.022)	(0.087)	(0.031)	(0.022)	(0.092)
	-0.078***	-0.071***	-0.242**	-0.081**	-0.077***	-0.280**	-0.016	-0.028	0.099
7	(0.021)	(0.014)	(0.115)	(0.035)	(0.022)	(0.121)	(0.033)	(0.024)	(0.091)
	-0.062***	-0.064***	-0.107	-0.068**	-0.073***	-0.189*	-0.001	-0.023	0.053
8	(0.021)	(0.014)	(0.107)	(0.035)	(0.021)	(0.096)	(0.033)	(0.023)	(0.099)
N	28234	28234	14426	15874	15874	1724	15880	15880	1744

Notes: Foot traffic and visit outcomes are constructed from Advan Research high-frequency mobile device data, aggregated to the census block group–week level. Consumer spending outcomes are derived from SafeGraph Spend Patterns data, aggregated to weekly totals within census block groups. All models are estimated using distributed-lag difference-in-differences specifications with census block group fixed effects and week fixed effects. Standard errors are clustered at the census block group level. Coefficients report log-point effects relative to the omitted period ($\tau = -1$). Placebo estimates are generated by shifting the enforcement event week to the same calendar week in 2024 and re-estimating the full distributed-lag specification. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.