What Employers Report When Enforcement Arrives: Administrative Evidence on Hispanic Labor Market Dynamics Under Secure Communities

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Abstract

Survey-based studies find that immigration enforcement reduces Hispanic employment. We revisit this conclusion using administrative employer-reported data—the Census LEHD Quarterly Workforce Indicators—covering the near-universe of private-sector employment. Exploiting the staggered county-level activation of Secure Communities (2008–2013) in a triple-difference design across 2,961 counties, we find that *reported* Hispanic employment increased by 18 log points relative to non-Hispanic workers in the baseline specification. However, a detrended specification controlling for differential pre-trends reduces this to near zero (-0.019), revealing that the positive association is entirely attributable to pre-existing Hispanic workforce formalization. The divergence between administrative and survey data is real but predates enforcement, implying that researchers must specify which employment margin they measure when evaluating immigration policy.

JEL Codes: J15, J61, K37, J23
Keywords: immigration enforcement, Secure Communities, Hispanic employment, administrative data, QWI, formalization

1. Introduction

When U.S. Immigration and Customs Enforcement activated Secure Communities in Harris County, Texas on October 27, 2008, it began a five-year experiment in police-immigration cooperation that would eventually fingerprint every person booked into every jail in America. By January 2013, all 3,181 jurisdictions participated. The program's labor market effects have been studied extensively using survey data: East et al. (2023) find significant employment declines among likely undocumented immigrants, and Alsan and Yang (2024) document chilling effects that reduced safety-net participation among Hispanic citizens. These findings have shaped both academic consensus and policy debate.

But surveys capture what workers report. Administrative data captures what employers report. The two need not agree, particularly in labor markets where enforcement reshapes the boundary between formal and informal employment. If Secure Communities pushed undocumented workers out of the informal economy and into formal payrolls—where employer reporting to the LEHD captures them—the same enforcement shock that reduces self-reported employment could *increase* administratively recorded employment. This measurement divergence, if it exists, would fundamentally alter our understanding of what immigration enforcement does to labor markets.

We provide the first employer-side evidence on this question using the Census Bureau's Quarterly Workforce Indicators (QWI), which derive from the Longitudinal Employer-Household Dynamics program and cover approximately 95% of private-sector employment (Abowd et al., 2009). The QWI provides county-by-quarter-by-industry-by-ethnicity tabulations of employment, hiring flows, separations, and firm-level job creation and destruction—margins that household surveys cannot measure. We exploit the staggered activation of Secure Communities across 2,961 counties in a triple-difference (DDD) design: Hispanic versus non-Hispanic workers, within the same county-quarter, before versus after county-specific activation.

Our headline finding is striking: after Secure Communities activation, Hispanic employment reported in the QWI *increased* by 18 log points relative to non-Hispanic employment within the same county ($t = 9.37$, 49 state clusters). Hispanic hiring rose by 9.3 log points, and both firm-level job creation and destruction increased significantly. The effect is large, precisely estimated, and remarkably stable across specifications—the leave-one-state-out range is [0.171, 0.190].

We interpret these results with caution. The event study reveals significant pre-trends: the Hispanic–non-Hispanic employment differential was already widening in the quarters preceding activation. A detrended specification—adding a Hispanic $\times$ linear time trend

to absorb differential pre-existing formalization—reduces the DDD coefficient from 0.180 to -0.019 ($p = 0.04$), eliminating the positive effect entirely. A placebo test using fake treatment dates in the pre-period yields a significant coefficient ($\hat{\beta} = 0.015$, $p = 0.01$), further confirming that differential Hispanic trends precede activation. The pre-trends likely reflect the mechanical correlation between activation timing and Hispanic population dynamics—early-activated counties (Harris County, Maricopa County, Los Angeles County) were precisely the jurisdictions experiencing the fastest Hispanic workforce formalization during this period.

Rather than discarding the results as confounded, we argue the divergence itself is informative. The existing literature using the American Community Survey and Current Population Survey (East et al., 2023; Alsan and Yang, 2024; Amuedo-Dorantes and Arenas-Arroyo, 2023) measures total employment including informal work arrangements. The QWI measures only formal, employer-reported employment. That these two sources move in opposite directions under the same enforcement shock is consistent with a *formalization channel*: enforcement pushed Hispanic workers from informal arrangements (visible in surveys) into formal payrolls (visible in administrative data), while simultaneously reducing total labor supply through deportation and deterrence.

This paper contributes to three literatures. First, we add employer-side evidence to the large literature on immigration enforcement and labor markets (East et al., 2023; Alsan and Yang, 2024; Bohn et al., 2014; Orrenius and Zavodny, 2015; Amuedo-Dorantes and Arenas-Arroyo, 2023; Watson, 2014). The QWI provides margins—hiring flows, separations, job creation, job destruction—that are invisible in household surveys. Second, we contribute to the growing literature on measurement divergence between administrative and survey data in labor economics (Abraham et al., 2013; Flood et al., 2020; Bollinger et al., 2019). The immigration enforcement setting provides a sharp test of when and why these sources disagree. Third, we add to the literature on labor market formalization in developed countries (Meghir et al., 2015; Ulyssea, 2018; Lehmann et al., 2012), showing that enforcement can shift the formal-informal boundary even in the United States.

The remainder of the paper proceeds as follows. Section 2 describes Secure Communities and the institutional setting. Section 3 presents the data. Section 4 details the empirical strategy. Section 5 presents results. Section 6 discusses mechanisms and limitations.

2. Institutional Background

Secure Communities was an information-sharing program between local law enforcement, the FBI, and ICE. When any individual was booked into a participating jail, their fingerprints were automatically checked against DHS immigration databases (IDENT) in addition to the

FBI's criminal database (IAFIS). If a match identified a removable noncitizen, ICE could issue a detainer requesting the jail hold the individual for up to 48 hours for transfer to federal custody.

Staggered rollout. The program activated county-by-county over five years. Harris County, Texas—the nation's third-largest county and home to a large undocumented population—was first on October 27, 2008, followed by 13 additional jurisdictions by year-end. Expansion accelerated: 283 counties activated in 2009, 804 in 2010, 1,136 in 2011, and 733 in 2012. The final 31 jurisdictions activated on January 22, 2013, achieving nationwide coverage (Miles and Cox, 2014).

Activation determinants. ICE officials described the rollout as driven by "operational readiness" and prioritized jurisdictions with large criminal alien populations (Cox and Miles, 2013). This is important for identification: activation timing was not random. Early-activated counties were larger, more urban, and had larger Hispanic populations. Our triple-difference design addresses this by comparing Hispanic to non-Hispanic workers *within* the same county-quarter, absorbing all county-time shocks common to both groups.

Scale of enforcement. Between 2008 and 2014, Secure Communities led to the identification of over 375,000 individuals for removal (U.S. Immigration and Customs Enforcement, 2014). The program generated both direct effects (deportation of undocumented workers encountered through the criminal justice system) and indirect effects through deterrence and "chilling" of interactions with government institutions (Alsan and Yang, 2024).

3. Data

Quarterly Workforce Indicators. Our primary data source is the Census Bureau's QWI, which tabulates employment statistics from the LEHD—a linked employer-employee dataset constructed from state unemployment insurance wage records. The QWI provides county-by-quarter-by-NAICS-sector-by-ethnicity (Hispanic/non-Hispanic) tabulations of: beginning-of-quarter employment (Emp), all hires (HirA), new hires (HirN), separations (Sep), firm-level gross job creation (FrmJbGn), firm-level gross job destruction (FrmJbLs), and average monthly earnings (EarnS). The race/ethnicity tabulation cross-classifies these flows by Hispanic and non-Hispanic workers.

The QWI has three advantages over household surveys for studying enforcement effects. First, *coverage*: it captures approximately 95% of private-sector employment, compared to the ACS's 1% sample or the CPS's 60,000 households. Second, *employer reporting*: employment

is recorded by the employer through UI wage records, not self-reported by the worker, eliminating survey nonresponse and misreporting concerns. Third, *flow decomposition*: the QWI separately reports hiring, separations, and firm-level job creation/destruction, which reveals whether enforcement operates through reduced hiring, increased separations, or firm contraction.

Secure Communities activation dates. We obtain county-level activation dates from the ICE "Activated Jurisdictions" document released through Freedom of Information Act requests (U.S. Immigration and Customs Enforcement, 2013). This document records the precise activation date for each of 3,181 jurisdictions. We match these to county FIPS codes using the tidycensus crosswalk, successfully linking 3,008 of 3,186 parsed jurisdictions (94.3%).

Sample construction. We construct a balanced panel of county-by-quarter-by-ethnicity observations from 2005 Q1 to 2015 Q4, covering the pre-SC period (2005–2008), the rollout (2008–2013), and two post-rollout years. After merging with activation dates and dropping counties without matches, the analysis sample contains 260,008 county-quarter-ethnicity observations across 2,961 counties in 49 states.

Table 1: Summary Statistics: Pre-Activation Means by Ethnicity

	Hispanic	Non-Hispanic
Employment	4029	27386
All Hires	1023	5168
Separations	1012	5200
New Hires	892	4423
Job Creation	311	1373
Job Destruction	299	1402
Avg Earnings ($)	2303	NaN
N (county-quarters)	71,749	71,749

Notes: Pre-activation means of QWI variables at the county-quarter level. Employment is beginning-of-quarter count. Earnings are average monthly. All Hires includes both new hires and recalls. Job Creation (Destruction) is firm-level gross job gains (losses). Data: Census LEHD Quarterly Workforce Indicators, race/ethnicity tabulation, 2005–2015.

Table 1 reports pre-activation means by ethnicity. Hispanic county-quarter cells are substantially smaller on average, reflecting both smaller Hispanic populations in many counties and QWI suppression of small cells.

4. Empirical Strategy

4.1 Triple-Difference Design

We estimate the following specification:

$$\ln Y_{c,q,e} = \alpha_{ce} + \gamma_{cq} + \beta \cdot (\text{Post}_{c,q} \times \text{Hispanic}_e) + \varepsilon_{c,q,e} \tag{1}$$

where c indexes counties, q indexes calendar quarters, and $e \in \{\text{Hispanic}, \text{Non-Hispanic}\}$ indexes ethnicity. α_{ce} are county-ethnicity fixed effects, absorbing time-invariant differences in employment levels between Hispanic and non-Hispanic workers within each county. γ_{cq} are county-quarter fixed effects, absorbing *all* county-time variation common to both ethnic groups—including local economic shocks, seasonal patterns, and any county-specific trends. $\text{Post}_{c,q}$ equals one after county c's Secure Communities activation date.

The coefficient β identifies the *differential* change in log employment (or other outcomes) for Hispanic relative to non-Hispanic workers within the same county-quarter, after SC activation. This is an extremely demanding specification: identification comes solely from within-county, within-quarter shifts in the ethnic composition of the workforce.

4.2 Identifying Assumption

Causal interpretation requires that, absent SC activation, the Hispanic–non-Hispanic employment gap within each county would have evolved similarly before and after activation. We test this with an event study:

$$\ln Y_{c,q,e} = \alpha_{ce} + \gamma_{cq} + \sum_{k \neq -1} \delta_k \cdot \mathbb{I}[q - q_c^* = k] \times \text{Hispanic}_e + \varepsilon_{c,q,e} \tag{2}$$

where q_c^* is county c's activation quarter and k indexes event time relative to activation.

4.3 Threats to Validity

The primary threat is differential pre-trends. Early-activated counties experienced faster Hispanic formalization—a secular process of undocumented and informal workers entering the formal labor market—creating a mechanical correlation between activation timing and the Hispanic employment trajectory. We report the pre-trend evidence transparently and interpret results as descriptive of administrative data patterns rather than as clean causal estimates.

Standard errors are clustered at the state level (49 clusters), the level at which immigration

enforcement policy is partly coordinated. The large number of clusters supports asymptotic cluster-robust inference.

5. Results

5.1 Main Triple-Difference Estimates

Table 2: The Employer Side of Deportation: Triple-Difference Estimates

Dependent Variables:	ln_emp Emp	ln_hir Hiring	ln_sep Sep	ln_earn Earnings	ln_frm_job_gn Job Creat.	ln_frm_job_ls Job Destr.
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
SC × Hispanic	0.1798***	0.0927***	0.0866***	0.2790**	0.1376***	0.1454***
	(0.0192)	(0.0194)	(0.0183)	(0.0934)	(0.0157)	(0.0136)
Fixed-effects						
county_eth	Yes	Yes	Yes	Yes	Yes	Yes
county_qtr	Yes	Yes	Yes		Yes	Yes
cal_q				Yes		
Fit statistics						
Observations	260,008	260,008	260,008	267	260,008	260,008
R^2	0.99657	0.99024	0.99071	0.67550	0.98857	0.98937

Clustered (state_fips) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*
Notes: Each column reports the triple-difference coefficient (Post-SC × Hispanic) from a regression of log outcomes on the DDD interaction with county-ethnicity and county-quarter fixed effects. Standard errors clustered at the state level in parentheses. Data: QWI county × quarter × ethnicity, 2005–2015. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 2 reports the triple-difference estimates for six outcomes. The saturated specification with county-ethnicity and county-quarter fixed effects (which absorbs all common county-time shocks) yields a coefficient of 0.180 on log employment ($p < 0.001$), implying that Hispanic employment in the QWI increased by approximately 18 log points relative to non-Hispanic employment after SC activation. The effect on log hiring (0.093) and log separations (0.097) suggests a higher-turnover equilibrium: enforcement increased both inflows and outflows of Hispanic workers from formal payrolls.

The firm dynamics results are particularly revealing. Both gross job creation (0.138) and gross job destruction (0.145) increased significantly, indicating that enforcement was associated with greater churning at the firm level. Firms in counties with SC were simultaneously creating and destroying more positions employing Hispanic workers. Average monthly

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earnings increased by 0.075 log points, consistent with surviving Hispanic workers in formal employment earning more than marginal entrants (composition) or with enforcement-driven wage premia.

5.2 Event Study and Pre-Trend Analysis

Table 4, Panel B reports the pre-trend falsification test. The event study coefficients reveal significant pre-trends: relative to $q = -1$, the Hispanic–non-Hispanic gap was 4.9 log points smaller at $q = -8$ and rose monotonically toward the activation date. After activation, the gap widened further, reaching 8.3 log points at $q = +8$.

The pre-trends undermine causal interpretation. Panel C reports the detrended specification, which adds a Hispanic $\times$ linear time trend to absorb differential pre-existing formalization. The DDD coefficient falls from 0.180 to -0.019 ($p = 0.04$), indicating that the entire positive effect is attributable to the pre-existing trend of differential Hispanic workforce formalization. The placebo coefficient (0.015, Panel B) confirms that statistically significant differential trends existed even within the pre-period alone.

This is a genuine null result for the causal effect of SC on formal employment: once we account for the trajectory that was already underway, activation adds nothing. But the null is itself informative—it rules out the hypothesis that enforcement caused a discrete jump in formal Hispanic employment at the moment of activation.

5.3 Industry Heterogeneity

If SC primarily affected industries reliant on immigrant labor, effects should concentrate in construction, accommodation, administrative services, and agriculture. Table 3 shows that the DDD coefficient on employment is 0.191 in high-immigrant industries and 0.181 in low-immigrant industries—essentially identical. The similarity across sectors suggests the pattern reflects a broad compositional shift in Hispanic formal employment, not a sector-specific enforcement response. This is more consistent with the formalization hypothesis than with targeted labor market disruption.

5.4 Robustness

Leave-one-state-out. Dropping each state in turn yields a coefficient range of [0.171, 0.190], confirming that no single state drives the baseline result (Table 4, Panel D). The estimate is not sensitive to the exclusion of large early-activated states (Texas, California, Arizona).

Table 3: Industry Heterogeneity: High- vs. Low-Immigrant Sectors

Dependent Variables:	ln_emp		ln_hir	
	High-Imm Emp	Low-Imm Emp	High-Imm Hir	Low-Imm Hir
Model:	(1)	(2)	(3)	(4)
Variables				
SC × Hispanic	0.1913***	0.1813***	0.0973***	0.1333***
	(0.0194)	(0.0098)	(0.0145)	(0.0098)
Fixed-effects				
county_ind_eth	Yes	Yes	Yes	Yes
county_qtr	Yes	Yes	Yes	Yes
Fit statistics				
Observations	259,972	259,744	259,972	259,744
R²	0.99408	0.99482	0.98796	0.98428

Clustered (state_fips) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: High-immigrant industries: Construction (23), Accommodation (72), Administrative (56), Agriculture (11). Low-immigrant industries: Finance (52), Professional Services (54), Education (61). All specifications include county-industry-ethnicity and county-quarter fixed effects. Standard errors clustered at the state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 4: Robustness Checks

Check	Coefficient	SE
Panel A: Main result		
SC × Hispanic (employment)	0.1798	(0.0192)
Panel B: Pre-trend falsification		
Placebo treatment ($t - 4$ to $t - 1$)	0.0151	(0.0056)
		$[p = 0.010]$
Panel C: Detrended specification		
SC × Hispanic (with Hispanic × linear trend)	-0.0195	(0.0093)
		$[p = 0.042]$
Panel D: Leave-one-state-out		
Minimum coefficient	0.1708	
Maximum coefficient	0.1902	

Notes: Panel A repeats the main DDD estimate from Table 2. Panel B reports a placebo test using only pre-activation data, with a fake treatment applied 4 quarters before actual SC activation. Panel C adds a Hispanic × linear time trend to the baseline specification, absorbing differential pre-existing formalization trends for Hispanic workers. Panel D shows the range of coefficients when dropping each state in turn (LOSO sensitivity). All specifications use county-ethnicity FE and state-clustered SEs.

Clustering. With 49 state clusters, asymptotic cluster-robust inference is reliable. The standard cluster-robust p-value is 2.0×10^{-12}.

QWI suppression and measurement. The QWI applies disclosure-avoidance suppression to cells with fewer than three workers, which could bias our estimates if suppression patterns change differentially for Hispanic workers after SC activation. If enforcement causes small Hispanic cells to fall below the suppression threshold, our sample would lose low-employment observations, mechanically inflating average employment. The triple-difference mitigates this by comparing within county-quarter, but we cannot rule out differential suppression effects entirely. Future work with restricted-access LEHD microdata could address this limitation directly.

Omitted policy confounders. Our specification omits controls for concurrent immigration policies—287(g) agreements and state E-Verify mandates—that also affected employer behavior during this period (Orrenius and Zavodny, 2015; Bohn et al., 2014). These policies would be absorbed by county-quarter fixed effects in our saturated specification only if they affected Hispanic and non-Hispanic workers identically within a county-quarter. To the extent that 287(g) or E-Verify disproportionately affected Hispanic formalization, they could contribute

to the pre-trends we observe.

6. Discussion

Administrative vs. survey divergence. The central finding of this paper is a measurement puzzle, though one whose interpretation we must now revise in light of the detrended analysis. Using household survey data, East et al. (2023) estimate that SC reduced employment among likely undocumented immigrants by 4.6% and among all Hispanic noncitizens by 1.6%. In our baseline DDD, administrative employer-reported data shows an 18% *increase* in reported Hispanic employment relative to non-Hispanic—but this effect vanishes entirely when we control for differential pre-trends ($\hat{\beta} = -0.019$ in the detrended specification). The positive association is attributable to pre-existing Hispanic workforce formalization, not to SC activation itself.

What does this mean? The CPS and ACS capture total labor supply, including informal arrangements, self-employment, and work paid in cash. The QWI captures only formal, UI-covered payroll employment. The fact that formal Hispanic employment was rising rapidly during 2005–2015 in precisely the counties where SC was deployed early—but that this trend predates activation—suggests that formalization was driven by broader forces (state E-Verify mandates, post-9/11 employer audits, labor market tightening) rather than by SC specifically. The survey-based evidence of employment declines from SC (East et al., 2023) may therefore capture real labor supply reductions that occur against a backdrop of ongoing formalization visible only in administrative data.

Pre-trends as formalization trends. The detrended analysis confirms that what appeared to be an enforcement effect is actually a pre-existing formalization trend. Hispanic workforce formalization accelerated throughout the 2000s, driven by state-level E-Verify mandates, employer audits, and the broader post-9/11 enforcement environment (Bohn et al., 2014; Orrenius and Zavodny, 2015). Counties that ICE prioritized for early SC activation were precisely those where formalization was already advancing fastest—not because SC caused the trend, but because ICE targeted jurisdictions with large unauthorized populations, which were also the jurisdictions where the informal-to-formal transition was most active. The fact that adding a single Hispanic × linear trend eliminates the entire effect (from 0.180 to −0.019) demonstrates the importance of trend controls when exploiting staggered policy adoption in settings where treatment timing is endogenous to the outcome's trajectory.

Limitations. The detrended specification shows that no causal effect of SC on formal employment survives trend adjustment. This limits what we can claim about enforcement's

direct role in formalization. Several avenues could strengthen identification in future work: individual-level LEHD microdata (restricted access) could track worker transitions between formal and informal employment directly; Callaway and Sant'Anna (2021) estimators could address heterogeneous treatment effects in the staggered rollout; and HonestDiD sensitivity bounds (Rambachan and Roth, 2023) could quantify how large trend violations would need to be to overturn any residual effect. Our omission of controls for concurrent 287(g) agreements and state E-Verify mandates also limits the ability to disentangle SC from the broader enforcement environment. Our contribution is documenting that administrative and survey data diverge sharply during this period, a fact that any credible analysis of immigration enforcement must confront.

7. Conclusion

Administrative employer data and household surveys can tell dramatically different stories about the same enforcement shock—not because one is wrong, but because they measure different objects. Our baseline estimates suggest a large positive effect of Secure Communities on formal Hispanic employment, but this effect is entirely attributable to pre-existing formalization trends: controlling for differential pre-trends reduces the coefficient to near zero. The honest conclusion is that SC did not cause a discrete shift in formal employment. But the measurement divergence we document—formal Hispanic employment rising rapidly in administrative data while survey-based studies find enforcement-driven declines—is real and important. It implies that researchers studying immigration enforcement must specify which margin of employment they are measuring. The ongoing formalization of the Hispanic workforce, driven by forces broader than any single enforcement program, means that administrative and survey data will continue to diverge, and treating either source in isolation will yield an incomplete picture of enforcement's true effects.

Acknowledgements

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A. Data Appendix

QWI data access. The Quarterly Workforce Indicators are produced by the Census Bureau's LEHD program and are freely available through the QWI Explorer and bulk download. We accessed pre-processed county-level QWI Parquet files from the APEP Azure data warehouse, which mirrors the Census Bureau's public QWI release. The race/ethnicity tabulation (code: rh) cross-classifies all 81 QWI variables by race (7 categories) and ethnicity (Hispanic/non-Hispanic). We selected ethnicity codes A1 (non-Hispanic, all races) and A2 (Hispanic, all races) with race code A0 (all races), NAICS sectors excluding "00" (all industries), for the years 2005–2015.

Secure Communities activation dates. The activation dates were parsed from the ICE "Activated Jurisdictions" PDF document obtained through FOIA. The document lists 3,181 jurisdictions with their activation dates, organized by state. We matched county names to FIPS codes using the tidycensus crosswalk, achieving a 94.3% match rate (3,008 of 3,186 parsed records). Unmatched records include territories, independent cities with non-standard names, and jurisdictions with naming discrepancies.

Sample filters. Starting from 5,027,288 QWI rows, we aggregate across industries to the county-quarter-ethnicity level (280,556 observations), merge with activation dates (260,008 observations after dropping unmatched counties), and construct treatment indicators based on each county's activation date.

B. Standardized Effect Sizes

Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Employment	0.1798	(0.0192)	2.356	0.0763	(0.0081)	Moderate positive
Hiring	0.0927	(0.0194)	2.371	0.0391	(0.0082)	Small positive
Separations	0.0866	(0.0183)	2.372	0.0365	(0.0077)	Small positive
Earnings	0.2790	(0.0934)	0.671	0.4161	(0.1393)	Large positive
Job Creation	0.1376	(0.0157)	2.036	0.0676	(0.0077)	Moderate positive
Job Destruction	0.1454	(0.0136)	2.032	0.0715	(0.0067)	Moderate positive

Notes: Standardized Diagnostic Effect sizes computed as SDE = $\hat{\beta}$/SD(Y) where $\hat{\beta}$ is the triple-difference coefficient (Post-SC × Hispanic) and SD(Y) is the pre-activation standard deviation of the log outcome among Hispanic county-quarter observations. Binary treatment (activated vs not yet). Classification refers to magnitude, not statistical significance. Research question: Does immigration enforcement through Secure Communities reduce Hispanic employment through employer-side adjustments? Data: QWI county × quarter × ethnicity, 2005–2015. Method: Triple-difference with county-ethnicity and county-quarter FE. $N = 260,008$ county-quarter-ethnicity observations across 2,961 counties.