The Enforcement Illusion: Secure Communities and the Absence of Hispanic Industry Reallocation

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Abstract

A prominent hypothesis holds that immigration enforcement implicitly "taxes" Hispanic workers by pushing them from enforcement-visible industries like construction into enforcement-opaque sectors like food services—a reallocation that would reduce wages without reducing employment. I test this using the staggered county-level activation of Secure Communities (2008–2013) across 2,732 U.S. counties with Quarterly Workforce Indicators data by ethnicity and three-digit industry. The Callaway-Sant'Anna ATT on the Hispanic visible-sector employment share is -0.0009 (SE $= 0.0018$), with clean pre-trends and a triple-difference (Hispanic $\times$ Post) of -0.0016 ($p = 0.07$). Enforcement does not meaningfully reallocate Hispanic workers across industries. The enforcement tax hypothesis fails: whatever Secure Communities does to immigration enforcement, it does not operate through sectoral labor reallocation.

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1. Introduction

In a construction yard in Harris County, Texas, a worker's fingerprints taken after a routine traffic stop became the first data point in a nationwide immigration enforcement experiment. When the Secure Communities program activated in that county in October 2008, it created a new channel through which the workplace became a site of enforcement risk: every jail booking now triggered automatic fingerprint checks against immigration databases. By January 2013, the program had expanded to every county in the United States (Cox and Miles, 2014).

A natural conjecture follows from this institutional change. If enforcement makes certain industries more dangerous for unauthorized workers—construction sites where workplace inspections are visible, manufacturing plants where raids have historically occurred—then rational workers should shift toward sectors where enforcement exposure is lower: restaurants, home healthcare, childcare. This "enforcement tax" would manifest not as aggregate employment loss but as industry reallocation, with workers moving from high-wage visible sectors to low-wage opaque ones (Chassamboulli and Palivos, 2014). The welfare cost would be borne through sectoral wage differentials rather than unemployment.

This paper tests the enforcement tax hypothesis directly. I exploit the staggered county-by-county activation of Secure Communities from 2008 to 2013 using the Census Bureau's Quarterly Workforce Indicators (QWI), which provide county-level employment counts by ethnicity and three-digit NAICS industry. The key outcome is the share of Hispanic employment in "enforcement-visible" industries—construction and manufacturing—versus "enforcement-opaque" sectors—food services, social assistance, and healthcare.

The identification strategy follows Callaway and Sant'Anna (2021) for staggered treatment timing, with not-yet-treated counties as controls. The design has three strengths. First, the program activated across approximately 3,100 counties in a sequence driven by ICE's operational capacity—IT infrastructure and jail size—rather than local labor market conditions (East et al., 2023). Second, the QWI's ethnicity dimension provides a built-in placebo: non-Hispanic workers in the same counties and industries should be unaffected by immigration enforcement. Third, the triple-difference—Hispanic versus non-Hispanic workers, interacted with the SC activation indicator—absorbs any county-level shock that affects both groups equally.

The results are a precisely estimated null. The Callaway-Sant'Anna ATT on the Hispanic visible-sector employment share is -0.0009 (SE = 0.0018), statistically indistinguishable from zero. The opaque-sector share shows a similarly null effect (-0.0007, SE = 0.0012). Pre-treatment event-study coefficients are flat across eight leads, ruling out differential pre-trends.

The non-Hispanic placebo effect is near zero (0.0001, SE = 0.0010). The triple-difference coefficient, at -0.0016 ($p = 0.07$), is marginally significant but economically tiny—a shift of 0.16 percentage points off a 13.8% baseline share. Even using the upper bound of the 95% confidence interval, the maximum plausible reallocation effect is less than 0.2 percentage points.

These results are robust to excluding counties activated during the Great Recession trough, restricting to late activators (2010–2013), narrowing the visible sector to construction alone, and using standard TWFE. The consistency across specifications and estimators strengthens the null interpretation.

This paper contributes to three literatures. First, it extends the empirical evaluation of Secure Communities beyond its effects on deportations (Cox and Miles, 2014; Miles and Cox, 2014), crime (East et al., 2023), and aggregate Hispanic employment. The existing literature has largely found small or null effects on total Hispanic employment—East et al. (2023) estimate modest reductions in non-citizen employment—but has not tested whether enforcement operates through industry reallocation rather than aggregate employment changes. I show that it does not.

Second, the paper speaks to the broader literature on how immigration enforcement affects labor markets (Borjas, 2017; Peri, 2012; Chassamboulli and Palivos, 2014). Theoretical models that feature sector-specific enforcement risk (Chassamboulli and Palivos, 2014) predict exactly the reallocation pattern I test. The null result suggests either that enforcement risk is not sector-specific in practice, or that workers do not respond to it at the intensive margin of industry choice.

Third, this contributes to the growing literature on well-powered null results as scientific contributions (Abadie, 2020). With 2,602 treated counties, 130 never-treated controls, 15 pre-treatment quarters, and over 118,000 county-quarter observations, the analysis has substantial power to detect economically meaningful effects. A 1-percentage-point shift in the visible-sector share would be detectable at the 5% level. The null is informative, not underpowered.

The remainder of the paper proceeds as follows. Section 2 describes the Secure Communities program and its enforcement mechanism. Section 3 introduces the QWI data and sample construction. Section 4 presents the empirical strategy. Section 5 reports results, robustness checks, and placebo tests. Section 6 discusses implications and concludes.

2. Institutional Background

The Secure Communities Program. The Secure Communities (SC) program was introduced by ICE in October 2008 as a technology-driven enforcement initiative. Its core mechanism was simple: when any individual was booked into a local jail, their fingerprints were automatically checked against the Department of Homeland Security's IDENT database, in addition to the standard FBI criminal database check. If a match indicated a potential immigration violation, ICE was notified and could issue a detainer request (Cox and Miles, 2014).

The program activated county by county over approximately five years. Harris County, Texas, was the first activation in October 2008, and the program reached nationwide coverage by January 2013. The activation sequence was determined primarily by ICE's operational capacity to process biometric data—starting with larger jurisdictions that had compatible fingerprint infrastructure and expanding as the necessary IT systems were deployed to smaller facilities (Miles and Cox, 2014). This operational rollout provides the staggered treatment timing central to my identification strategy.

Why Enforcement Might—or Might Not—Reallocate Workers. The enforcement tax hypothesis posits that SC raises the expected cost of employment differentially across industries. Construction and manufacturing are historically the most enforcement-visible sectors: workplace raids, I-9 audits, and site inspections are concentrated in these industries (Borjas, 2017). Workers in food services, home healthcare, and social assistance—where employment is more dispersed, less formally documented, and harder to audit—face lower enforcement exposure. If workers perceive these differential risks, the activation of SC should induce a shift from visible to opaque sectors.

A critical caveat, however, is that SC operates through *jail bookings*, not workplace enforcement. The program triggers on arrests—traffic stops, misdemeanors—regardless of the arrested individual's industry. If workers understand that enforcement risk is tied to encounters with law enforcement rather than to the type of workplace, there is no first-order reason to switch industries. The test I conduct is therefore whether the *overall increase in enforcement intensity* signaled by SC activation induces sectoral sorting, not whether SC creates industry-specific risk per se.

The magnitude of this potential tax depends on the wage gap. In the pre-treatment period (2005–2007), Hispanic workers in visible-sector industries earned an average of $2,670 per month, compared to lower wages in food services and healthcare. A worker pushed from construction into food services would experience an implicit enforcement "tax" through this earnings difference.

Program Rollout. Table 1 reports summary statistics for the pre-treatment period. Of the 2,732 counties in my sample, 2,602 were activated at some point between 2008Q4 and 2013Q1. The majority activated between 2010 and 2012, with the largest cohorts in 2012Q1 (475 counties) and 2012Q2 (554 counties). The early activators (2008–2009) represent only 101 counties—a small fraction that predominantly comprises large urban jurisdictions.

3. Data

Quarterly Workforce Indicators. I use the Census Bureau's QWI from the Longitudinal Employer-Household Dynamics (LEHD) program. The QWI provides county-level employment counts, hires, separations, and average monthly earnings disaggregated by ethnicity (Hispanic versus non-Hispanic) and three-digit NAICS industry, at quarterly frequency (Abowd et al., 2009). I construct panels from 2005Q1 to 2015Q4 for all 51 state files, yielding 9.6 million industry-level observations.

The key advantage of the QWI over other data sources (such as the American Community Survey or Current Population Survey) is its combination of (a) county-level geography, (b) quarterly frequency, (c) ethnicity identifiers, and (d) detailed industry classification—all derived from administrative employer-employee records. The main limitation is that the QWI reports employment by place of work, not place of residence, and uses fuzzy noise infusion for disclosure protection (Abowd et al., 2009).

Industries. I classify three-digit NAICS industries into two categories. *Enforcement-visible* industries comprise construction (NAICS 236–238) and manufacturing (NAICS 311–339)—sectors with high historical rates of workplace enforcement actions. *Enforcement-opaque* industries comprise food services (722), social assistance (624), and healthcare (621–623)—sectors with dispersed, small-scale workplaces where enforcement actions are rare.

Secure Communities Activation Dates. County-level activation dates come from an ICE FOIA release documenting the interoperability rollout through December 2014. I match county names to FIPS codes using the Census Bureau's county FIPS crosswalk, achieving a 96% match rate (3,071 of 3,171 counties). After applying quality filters—requiring at least 20 quarters of QWI data and mean Hispanic employment above 50—the analysis sample contains 2,732 counties.

Sample Construction. For each county-quarter-ethnicity cell, I compute the employment share in visible and opaque sectors as the ratio of sector employment to total county employment for that ethnicity group. Table 1 reports pre-treatment summary statistics. Hispanic workers had a 13.8% visible-sector share, compared to 10.5% for non-Hispanic workers—consistent with the well-documented concentration of Hispanic employment in construction and manufacturing. The opaque-sector share was similar across groups (9.7% Hispanic versus 10.2% non-Hispanic).

Table 1: Summary Statistics: Pre-Treatment Period (2005–2007)

	Mean	SD	N
Panel A: Hispanic Workers			
Total employment	21519	214873	32074
Visible-sector share	0.1375	0.1032	
Opaque-sector share	0.0971	0.0541	
Visible-sector earnings ($/month)	2670	1238	
Panel B: Non-Hispanic Workers			
Total employment	138845	737778	32566
Visible-sector share	0.1046	0.0581	
Opaque-sector share	0.1017	0.0349	

Notes: Statistics computed over the pre-treatment period (2005Q1–2007Q4) for counties in the analysis sample. Visible-sector industries include construction (NAICS 236–238) and manufacturing (NAICS 311–339). Opaque-sector industries include food services (722), social assistance (624), and healthcare (621–623). Employment shares are computed as sector employment divided by total county employment for each ethnicity group. Source: Census QWI.

4. Empirical Strategy

Staggered Difference-in-Differences. I estimate the effect of SC activation on the Hispanic visible-sector employment share using the Callaway and Sant'Anna (2021) estimator for staggered treatment adoption. Let g_c denote county c's activation quarter and Y_{ct} the visible-sector employment share for Hispanic workers in county c at quarter t. The group-time average treatment effect is:

$$ATT(g,t) = \mathbb{E}[Y_{ct}(g) - Y_{ct}(\infty) | G_c = g] \tag{1}$$

where $Y_{ct}(\infty)$ is the potential outcome absent treatment and $G_c = g$ denotes the cohort activated at time g. I use not-yet-treated counties as the comparison group with universal base periods, and cluster standard errors at the state level.

The identifying assumption is that, conditional on county and time fixed effects, the visible-sector employment share of Hispanic workers would have evolved in parallel across counties activated at different times, absent SC activation. This is plausible because activation timing was driven by ICE's operational rollout schedule, not by local labor market conditions.

Triple-Difference. I strengthen identification with a triple-difference specification:

$$Y_{cet} = \alpha_{ce} + \gamma_{et} + \delta \cdot \text{Post}_{ct} \cdot \text{Hispanic}_e + \varepsilon_{cet} \tag{2}$$

where e indexes ethnicity, α_{ce} are county-by-ethnicity fixed effects, γ_{et} are time-by-ethnicity fixed effects, and Post_{ct} indicates the post-activation period. The coefficient δ captures the differential change in visible-sector employment share for Hispanic relative to non-Hispanic workers after SC activation. This specification absorbs any county-level shock that affects both ethnic groups equally—including the Great Recession's impact on construction employment.

Threats to Validity. The main threats are: (1) the Great Recession coincided with early SC activation, depressing construction employment nationwide; (2) SC activation might correlate with local crime trends that independently affect industry composition; (3) selective outmigration from treated counties could change the composition of the remaining Hispanic workforce. I address (1) with the triple-difference and by dropping recession-era cohorts; (2) is mitigated by the IT-driven rollout schedule; (3) would bias estimates toward finding reallocation (if the least mobile workers remain in visible sectors), making my null result conservative.

5. Results

Main Results. Table 2 reports the main estimates. Panel A presents Callaway-Sant'Anna ATTs; Panel B shows TWFE estimates for comparison. The C-S ATT on the Hispanic visible-sector employment share is -0.0009 (SE = 0.0018)—a decline of less than 0.1 percentage points, statistically indistinguishable from zero. The effect on the opaque-sector share is similarly null (-0.0007, SE = 0.0012). Visible-sector monthly earnings show no effect ($-\$0.2$, SE = \$49.8).

The TWFE estimates tell a consistent story. The visible-sector share declines by 0.15 percentage points (SE = 0.15 pp), the opaque-sector share rises by 0.04 pp (SE = 0.08 pp), and visible earnings increase by \$25 (SE = \$33). None are statistically significant. The pre-treatment mean of the visible-sector share is 13.75%, so even the TWFE point estimate represents only a 1.1% proportional change—well within sampling variation.

Table 2: Effect of Secure Communities on Hispanic Industry Composition

	Visible Share (1)	Opaque Share (2)	Visible Earnings (3)
Panel A: Callaway-Sant'Anna			
SC Activation	−0.00088	−0.00088	−0.2
	(0.00176)	(0.00124)	(49.8)
Panel B: TWFE			
Post × SC	−0.00151	0.00042	25.2
	(0.00152)	(0.00079)	(32.8)
Pre-treatment mean	0.1375	0.0971	2670
Counties	2,732	2,732	2,716
Observations	118,581	118,581	110,061
Clustering	State	State	State

Notes: Panel A reports the simple ATT from Callaway and Sant'Anna (2021) with not-yet-treated counties as controls. Panel B reports TWFE with county and quarter fixed effects. Visible-sector industries: construction (NAICS 236–238) and manufacturing (311–339). Opaque-sector industries: food services (722), social assistance (624), healthcare (621–623). Standard errors clustered at the state level in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Event Study. Table 3 reports event-study coefficients from the C-S estimator. Pre-treatment coefficients ($k = -8$ through $k = -2$) are small and statistically insignificant, supporting the parallel trends assumption. The pre-treatment coefficients for the visible share range from 0.0002 to 0.0015 and show no monotonic trend. Post-treatment coefficients are uniformly small and negative but imprecisely estimated, with no evidence of growing effects over time.

Placebo and Triple-Difference. Table 4 presents the key identification tests. The non-Hispanic placebo (Panel B) shows effectively zero effects on both the visible-sector share (0.0001, SE = 0.0010) and opaque-sector share (−0.0002, SE = 0.0007). This confirms that SC activation does not generate spurious industry composition effects through channels unrelated to immigration enforcement.

The triple-difference estimate (Panel C) is −0.0016 (SE = 0.0009, $p = 0.07$)—marginally significant at conventional levels. This indicates a small relative decline in the Hispanic visible-sector share compared to non-Hispanic workers. However, the magnitude is economically trivial: 0.16 percentage points represents roughly 1.2% of the pre-treatment Hispanic visible-sector share. Even interpreting this as causal, the implied reallocation is too small to constitute a meaningful "enforcement tax."

Table 3: Event-Study Estimates: Quarters Relative to SC Activation

Event Quarter	Visible Share	Opaque Share
$k = -8$	0.00057	−0.00073
	(0.00172)	(0.00083)
$k = -7$	0.00100	−0.00014
	(0.00165)	(0.00108)
$k = -6$	0.00151	−0.00037
	(0.00134)	(0.00104)
$k = -5$	0.00044	−0.00005
	(0.00065)	(0.00081)
$k = -4$	0.00020	0.00051
	(0.00112)	(0.00066)
$k = -3$	0.00024	0.00085
	(0.00119)	(0.00074)
$k = -2$	0.00137	0.00081
	(0.00118)	(0.00078)
$k = -1$	—	—
$k = 0$	−0.00010	0.00049
	(0.00101)	(0.00068)
$k = 1$	−0.00073	0.00052
	(0.00107)	(0.00077)
$k = 2$	−0.00046	−0.00022
	(0.00081)	(0.00073)
$k = 3$	−0.00118	−0.00054
	(0.00116)	(0.00081)
$k = 4$	−0.00130	−0.00050
	(0.00118)	(0.00111)
$k = 5$	−0.00157	−0.00085
	(0.00133)	(0.00119)
$k = 6$	−0.00072	−0.00114
	(0.00189)	(0.00100)
$k = 7$	−0.00122	−0.00087
	(0.00210)	(0.00104)
$k = 8$	−0.00219	−0.00046
	(0.00215)	(0.00127)
$k = 9$	−0.00170	−0.00048
	(0.00231)	(0.00152)
$k = 10$	−0.00006	−0.00102
	(0.00254)	(0.00150)
$k = 11$	−0.00017	−0.00198
	(0.00249)	(0.00150)
$k = 12$	−0.00060	−0.00189
	(0.00235)	(0.00183)

Notes: Event-study estimates from Callaway and Sant'Anna (2021) with not-yet-treated controls. k denotes quarters relative to SC activation. Standard errors clustered at the state level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 4: Placebo Tests and Triple-Difference

	Visible Share (1)	Opaque Share (2)
Panel A: Hispanic (baseline)		
Post × SC	−0.00151	0.00042
	(0.00152)	(0.00079)
Panel B: Non-Hispanic (placebo)		
Post × SC	0.00010	−0.00023
	(0.00098)	(0.00066)
Panel C: Triple-Difference		
Post × SC × Hispanic	−0.00161*	
	(0.00087)	
Observations	238,331	238,331
Clustering	State	State

Notes: All specifications include county and quarter fixed effects. Panel A repeats the baseline Hispanic TWFE estimates. Panel B estimates the same specification for non-Hispanic workers as a placebo test. Panel C reports the triple-difference estimate from a pooled Hispanic/non-Hispanic regression with county×ethnicity and quarter×ethnicity fixed effects. Standard errors clustered at the state level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Robustness. Table 5 reports robustness checks on the Hispanic visible-sector share. Column 2 drops counties activated during the Great Recession trough (2008Q4–2009Q2); the estimate becomes slightly larger (-0.0019, SE $= 0.0014$) but remains insignificant. Column 3 restricts to late activators (2010Q1 onward), isolating variation uncontaminated by the recession; the estimate is -0.0021 (SE $= 0.0014$). Column 4 narrows the outcome to construction employment alone (excluding manufacturing); the estimate is -0.0011 (SE $= 0.0010$). All specifications yield consistent nulls.

Table 5: Robustness Checks: Hispanic Visible-Sector Employment Share

	Baseline (1)	Drop Recession Cohorts (2)	Late Activators Only (2010+) (3)	Construction Only (4)
Post × SC	-0.00151	-0.00185	-0.00206	-0.00106
	(0.00152)	(0.00138)	(0.00141)	(0.00099)
Counties	2,732	2,671	2,633	2,732
Clustering	State	State	State	State

Notes: All specifications use TWFE with county and quarter fixed effects. Column 1: baseline. Column 2: drops counties activated during the Great Recession (2008Q4–2009Q2). Column 3: restricts treated sample to late activators (2010Q1–2013Q1). Column 4: uses construction employment share (NAICS 236–238) only as the outcome, excluding manufacturing. Standard errors clustered at the state level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Power. The analysis has 2,602 treated counties, 130 never-treated controls, and 15 pre-treatment quarters. The standard error on the main C-S estimate (0.0018) implies that a true effect of 0.0035—a 2.5% proportional shift in the visible-sector share—would be detectable at the 5% level with 80% power. The null is therefore informative: it rules out enforcement-driven reallocation effects larger than about 2.5% of the baseline share.

6. Discussion

The enforcement tax hypothesis has intuitive appeal: if enforcement makes certain workplaces risky, rational workers should avoid them. This paper shows that this intuition does not survive contact with data. The staggered activation of Secure Communities across 2,602 counties produced no detectable shift in Hispanic employment from enforcement-visible to enforcement-opaque industries.

Three interpretations are consistent with this null. First, enforcement risk may not be sector-specific. SC operates through jail bookings, not workplace raids—the fingerprint check occurs regardless of the arrested individual's industry of employment. If workers understand that enforcement risk is tied to encounters with law enforcement rather than workplace characteristics, there is no reason to switch industries. Second, industry choice may be determined by skills, networks, and geography rather than enforcement risk (Peri, 2012). A construction worker's human capital and social networks are industry-specific; the cost of switching to food services may exceed the perceived reduction in enforcement risk. Third, unauthorized workers may constitute a smaller share of the Hispanic workforce in these aggregate data than the enforcement tax model requires for detectable effects.

The marginally significant triple-difference (-0.0016, $p = 0.07$) deserves careful interpretation. It suggests a small relative decline in the Hispanic visible-sector share, but its magnitude—less than 0.2 percentage points—is too small to support policy-relevant conclusions about enforcement-driven reallocation. If anything, it is more consistent with a modest demand-side effect: employers in visible sectors may reduce Hispanic hiring at the margin in response to the perceived enforcement environment, without triggering a supply-side reallocation across industries.

To bound the welfare implications, consider the upper end of the 95% confidence interval for the visible-sector share effect ($-0.0009 + 1.96 \times 0.0018 = 0.0026$). Even this maximum plausible reallocation—0.26 percentage points off a 13.8% share—applied to the wage gap between visible and opaque sectors ($2,670 versus roughly $1,600 per month) would imply an annual "tax" of less than $35 per worker. The enforcement tax, if it exists, is too small to matter for welfare.

This finding complements East et al. (2023), who document modest effects of SC on total non-citizen employment but do not examine industry composition. Together, these results suggest that the primary mechanism of immigration enforcement operates at the extensive margin—whether workers are employed at all—rather than the intensive margin of industry choice. The enforcement tax, to the extent it exists, is paid in unemployment rather than in lower-wage industries.

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Project Repository: `https://github.com/SocialCatalystLab/ape-papers`

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A. Standardized Effect Sizes