The Chilling Effect: How E-Verify Mandates Reduce Hispanic Employment Across State Lines

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Abstract

When a state mandates E-Verify, does displaced Hispanic labor flow into adjacent untreated states? Using Quarterly Workforce Indicators for 1,979 counties over 2004–2024, I estimate a triple-difference comparing Hispanic versus Non-Hispanic employment in border versus interior counties of non-mandating states, before versus after adjacent E-Verify adoption. Rather than displacement, I find a *chilling effect*: Hispanic employment in border counties falls by 16.2 log points ($p < 0.01$) relative to the counterfactual. The effect concentrates in accommodation and food services, is absent in professional services, and survives placebo and distance-decay tests. New Hispanic hires decline sharply (-17.0 log points), suggesting the mechanism operates through reduced hiring rather than layoffs. State-level enforcement creates geographic spillovers that extend well beyond the mandating state's borders.

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1. Introduction

In December 2007, Arizona became the first U.S. state to mandate E-Verify—the federal electronic employment verification system—for all employers. Within five years, nine more states followed. The standard question in this literature is: what happens to Hispanic employment *within* the mandating state? The answer is well established: formal employment of likely unauthorized workers declines (Amuedo-Dorantes and Bansak, 2020; Bohn et al., 2014; Good, 2016). But this finding raises an immediate follow-up: *where do those workers go?*

The prevailing assumption in the policy debate is geographic displacement. If Arizona mandates E-Verify, workers move to Nevada or New Mexico. State legislators frequently cite this concern to justify inaction, arguing that unilateral enforcement merely relocates the problem (Rosenblum, 2011). Yet no study has tested this claim with administrative data at the county level, where the geographic boundary between enforcement and non-enforcement is sharpest.

This paper exploits the staggered adoption of E-Verify mandates across ten U.S. states (2008–2023) to test for geographic spillovers into neighboring untreated states. Using the Census Bureau's Quarterly Workforce Indicators (QWI)—universe-coverage administrative records from state unemployment insurance programs—I construct a panel of 1,979 counties in 38 non-mandating states over 86 quarters. The identification strategy is a triple-difference design that compares: (i) Hispanic versus Non-Hispanic workers (within the same county), (ii) in border counties adjacent to E-Verify states versus interior counties (geographic variation), (iii) before versus after the adjacent state adopts its mandate (temporal variation). County-ethnicity fixed effects absorb all time-invariant county$\times$group heterogeneity, and quarter$\times$ethnicity fixed effects absorb national-level trends in Hispanic employment.

The central finding overturns the displacement hypothesis. Rather than an influx of Hispanic workers into border counties of untreated states, I find the opposite: Hispanic employment *falls* by 16.2 log points ($p < 0.01$) in these counties after the adjacent state mandates E-Verify. I call this the *chilling effect*: the proximity of enforcement deters Hispanic hiring even where no mandate exists. The effect is not a pre-existing trend—a placebo test on 2004–2007 data yields a coefficient of -0.003 ($p = 0.85$)—and is not driven by COVID-era disruptions, as restricting the sample to pre-2020 yields a nearly identical estimate of -0.149.

The chilling effect operates primarily through reduced hiring. New Hispanic hires in border counties decline by 17.0 log points ($p < 0.001$), while there is no significant effect on earnings. This pattern is consistent with employers in border counties preemptively avoiding Hispanic workers due to anticipated enforcement risk, rather than firing incumbent employees.

Industry heterogeneity confirms the mechanism: the effect concentrates in accommodation and food services (NAICS 72, $\hat{\beta} = -0.143$, $p < 0.01$), where Hispanic employment shares are high and small employers dominate. Professional services (NAICS 54), where Hispanic employment shares are low, shows a precisely estimated null ($\hat{\beta} = 0.008$, $p = 0.93$).

Two additional results sharpen the interpretation. First, the chilling effect does not decay with geographic distance: counties two rings removed from the enforcement boundary show effects of similar magnitude (-0.172, $p = 0.02$). This suggests the chill reflects a regional perception of enforcement risk rather than a purely local phenomenon. Second, adding state×quarter fixed effects does not attenuate the DDD coefficient, ruling out state-level economic shocks as a confound.

This paper contributes to three literatures. First, it adds to the study of E-Verify's labor market effects (Amuedo-Dorantes and Bansak, 2020; Bohn et al., 2014; Good, 2016; Orrenius and Zavodny, 2015) by documenting cross-border spillovers that have been theorized but never measured. Second, it speaks to the broader literature on policy spillovers in federal systems (Baicker, 2005; Dube et al., 2010), showing that enforcement policies can extend well beyond their jurisdictional boundaries through the expectations channel. Third, it contributes to the measurement of discrimination and statistical profiling in labor markets (Agan and Starr, 2018; Doleac and Hansen, 2020), as the chilling effect implies that employers use geographic proximity to enforcement as a signal when screening Hispanic job applicants.

2. Institutional Background

E-Verify mandates. E-Verify is a web-based system operated by U.S. Citizenship and Immigration Services that allows employers to verify the employment eligibility of newly hired employees by comparing information from an employee's Form I-9 against Department of Homeland Security and Social Security Administration records. While federal contractors have been required to use E-Verify since 2009, state-level mandates extend the requirement to private employers.

Ten states adopted E-Verify mandates between 2008 and 2023: Arizona (2008), Utah (2010), Mississippi (2011), Louisiana (2011), Alabama (2012), Georgia (2012), North Carolina (2013), Tennessee (2017), South Carolina (2021), and Florida (2023). The mandates vary in scope—some apply to all employers, others to employers above a size threshold or to public contractors only—but all impose penalties for non-compliance, creating strong incentives for employers to verify eligibility.

The displacement hypothesis. The standard prediction is that E-Verify mandates reduce demand for unauthorized labor in mandating states, causing geographic displacement to non-mandating states. This prediction has two variants: the *strong* version, in which workers physically relocate across state lines, and the *weak* version, in which new unauthorized entrants divert to non-mandating states. Either mechanism would generate increased Hispanic employment in border counties of untreated states—precisely the pattern this paper tests for and fails to find.

The chilling effect mechanism. An alternative prediction is that E-Verify mandates create enforcement externalities. Employers in nearby states may observe the mandate, fear that enforcement is spreading, and preemptively reduce Hispanic hiring. This mechanism operates through employer expectations rather than worker migration, and predicts *reduced* Hispanic employment in border counties—the pattern the data reveal. The chilling effect is analogous to the well-documented deterrent effects of immigration enforcement on service utilization (Watson, 2014; Alsan and Yang, 2022): even people who are legally present reduce their engagement with formal institutions when enforcement intensifies nearby.

3. Data

The primary data source is the Quarterly Workforce Indicators (QWI), produced by the Census Bureau's Longitudinal Employer-Household Dynamics (LEHD) program. The QWI are derived from administrative unemployment insurance records covering virtually all private-sector employers. I use the race×ethnicity (rh) tabulation at the county×NAICS-sector level, which disaggregates employment by Hispanic/Non-Hispanic origin. The key outcomes are beginning-of-quarter employment (Emp), average monthly earnings (EarnS), and all hires (HirA). The panel spans 2004Q1–2024Q4.

I restrict the sample to counties in the 38 states (plus D.C.) that never mandated E-Verify during the sample period. Using the Census Bureau's 2024 county adjacency file, I classify these counties as *border* (sharing a boundary with a county in an E-Verify state) or *interior* (no such adjacency). The final panel contains 1,979 counties (73 border, 1,906 interior), observed quarterly over two ethnicity groups and 86 quarters, yielding 120,996 county-quarter-ethnicity observations.

Table 1: Summary Statistics: Pre-Period County-Quarter Means (2004–2007)

Group	Mean Emp.	SD Emp.	Mean Earn.	Mean Hires	Counties	Obs.
Border, Hispanic	4,065	24,541	2,070	1,250	73	899
Border, Non-Hispanic	18,523	80,774	2,535	4,979	73	899
Interior, Hispanic	5,928	45,614	2,150	1,704	1891	24,758
Interior, Non-Hispanic	64,834	326,223	2,663	13,123	1891	24,758

Notes: Pre-period (2004–2007) means. Employment is beginning-of-quarter count. Earnings are average monthly earnings ($). Border counties are in non-E-Verify states adjacent to an E-Verify state. Interior counties are in non-E-Verify states with no border adjacency.

Table 1 reports pre-period (2004–2007) summary statistics. Hispanic employment levels are smaller on average than Non-Hispanic levels across both border and interior counties, reflecting population composition. Border counties are somewhat smaller than interior counties on average.

4. Empirical Strategy

4.1 Identification

The identification strategy is a triple-difference (DDD) that exploits three sources of variation:

1. **Ethnicity (within-county):** Hispanic versus Non-Hispanic employment in the same county. The within-county comparison absorbs all county-level shocks (local business cycles, weather, industry composition changes) that affect both groups equally.

2. **Geography:** Border counties of untreated states (adjacent to an E-Verify state) versus interior counties (no such adjacency). If E-Verify creates geographic spillovers, they should concentrate in border counties.

3. **Time:** Before versus after the adjacent E-Verify state adopts its mandate. The staggered timing of adoption across ten states provides eight distinct treatment events.

4.2 Estimation

I estimate:

$$\ln Y_{cte} = \beta_1(\text{Post}_{ct} \times \text{Hispanic}_e) + \beta_2 \text{Post}_{ct} + \alpha_{ce} + \gamma_{te} + \varepsilon_{cte} \tag{1}$$

where c indexes counties, t indexes quarters, and $e \in \{\text{Hispanic, Non-Hispanic}\}$. Post_{ct} equals one for border counties after the adjacent state adopts E-Verify and zero for all interior counties. α_{ce} are county×ethnicity fixed effects, and γ_{te} are quarter×ethnicity fixed effects. The coefficient β_1 is the DDD estimate: the differential change in Hispanic (versus Non-Hispanic) employment in border (versus interior) counties after adjacent E-Verify adoption, net of national Hispanic employment trends.

Standard errors are clustered at the state level, the level at which treatment is assigned (Bertrand et al., 2004). The key identifying assumption is that, absent the adjacent state's E-Verify mandate, the Hispanic–Non-Hispanic employment gap in border counties would have evolved in parallel with the corresponding gap in interior counties. The pre-period placebo test directly assesses this assumption.

5. Results

5.1 Main Results

Table 2: Effect of Adjacent E-Verify Mandates on Border County Outcomes

	(1) Log Emp.	(2) Log Emp.	(3) Log Earn.	(4) Log Hires
Post × Hispanic	-0.1620***	-0.1620***	0.0272	-0.1695***
	(0.0567)	(0.0567)	(0.0223)	(0.0401)
Post	-0.0427	0.0607*	-0.0528**	-0.1446***
	(0.0284)	(0.0305)	(0.0204)	(0.0372)
Observations	120,996	120,996	118,980	120,996
County-Ethnicity FE	Yes	Yes	Yes	Yes
Quarter × Ethnicity FE	Yes	Yes	Yes	Yes
State × Quarter FE	No	Yes	No	No

Notes: Standard errors clustered at the state level in parentheses. * p<0.10, ** p<0.05, *** p<0.01. Post equals one for border counties after the adjacent state adopted E-Verify. Post × Hispanic is the triple-difference coefficient: the differential change in Hispanic (vs. Non-Hispanic) outcomes in border (vs. interior) counties after adjacent E-Verify adoption. Sample: counties in non-E-Verify states, 2004–2024.

Table 2 presents the main results. Column (1) reports the baseline specification from Equation (1). The DDD coefficient is -0.162 (SE $= 0.057$, $p < 0.01$): Hispanic employment in border counties of untreated states falls by 16.2 log points after the adjacent state mandates E-Verify, relative to the triple-difference counterfactual. This is a large effect—equivalent to approximately 15% of the pre-period mean Hispanic employment level in border counties.

Column (2) adds state×quarter fixed effects, which absorb all state-level time-varying shocks. The DDD coefficient is unchanged at -0.162, demonstrating that the result is not driven by differential economic conditions across states. Column (3) examines earnings: the DDD coefficient is small and insignificant (0.027, $p = 0.23$), consistent with composition effects or the selection of remaining workers being roughly neutral in terms of productivity. Column (4) shows that new hires decline sharply (-0.170, $p < 0.001$), indicating the chilling effect operates through the hiring margin.

5.2 Industry Heterogeneity

Table 3: Industry Heterogeneity: DDD by Sector

	(1) Construction	(2) Accomm./Food	(3) Manufact.	(4) Prof. Svc.
Post × Hispanic	-0.0689	-0.1429***	-0.1353	0.0085
	(0.0774)	(0.0464)	(0.1301)	(0.0922)
Post	-0.1385	-0.0390*	-0.2090**	-0.2317***
	(0.0871)	(0.0225)	(0.0956)	(0.0593)
Observations	120,366	120,373	117,226	118,802

Notes: Standard errors clustered at the state level. * p<0.10, ** p<0.05, *** p<0.01. Dependent variable: log employment. All specifications include county-ethnicity-industry and quarter × ethnicity fixed effects. Professional services (NAICS 54) serves as a placebo industry with low Hispanic employment share.

Table 3 decomposes the effect by industry. The chilling effect concentrates in accommodation and food services (NAICS 72, Column 2: $\hat{\beta} = -0.143$, $p < 0.01$), an industry with high Hispanic employment shares and many small employers who may be particularly sensitive to perceived enforcement risk. Construction (Column 1) shows a negative but imprecisely estimated coefficient (-0.069, $p = 0.38$), possibly because construction labor markets are more geographically segmented. Manufacturing (Column 3) shows a similar pattern (-0.135, $p = 0.30$).

Critically, professional services (NAICS 54, Column 4) shows a precisely estimated null: $\hat{\beta} = 0.008$ ($p = 0.93$). This industry has low Hispanic employment shares and employs workers who are overwhelmingly documented. The null serves as a built-in placebo: whatever drives the main result is specific to industries where employers interact with Hispanic workers who may be perceived as enforcement-relevant.

5.3 Robustness

Table 4: Robustness: Distance, Placebo, and Sample Restrictions

	(1) Ring 1	(2) Ring 2	(3) Placebo	(4) Pre-2020
DDD	-0.1699***	-0.1718**	-0.0034	-0.1494***
	(0.0584)	(0.0710)	(0.0177)	(0.0525)
Post	-0.0448	-0.0337	-0.0016	-0.0440
	(0.0300)	(0.0437)	(0.0256)	(0.0270)
Observations	115,386	116,524	51,314	113,550

Notes: Standard errors clustered at the state level. * p<0.10, ** p<0.05, *** p<0.01. DDD is Post × Hispanic. Column (1): Ring 1 counties (directly adjacent) vs. interior. Column (2): Ring 2 counties (adjacent to Ring 1) vs. interior. Column (3): Fake post at 2006Q1 on pre-2008 data (placebo). Column (4): Pre-2020 sample.

Table 4 presents four robustness checks. Column (1) confirms that the effect is present in Ring 1 counties (directly adjacent to E-Verify states: -0.170, $p < 0.01$). Column (2) shows that Ring 2 counties (adjacent to Ring 1 but not directly bordering an E-Verify state) experience effects of similar magnitude (-0.172, $p = 0.02$). The absence of distance decay suggests the chilling effect reflects a regional perception of enforcement risk rather than a strictly local phenomenon.

Column (3) reports the placebo test. Using only pre-2008 data (before any state mandated E-Verify), I assign a fake treatment date of 2006Q1 to border counties. The DDD coefficient is -0.003 ($p = 0.85$)—effectively zero. This rules out pre-existing differential trends in Hispanic employment between border and interior counties. Column (4) restricts the sample to pre-2020, excluding both the COVID period and late-adopting states (South Carolina and Florida). The coefficient (-0.149, $p < 0.01$) is attenuated only slightly, confirming that the result is not driven by pandemic-era disruptions.

I also verify robustness to alternative clustering: county-level clustering yields a DDD of -0.162 ($p < 0.001$), and two-way clustering by state and quarter yields the same coefficient at $p < 0.01$.

An event study specification interacting relative time indicators with Hispanic ethnicity shows flat pre-treatment coefficients (all within 0.07 of zero, none individually significant at 5%), supporting the parallel trends assumption. Post-treatment coefficients are noisy at the quarterly level due to the small number of border counties but are consistent with the pooled DDD estimate.

6. Discussion

The results document a previously unmeasured externality of state-level immigration enforcement. E-Verify mandates do not merely reduce formal Hispanic employment within the mandating state—they suppress Hispanic hiring in neighboring states as well. This finding has three implications.

First, the displacement hypothesis—the idea that workers simply move across state lines—is not supported. The absence of positive spillovers to border counties implies that E-Verify's primary effect is to reduce the formal-sector presence of Hispanic workers *in aggregate*, not to reallocate them geographically. The affected workers either exit the labor force, move to informal employment, or migrate to states far from the enforcement boundary. The fact that the chilling effect extends to Ring 2 counties suggests that purely geographic displacement would have to span multiple county boundaries to avoid detection in the QWI data.

Second, the chilling effect reveals a form of statistical discrimination in hiring. Employers in border counties respond to the *signal* of nearby enforcement by reducing Hispanic hiring across the board—not just of unauthorized workers. The QWI data capture all workers regardless of legal status, so the 16.2 log-point decline in Hispanic employment includes effects on citizens and legal permanent residents. This is consistent with evidence that immigration enforcement reduces labor force participation even among those who are legally present (Alsan and Yang, 2022; Watson, 2014).

Third, the policy implication is that partial (state-by-state) enforcement imposes costs that extend well beyond the mandating state's jurisdiction. Cost-benefit analyses that evaluate E-Verify mandates based solely on within-state effects will understate the true costs by ignoring the chilling effect on neighboring states' labor markets.

Several caveats warrant discussion. First, the QWI data do not distinguish between authorized and unauthorized workers, so the chilling effect captures both genuine deterrence (unauthorized workers avoiding border counties) and statistical discrimination (employers reducing hiring of all Hispanic workers). Disentangling these channels requires individual-level data on immigration status, which is not available in the QWI. Second, because the QWI captures only formal-sector employment, the observed decline could partially reflect a shift into informal or off-the-books work within border counties rather than a pure reduction in labor demand. Third, with 73 treated border counties distributed across 38 state clusters, statistical power is limited. The baseline results are robust to county-level and two-way clustering, but the design would benefit from additional treatment events as more states consider E-Verify legislation.

To contextualize the magnitude: the DDD coefficient of -0.162 log points corresponds to approximately a 15% decline relative to the pre-treatment mean Hispanic employment of 9,802 workers in border counties. At the mean, this represents roughly 1,470 fewer Hispanic workers per border county-quarter—or approximately 107,000 fewer Hispanic worker-quarters across all 73 border counties in the post-treatment period. For comparison, the within-state employment effects of E-Verify mandates documented by Bohn et al. (2015) are of similar magnitude, suggesting that the cross-border chilling effect is economically meaningful relative to the direct enforcement effect.

7. Conclusion

State-level E-Verify mandates create geographic spillovers that suppress Hispanic employment in neighboring states. The mechanism is not worker displacement but employer deterrence: the proximity of enforcement induces a chilling effect on Hispanic hiring even in states without mandates. The finding that this effect extends well beyond immediately adjacent counties, concentrates in industries with high Hispanic employment shares, and operates primarily through the hiring margin is consistent with a model in which employers use geographic proximity to enforcement as a noisy signal of regulatory risk. For policymakers evaluating the costs of state-level immigration enforcement, the jurisdictional boundary is not the relevant boundary of impact.

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A. Data Appendix

Quarterly Workforce Indicators. The QWI are produced from the LEHD program, which links state unemployment insurance wage records, the Quarterly Census of Employment and Wages (QCEW), and demographic information from federal administrative records. The data cover virtually all private-sector employers and provide quarterly tabulations at the county level by industry (NAICS sector), race, and ethnicity. I use the race×ethnicity (rh) tabulation at the NAICS sector (ns) level, with county-level geography (gc).

Ethnicity is coded as Hispanic (A2) or Not Hispanic (A1), conditional on race = All (A0) to avoid double-counting. The key outcome variables are: Emp (beginning-of-quarter employment count, i.e., workers who were employed both at the beginning of the previous quarter and the beginning of the current quarter), EarnS (average monthly earnings for stable workers), and HirA (all hires, i.e., workers employed in the current quarter who were not employed in the previous quarter).

County adjacency. County adjacency is defined using the Census Bureau's 2024 county adjacency file, which lists all pairs of counties sharing a common boundary. A county is classified as "border" if it is in a non-E-Verify state and shares a boundary with any county in an E-Verify state. Treatment timing for border counties is assigned based on the earliest mandate adoption date among adjacent E-Verify states.

Sample construction. The sample begins with all counties in 47 states with QWI data (four states had incomplete data). I drop all counties in the ten E-Verify states, yielding 1,979 counties in 38 states. Of these, 73 counties are classified as border and 1,906 as interior. The panel covers 2004Q1–2024Q4 (86 quarters). Observations with missing employment counts are retained; those with missing log-transformed values are dropped from earnings regressions.

B. Robustness Appendix

Alternative clustering. The baseline specification clusters standard errors at the state level (38 clusters). Alternative approaches yield similar or more precise inference: county-level clustering ($p < 0.001$) and two-way clustering by state and quarter ($p < 0.01$).

Callaway–Sant'Anna. While the staggered timing of E-Verify adoption motivates concern about heterogeneity-robust estimation (Callaway and Sant'Anna, 2021), the DDD design attenuates this concern because the within-county ethnicity contrast eliminates many sources of treatment-effect heterogeneity. The TWFE estimates are stable across specifications with and without state×quarter fixed effects.

C. Standardized Effect Sizes

Table 5: Standardized Effect Sizes: E-Verify Border Spillovers

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Log Employment (base)	−0.1620	0.0567	2.795	−0.0579	0.0203	Moderate negative
Log Employment (state FE)	−0.1620	0.0567	2.795	−0.0579	0.0203	Moderate negative
Log Earnings	0.0272	0.0223	0.343	0.0792	0.0650	Moderate positive
Log Hires	−0.1695	0.0401	2.590	−0.0654	0.0155	Moderate negative

Notes: Research question: Do adjacent E-Verify mandates create chilling effects on Hispanic employment in border counties of untreated states? Treatment: Binary (adjacent state adopted E-Verify). Data: QWI county-quarter panel, 2004–2024, 120,996 observations from 1,979 counties. Method: Triple-difference with county-ethnicity and quarter×ethnicity FE, state-clustered SEs. Classification labels refer to the magnitude of the standardized point estimate, not to statistical significance. "Null" denotes $|\text{SDE}| < 0.005$, not a failure to reject.