THE EFFECT OF E-VERIFY LAWS ON CRIME

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E-Verify laws, which have been adopted by 23 states, require employers to verify whether new employees are eligible to legally work prior to employment. This study explores the impact of state E-Verify laws on crime. Using data from the 2004–2015 National Incident Based Reporting System, the authors find that the enactment of E-Verify is associated with a 7% reduction in property crime incidents involving Hispanic arrestees. This finding was strongest for universal E-Verify mandates that extend to private employers and its external validity bolstered by evidence from the Uniform Crime Reports. Supplemental analyses from the Current Population Survey suggest two mechanisms to explain this result: E-Verify-induced increases in the employment of low-skilled natives of Hispanic descent and out-migration of younger Hispanics. Findings show no evidence that arrests were displaced to nearby jurisdictions without E-Verify or that violent crime or arrests of African Americans were affected by E-Verify laws. The magnitudes of the estimates suggest that E-Verify laws averted $491 million in property crime costs to the United States.

Interior enforcement policies have grown substantially in the United States during the past two decades (Michaud 2010; Miles and Cox 2014; Treyger, Chalfin, and Loeffler 2014). One of the most widespread of these policies requires employers to verify the work eligibility of recent hires. Such statutes, known as E-Verify mandates, have been adopted by 23 states and the federal government.

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Under an E-Verify mandate, employers must compare information from their new hires' Employment Eligibility Verification (I-9) forms with electronic records from the Social Security Administration and Department of Homeland Security (DHS 2018). An employee's name, Social Security number, date of birth, citizenship status, and (if applicable) non-citizen-related information is compared to electronic federal records to assess work eligibility. A mismatch prompts an alert to the employer that must be resolved within 10 federal workdays or the employee must be fired. Failure to comply with state E-Verify laws can result in substantial fines for employers as well as business license revocation. The majority of state E-Verify mandates (16/23) apply to public employers or private employers with public contracts, while eight (8) state mandates extend to private employers (NCSL 2015).[1]

This study asks, Do E-Verify laws have important spillover effects on crime? The impact of state E-Verify mandates on crime is theoretically ambiguous. The policy's net crime impact depends on the magnitudes of its effects on 1) labor market outcomes for low-skilled immigrants (unauthorized and authorized) and natives, 2) mobility of affected workers, and 3) the distribution of these effects across low-skilled populations with heterogeneous propensities for crime. On one hand, E-Verify may reduce unauthorized immigrants' labor market prospects (Amuedo-Dorantes and Bansak 2014; Orrenius and Zavodny 2015), leading to an increase in property or drug crimes for income-generating purposes (Raphael and Winter-Ebmer 2001; Öster and Agell 2007). Furthermore, if unauthorized immigrants serve as complements to higher-skilled native workers (Lee, Peri, and Yasenov 2017; East, Luck, Mansour, and Velasquez 2018), unemployment among natives may increase crime. E-Verify laws may also shift the population composition away from likely unauthorized immigrants and toward low-skilled natives (Good 2013; Bohn, Lofstrom, and Raphael 2014; Orrenius and Zavodny 2016).

On the other hand, if low-skilled natives and immigrants are labor substitutes (Amuedo-Dorantes and Bansak 2014; Orrenius and Zavodny 2015), then improved labor market prospects for natives may reduce crime involving natives. E-Verify mandates may also reduce crime if a net out-migration of low-skilled Hispanics occurs.[2] Finally, E-Verify may reduce unauthorized immigrants' willingness to report crime due to fear of detection.

Using data from the 2004–2015 National Incident-Based Reporting System (NIBRS) and the Uniform Crime Reports (UCR)—and a difference-in-differences empirical strategy—this study is the first to comprehensively examine the impact of state E-Verify mandates on crime, with particular attention to criminal incidents involving arrestees of Hispanic descent.

[1]Tennessee and Louisiana adopted E-Verify laws that apply to both private and public employers but gave employers an alternative means of verifying employment outside of the E-Verify system (NCSL 2015). The federal E-Verify mandate applies to public employees and government contractors.

[2]If Hispanic women increase informal labor market work in response to formal-sector job loss for Hispanic men (Orrenius and Zavodny 2015), female crime may fall.

Supplemental analysis of the 2004–2015 Current Population Survey (CPS) explores potential mechanisms through which E-Verify could affect crime: 1) employment opportunities for low-skilled immigrants and natives, and 2) the demographic (i.e., low-skilled immigrant) composition of states.

Background and Literature

History of Interior Immigration Reform

The Immigration Reform and Control Act (IRCA) of 1986 granted amnesty to previously unauthorized immigrants, while also attempting to limit the involvement of unauthorized immigrants in the US economy through mandated use of the I-9 system. However, IRCA did not require employers to verify the authenticity of workforce eligibility documentation provided by employees "if the document reasonably appear[ed] . . . to be genuine" (Kerwin and McCabe 2011). Employers complying in good faith with the I-9 system were entitled to an affirmative defense to federal sanctions (Castillo and Schulman 2011). During this period, false documentation was common, penalties were rarely administered (Baker 2015; Orrenius and Zavodny 2015), and attempts to increase enforcement were met by political opposition (Hanson 2006).

A decade later, the Illegal Immigration Reform and Immigrant Responsibility Act (IIRIRA) of 1996 addressed several problems left by IRCA. IIRIRA introduced the 287(g) program, authorizing Immigration and Customs Enforcement (ICE) to enter into agreements with state and local law enforcement agencies. The agreements allow these agencies to carry out some immigration enforcement actions, including, in some cases, interrogation and arrest of non-citizens.[3] Later, the Secure Communities program was launched to target criminal unauthorized immigrants by comparing fingerprints collected at the time of arrest with the DHS Automated Biometric Identification System (Kubrin 2014). The system notifies ICE when a deportable criminal is arrested, though there is little evidence that Secure Communities had a discernable effect on crime (Miles and Cox 2014; Treyger et al. 2014).

IIRIRA also introduced an electronic employment verification system known as the Basic Pilot Program, which was made available to employers in all states for voluntary use in 2003 (National Immigration Law Center 2011), becoming the precursor to E-Verify. Colorado became the first state to mandate E-Verify for public employers and state contractors in 2006. Subsequently, 22 additional states have enacted E-Verify laws, with the majority of these mandates applying to public employers (or private employers with state contracts). Eight states mandate all employers use E-Verify. In

[3]ICE touts that program as a "force multiplier in the identification, arrest, and service of warrants and detainers of incarcerated foreign-born individuals." See https://www.ice.gov/287g.

addition, E-Verify is required for all federal workers and contractors and is annually reauthorized by Congress (Park and Friedman 2008).

Enforcement of E-Verify occurs at both the federal and state levels. The Monitoring and Compliance Branch of US Citizenship and Immigration Services (USCIS) enforces federal E-Verify policies with the goal of reducing abuse and fraud. Most state E-Verify laws include sanctions for non-compliance, such as fines (usually between $250 and $1,000 per violation, but sometimes reaching as high as $10,000 per offense), termination of business licenses, or temporary bans from state contracts. Some states grant firms immunity from liability for employing undocumented workers if E-Verify was used (Park and Friedman 2008). In 2015, 50% of new hires nationwide were verified through the system (Orrenius and Zavodny 2017), representing a 150% increase from 2011 (Rosenblum 2011).

Labor Market Effects of E-Verify Laws

The first wave of studies on E-Verify examined the labor market effects of these mandates. Using data from the 2004–2010 Current Population Survey (CPS), Amuedo-Dorantes and Bansak (2012) found that E-Verify mandates were associated with a 3 to 7% decline in employment for likely unauthorized immigrants. Similarly, Bohn and Lofstrom (2012) found that Arizona's E-Verify law reduced employment of likely unauthorized men by 11 percentage points and shifted some toward informal work. Only limited evidence suggests that E-Verify laws increase job lock (Orrenius, Zavodny, and Gutierrez 2018).

E-Verify mandates also generate important spillover effects. Orrenius and Zavodny (2015) used data from the 2002–2012 CPS to explore the impact of E-Verify on employment and wages among workers who may compete with unauthorized immigrants. They found universal E-Verify mandates increased wage-and-salary employment among Mexican-born naturalized citizens by 8 percentage points and increased real earnings among native-born Hispanics by 9%. The authors found little evidence, however, that E-Verify affected employment among non-Hispanic whites. Of interest, these authors found evidence that E-Verify laws increased labor force participation among likely unauthorized females (often in the informal sector), and they attributed this result to female spouses responding to the decline in formal-sector employment for their husbands.

E-Verify may also affect labor market outcomes through selective migration. Bohn et al. (2014) used data from the 1998–2009 CPS to examine the impact of Arizona's E-Verify mandate. Using a synthetic control approach, they found this law was associated with a 2 to 3% reduction in the share of the state population comprising non-native Hispanics. Extending this analysis to E-Verify mandates adopted nationwide, Orrenius and Zavodny (2016) found that E-Verify mandates were associated with a 50% reduction in the number of newly arriving, low-skilled, prime-age immigrants from Mexico and Central America.

Immigration, Labor Market Opportunities, and Crime

Becker's (1968) theory of rational crime suggests that criminal behavior should be responsive to labor market conditions. Indeed, empirical studies that analyzed those on the margin of criminal behavior found that arrests are positively related to local unemployment rates (Levitt 2004; Lin 2008; Schnepel 2018) and business cycle contractions (Rosenfeld and Fornango 2007), and are negatively related to low-skilled wages (Gould, Weinberg, and Mustard 2002).

The relationship between labor market opportunities and crime appears to extend to immigrants. Baker (2015) found IRCA's amnesty provision reduced property crime by 3 to 5% through enhanced levels of human capital and greater labor market opportunities. Subsequently, Freedman, Owens, and Bohn (2018) found that increased barriers to legal employment associated with the expiration of IRCA's amnesty provisions led to a 59% increase in felonious prosecutions of Hispanic residents of Bexar County, Texas, for income-generating crime (i.e., theft, prostitution, fraud).[4]

E-Verify and Crime

Two papers of which we are aware have explored the relationship between E-Verify and arrests. Zhang, Palma, and Xu (2016) used 1998–2014 UCR data to examine the impact of Alabama's HB 56 E-Verify statute on arrests. Using a synthetic control approach, the authors found that Alabama's E-Verify mandate was associated with approximately 100 more violent crime arrests per year for every 100,000 adults, but it was statistically unrelated to property crime arrests. The authors attributed this result to diminished labor market opportunities for likely unauthorized immigrants but offered little compelling reason why violent crime rose while income-generating property crime was unaffected.[5]

Chalfin and Deza (forthcoming) explored the effect of Arizona's E-Verify mandate on arrests using UCR data and two identification strategies: a state-level synthetic control approach and an agency-level difference-in-differences approach. They found that the mandate was associated with a 20% decrease in property crime arrests involving young men ages 15 to 24, but they found no statistically significant change in violent crime arrests. The authors posited that the property crime reduction was due to out-migration of foreign-born Mexicans.

[4]Other studies have more broadly explored the relationship between immigration and crime; see, for example, Butcher and Piehl (1998a,b), Amuedo-Dorantes, Banzak, and Pozo (2021), and Lott (2018).

[5]They suggested that E-Verify could have incited violence against unauthorized immigrants. One concern about this study is that in 2011, the Alabama Criminal Justice Information Center (ACJIC) adopted a new electronic record system, which may have improved arrest reporting (ACJIC 2011).

The current study makes a number of contributions. It is the first to comprehensively examine the effect of state E-Verify laws on crime, exploiting policy changes in up to 23 US states. Second, we provide the first estimates of the effect of E-Verify on criminal incidents involving Hispanic arrestees. This contribution is important given that the E-Verify literature suggests heterogeneous labor market and migration effects for both Hispanic unauthorized immigrants and natives of Hispanic descent. Moreover, given that native Hispanic arrestees make up the majority of all Hispanic arrestees, the impact of E-Verify on immigrants alone will fail to capture the full policy impact.[6] Third, we explore spillover effects of E-Verify on arrests involving African American and white adults. Finally, this study is the first to examine whether the crime effects of E-Verify laws differ by whether mandates require private as well as public employers to comply with E-Verify.

Data and Methods

National Incident-Based Reporting System

We use agency-by-month data from the 2004–2015 National Incident-Based Reporting System (NIBRS) to estimate the impact of state E-Verify mandates on criminal incidents involving Hispanic arrestees. The NIBRS is collected by the Federal Bureau of Investigation (FBI) and includes incident-level crime data collected from local law enforcement agencies. The reporting agency is usually a city or township, with NIBRS agencies often serving relatively small populations.[7] Approximately 93 million Americans, or 29% of the US population, are covered by the NIBRS, accounting for 27% of all US crime (FBI National Press Office 2015).

The NIBRS data are especially useful for this study because they contain information on the arrestee's ethnicity; however, their coverage is much sparser than the UCR. Thus, in contrast to studies using UCR data to generate county or state arrest rates (Gould et al. 2002; Cáceres-Delpiano and Giolito 2012; Anderson 2014), best practices using NIBRS data examine agency-level observations in panel-based analyses with agency fixed effects (Card and Dahl 2011; Heaton 2012; Lindo, Siminski, and Swensen 2018).[8]

[6]Landgrave and Nowrasteh (2019) estimated incarceration rates per 100,000 population, ages 18–54 in 2017, are 1,792 for Hispanic natives, 507 for legal Hispanic immigrants, and 1,097 for unauthorized Hispanic immigrants. In addition, Lott (2018) found that 72% of all Hispanic incarcerations in Arizona between 1985 and 2017 involved US citizens of Hispanic descent.

[7]In 2014, 86% of all NIBRS reporting agencies served local populations of less than 50,000, and 42% served local populations of less than 10,000.

[8]While intercensal estimates of age- and race/ethnicity-specific population counts are readily available at the state, county, and large metropolitan area levels, this is not always the case for smaller cities and townships. Because NIBRS coverage is often not complete within counties or states, it is not appropriate to aggregate agency-level arrest counts to the county or state levels and generate age/race/ethnicity-specific arrest rates. Given these challenges, NIBRS-based studies in the economics of crime literature have generally used arrest counts (controlling for overall agency population, which we do) rather than arrest rates as the outcome of interest.

We measure criminal incidents at the agency-by-month level. Our main sample consists of a balanced panel of agency-months, which is expected to reduce measurement error generated by clustered or inconsistent crime reports. However, we experimented with alternate samples, including agencies that reported in at least half the years covering the sample period, or agencies serving counties of at least 10,000 population. These strategies produced a qualitatively similar pattern of results.

Our main outcome from the NIBRS, *Hispanic property crime*, is an agency-by-month count of property crime incidents involving a Hispanic arrestee age 16 to 64. Property crime is defined as burglary, larceny theft, motor vehicle theft, and arson. We generate *Hispanic violent crime* analogously. These crimes include murder, rape, robbery, and aggravated assault.[9] In Online Appendix Table A.1A (hereafter, numbering for Online Appendix material is prefaced with an "A."), we report the mean number of property and violent criminal incidents involving Hispanics ages 16 to 64, and populations of non-Hispanic whites and African Americans, respectively, are also explored below.

Uniform Crime Reports

While the NIBRS data allow us to identify criminal incidents involving Hispanic arrestees, these data lack national coverage. By contrast, the UCR data are representative of the entire United States, covering 98% of the population. As shown in Table 1, all 23 states that enacted E-Verify legislation between 2004 and 2015 contribute to identification in the UCR. Unfortunately, the UCR do not report arrestee ethnicity. We are able to measure criminal arrests of adults by race only, and race is coded as white or African American. According to 2004 data from the Surveillance Epidemiology and End Results (SEER) program, 15.7% of all whites in the United States were of Hispanic descent and 4.6% of all African Americans were of Hispanic descent.[10] While we cannot measure ethnicity in the UCR, we do explore heterogeneity in the effects of E-Verify laws on arrests of whites by jurisdictions that had relatively higher shares of Hispanic white residents in 2004 prior to the adoption of E-Verify laws.

In contrast to the NIBRS, which is measured at the incident level, the UCR data are measured at the arrest level. We measure agency-by-month criminal arrest counts for adults ages 18 and older. We restrict our sample to agency-month observations that report in at least 90% of our sample period. Our key outcome variables in the UCR include *African American property arrests, African American violent arrests, White property arrests*, and *White*

[9]Approximately one-fifth of all criminal incidents involving a 16- to 64-year-old arrestee had an unknown, unreported, or missing ethnicity. Less than 1% of incidents involved an arrestee of unknown race. We found little evidence of a relationship between E-Verify and reporting of arrestee ethnicity (or race).

[10]Using the 2004–2015 NIBRS, we find that less than 5% of all white arrestees are of Hispanic descent.

Table 1. Effective Dates of State E-Verify Laws, 2004–2015

State	Effective date	Coverage	*Identifying variation?* NIBRS	UCR	CPS
Alabama	April 1, 2012	Universal	No	Yes	Yes
Arizona	December 31, 2007	Universal	No	Yes	Yes
Colorado	August 7, 2006	Public	Yes	Yes	Yes
Florida	January 4, 2011	Public	No	Yes	Yes
Georgia	July 1, 2007	Public	No	Yes	Yes
	January 1, 2012	Universal	No	Yes	Yes
Idaho	July 1, 2009	Public	Yes	Yes	Yes
Indiana	July 1, 2011	Public	No	Yes	Yes
Louisiana	August 18, 2011	Partial	Yes	Yes	Yes
Michigan	March 1, 2013	Public	Yes	Yes	Yes
Minnesota	January 1, 2008	Public	No	Yes	Yes
Mississippi	July 1, 2008	Universal	No	Yes	Yes
Missouri	January 1, 2009	Public	No	Yes	Yes
Nebraska	October 1, 2009	Public	Yes	Yes	Yes
North Carolina	January 1, 2007	Public	No	Yes	Yes
	October 1, 2012	Universal	No	Yes	Yes
Oklahoma	February 2, 2010	Public	No	Yes	Yes
Pennsylvania	January 1, 2013	Public	No	Yes	Yes
Rhode Island	October 17, 2008[a]	Public	Yes	Yes	Yes
South Carolina	January 1, 2009	Public	Yes	Yes	Yes
	January 1, 2012	Universal	Yes	Yes	Yes
Tennessee	October 1, 2011	Partial	Yes	Yes	Yes
Texas	September 1, 2015	Public	Yes	Yes	Yes
Utah	July 1, 2009	Public	Yes	Yes	Yes
	July 1, 2010	Universal	Yes	Yes	Yes
Virginia	December 1, 2012	Public	Yes	Yes	Yes
West Virginia	June 24, 2012	Public	Yes	Yes	Yes

Sources: National Conference of State Legislatures (2015) and Urban Institute (2017).
Notes: CPS, Current Population Survey; NIBRS, National Incident-Based Reporting System; UCR, Uniform Crime Reports.
[a]The E-Verify mandate enacted in Rhode Island in 2008 was repealed on January 5, 2011.

violent arrests. As noted above, to examine E-Verify law effects on Hispanics, we interact indicators for quartiles of the distribution of white Hispanics in the state in 2004 with the E-Verify policy. Table A.1B shows descriptive statistics for arrests from the UCR.

Current Population Survey

To explore the mechanisms through which E-Verify may affect crime, we use the 2004–2015 Current Population Survey (CPS) Basic Monthly Data (BMS). The surveys are administered by the US Bureau of Labor Statistics and are representative of the US population when weighted using appropriate sample weights. The CPS data include information on labor market outcomes, citizenship status, and other demographic characteristics.

In analyzing the potential mechanisms, we restrict our sample to lower-skilled individuals ages 16 to 64 with at most a high school diploma

(Orrenius and Zavodny 2015). We measure whether the respondent is employed (*Any employment*) and then isolate work for pay (*Employed, salary and wages*). We focus on the labor market effects of E-Verify separately for 1) likely unauthorized immigrants defined as less-educated non-citizens of Hispanic descent (Amuedo-Dorantes and Bansak 2014; Orrenius and Zavodny 2015), and 2) US-born Hispanics (citizens). In supplemental analysis, we also examine naturalized citizens of Hispanic descent. Summary statistics of each of the above variables are available in Table A.3.

Methods

In the NIBRS, our unit of analysis is the agency-by-month from 2004–2015 and our dependent variable is a count of criminal incidents involving working-age individuals of Hispanic descent. Following Card and Dahl (2011) and Lindo et al. (2018), we first estimate the following Poisson regression model:

$$(1)\quad \textit{Hispanic Crime}_{at} = \kappa_{at}\mathrm{Exp}(\beta_0 + \beta_1 \textit{EVerify}_{st} + \beta_2 \textit{log}(\textit{pop}_{at}) + \boldsymbol{\beta_3}'\mathbf{X}_{ct} + \boldsymbol{\beta_4}'\mathbf{Z}_{st} + \alpha_a + \tau_t + \varepsilon_{at})$$

where our primary outcome of interest, $\textit{HispanicCrime}_{at}$, measures the number of criminal incidents involving a 16- to 64-year-old Hispanic arrestee reported by agency a in month-by-year t. Each agency is linked to its primary county and state to code the covariates in Equation (1). Our key policy variable, $\textit{EVerify}_{st}$, is an indicator for whether an E-Verify mandate had been enacted in state s at time t (NCSL 2015; Urban Institute 2017). In alternate specifications, we explore heterogeneous treatment effects by whether the law applies solely to public employers (*Public E-Verify*), is mandatory and extends to all private employers (*Mandatory universal*), or extends to private employers, but allows employers an alternate means of compliance (*Partial E-Verify*).

Our controls include the following: $\textit{log}(\textit{pop}_{at})$ denotes the natural log of the agency-level population;[11] the vector $\mathbf{X}_{ct}$ includes county-level controls including demographic characteristics (the age distribution of the county population and the shares of the county population that are male and African American); county-level immigration policies (an indicator for the presence of a 287(g) program, Secure Communities, and an omnibus immigration law); and an indicator for the presence of a ban-the-box law (Doleac and Hansen 2017, 2020; Sabia, Mackay, Nguyen, and Dave 2021).[12] The vector $\mathbf{Z}_{st}$ includes state-level controls for economic conditions (the

[11] Using agency-level Hispanic population produces nearly identical estimates.

[12] Ban-the-box laws prohibit employers from asking employees about their criminal histories at initial job screening and have been found to increase statistical discrimination against African American and Hispanic young adults (Doleac and Hansen 2017, 2020). Recent work by Sabia et al. (2021) suggested that ban-the-box laws increase crime among Hispanic males because of diminished labor market opportunities.

natural log of per capita income, the natural log of the state unemployment rate, and the share of population ages 25 and older with a bachelor's degree); the political climate (an indicator for whether the governor is a Democrat); crime policy controls (the natural logs of lagged police expenditure per capita and lagged police employment per capita); gun policy controls (shall issue concealed carry permit laws [see Donohue et al. 2019], stand-your-ground laws, and the lagged number of background checks); and social policy controls (the natural log of the state minimum wage, the refundable Earned Income Tax Credit [EITC] refundable credit rate, Affordable Care Act-related [ACA] Medicaid expansions, and Supplemental Nutrition Assistance Program [SNAP] asset test vehicle exemptions). (See Table A.4 for the sources for these variables.) Finally, θ_a is a time-invariant agency fixed effect and τ_t is an agency-invariant month-by-year fixed effect.

Identification of β_1 comes from temporal variation in E-Verify laws across states. Figure A.1 depicts the states adopting an E-Verify mandate between 2004 and 2015, and Table 1 lists the effective dates of the laws and comprehensiveness of the state statute. For our NIBRS-based analysis, 12 states contribute to identifying variation. Figure A.2 shows trends in crime for E-Verify and non-E-Verify states.

A causal interpretation of β_1 requires that crime would have evolved similarly in the treatment and control states in absence of the policy change. While this is fundamentally untestable, we examine whether crime among Hispanics evolved similarly *prior* to the implementation of an E-Verify mandate in treatment and control states:

$$\text{(2)} \quad \text{Hispanic Crime}_{atj(s,t)} = \kappa_{at}\text{Exp}(\beta_0 + \textstyle\sum_{j\neq -1} \beta_1^j 1\{EVerify\}_{st} * 1\{Event\ Year\}_{j(s,t)} + \beta_2 log(pop_{at}) + \boldsymbol{\beta_3}'\mathbf{X}_{ct} + \boldsymbol{\beta_4}'\mathbf{Z}_{st} + \alpha_a + \tau_t + \varepsilon_{at})$$

The event study in Equation (2) differs from our primary specification in how it expresses policy variation. The subscript j denotes the number of years before and after a state enacts an E-Verify Law ("event time"). Each β^j_1 describes the change in criminal incidents involving Hispanic arrestees in states that enacted E-Verify compared to those that did not. Specifically, it involves a differential change from year j relative to the event period $j(s,t) = -1$, one year prior to enactment. This approach is designed to descriptively explore whether pre-existing trends in Hispanic arrests drive the adoption of E-Verify laws.

Additionally, we experiment with including state-specific linear time trends, county-level linear time trends, and county-level quadratic time trends to account for time-varying spatial heterogeneity correlated with E-Verify and arrests. Note that the inclusion of geographic-specific trends can bias estimated treatment effects (Goodman-Bacon 2021).

Next, we turn to the UCR, for which we estimate Equation (1), but replace the left-side variable with *White arrests* or *African American arrests*, measured separately for property and violent crime arrests. An important advantage of the UCR over the NIBRS is that all 23 E-Verify states contribute to identification.[13] A limitation, however, is that we cannot explicitly separate arrests of Hispanic whites versus non-Hispanic whites. Therefore, we proxy for arrests that are more likely to be of Hispanic whites using the share of the state population that comprises Hispanics:

$$(3)\quad \text{White Arrests}_{at} = \kappa_{at}\text{Exp}(\gamma_0 + \gamma_1\text{EVerify}_{st} + \gamma_2\text{EVerify}_{st}{*}\text{PctHISP}_{s2004} + \gamma_3\log(\text{pop}_{at}) + \boldsymbol{\gamma_4}'\mathbf{X}_{ct} + \boldsymbol{\gamma_5}'\mathbf{Z}_{st} + \alpha_a + \tau_t + \mu_{at})$$

where $PctHISP_{s2004}$ indicates the pre-E-Verify (year = 2004) distribution of state share of the white population in the state who are of Hispanic descent (focusing on quartiles of the ranked state-specific distribution).

Finally, our CPS-based analysis explores the mechanisms through which E-Verify may affect crime, including employment and state population demographics. We estimate ordinary least squares (OLS) regressions for low-skilled, working-age Hispanics with a high school degree or less. We then stratify that low-skilled Hispanic sample by citizenship status, focusing on non-citizen immigrants and US-born citizens of Hispanic descent.[14] Our primary outcomes of interest measure employment and demographic composition of the respondent. First, we generate a variable indicating whether an individual is employed; then we generate an employment indicator for those employed for wage-and-salary pay (that is, not self-employed). Second, we examine the impact of E-Verify on the demographic composition of the state, with attention to Hispanic immigrants and natives.

Results

Our main findings appear in Tables 2 through 9. Our NIBRS-based tables focus on the estimate of β_1. Coefficient estimates on control variables from our main specifications are shown in Table A.5. Standard errors are clustered at the state level (Bertrand, Duflo, and Mullainathan 2004).

E-Verify Laws and Criminal Incidents Involving Hispanics

In Table 2, row (1), we present estimates of β_1 from Equation (1) for property crime incidents involving Hispanic arrestees. Our most parsimonious specification, which includes agency fixed effects and month-by-year fixed effects (column (1)), shows that the enactment of an E-Verify mandate is associated with an 11.1% [1-exp (–0.118)] *reduction* in property crime incidents involving Hispanic arrestees. The inclusion of county-level

[13] We also experiment with restricting our UCR analysis to the treatment and control states from the NIBRS as well as using the NIBRS data source itself. The pattern of results we uncover suggests that any differences in estimates obtained from the UCR and NIBRS are not attributable to sample selection.

[14] We include state-specific linear time trends as controls in the spirit of the specification estimated by Orrenius and Zavodny (2015). In addition, in results reported in Table A.6, we examine the sensitivity of findings to the use of the Outgoing Rotation Groups (ORG) instead of the Basic Monthly Survey (BMS).

Table 2. Estimated Effect of E-Verify on Crime Involving Hispanic Arrestees, NIBRS 2004–2015

	(1)	(2)	(3)	(4)	(5)	(6)
Property crime	–0.118**	–0.112**	–0.123***	–0.103***	–0.080**	–0.075**
	(0.051)	(0.048)	(0.047)	(0.039)	(0.032)	(0.036)
N	255,744	255,744	255,744	255,744	255,744	255,744
Violent crime	–0.043	–0.023	–0.022	0.015	0.021	–0.007
	(0.063)	(0.053)	(0.053)	(0.046)	(0.042)	(0.049)
N	255,744	255,744	255,744	255,744	255,744	255,744
Agency fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes
Year-by-Month fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes
Demographic controls?	No	Yes	Yes	Yes	Yes	Yes
Political & economic controls?	No	No	Yes	Yes	Yes	Yes
Crime policy controls?	No	No	No	Yes	Yes	Yes
Immigration policy controls?	No	No	No	No	Yes	Yes
Social policy controls?	No	No	No	No	No	Yes

Notes: Poisson estimates are generated using agency-level data drawn from the 2004–2015 National Incident-Based Reporting System (NIBRS). Each regression has controls for agency fixed effects and year-by-month fixed effects. Demographic controls include the share of population ages 25 and older with a bachelor's degree, the share of county population ages 25–54 and ages 55 and older, the share of population that are male, and the share of population that are African American. Political and economic controls include the natural log of per capita income, the natural log of unemployment rates, and an indicator if the state governor is a Democrat. Crime policy controls include the natural logs of police expenditure per capita and police employment per capita and indicators for shall issue laws, stand-your-ground laws, and background check laws. Immigration policy controls include indicators for 287(g) programs, Secure Communities, and omnibus immigration bills. Social policy controls include the natural logs of minimum wages, refundable Earned Income Tax Credit (EITC) rates, indicators for ban-the-box laws, Supplemental Nutrition Assistance Program (SNAP) vehicle exemptions, and Affordable Care Act (ACA) Medicaid expansion. Standard errors are clustered at the state level.
***Significant at 1% level; ** at 5% level; * at 10% level.

demographic controls (column (2)), economic and political controls (column (3)), and crime policy controls (column (4)) has little effect on this estimate. In column (5), we add controls for other state immigration policies, and in column (6), we include a wide set of social welfare policy controls. The result from our most saturated specification (column (6)) suggests that E-Verify mandates are associated with a 7.2% reduction in property crime involving Hispanics. The stability of our estimated policy effects across specifications lends credence to the hypothesis that E-Verify mandates are implemented exogenously to other crime determinants.

In contrast to our property crime estimates, we find no evidence that E-Verify reduces violent criminal incidents among Hispanics (row (2), Table 2). The estimated effects are smaller in magnitude (0.021 to −0.043) and statistically insignificant.[15]

[15]We also calculated p values using a wild cluster bootstrap standard error approach (Cameron, Gelbach, and Miller 2008; Cameron and Miller 2015), which did not qualitatively change our policy conclusions (column (6), p value = 0.06 for property crime and 0.79 for violent crime).

The results in Table 2 suggest that E-Verify mandates reduce economically motivated crimes, which is consistent with E-Verify-induced increases in employment among low-skilled US citizens of Hispanic descent (Bohn, Lofstrom, and Raphael 2015; Orrenius and Zavodny 2015; Orrenius et al. 2018). How plausible is a 7.2% decline in property crime? Orrenius and Zavodny (2015) found that E-Verify was associated with an 8 percentage point increase in wage-and-salary employment among Mexican-born naturalized citizens. Lin (2008) found that a 1 percentage point increase in unemployment was associated with a 2 to 4% increase in property crime. Thus, even after accounting for negative employment effects for unauthorized immigrants (Amuedo-Dorantes and Bansak 2014), a net crime decline of 7.2% among Hispanics is certainly plausible. Given the absence of violent crime declines, our results are unlikely to be entirely driven by outmigration of Hispanics.

Figure 1 plots our event study coefficients. In panel (a), we find no evidence that Hispanic property crime arrests were trending downward in states that later adopted E-Verify compared to states that did not. A reduction occurs approximately one year following enactment and grows over time. By contrast, we continue to find no evidence that E-Verify mandates affect violent crime incidents involving Hispanic arrestees (panel (b)).[16]

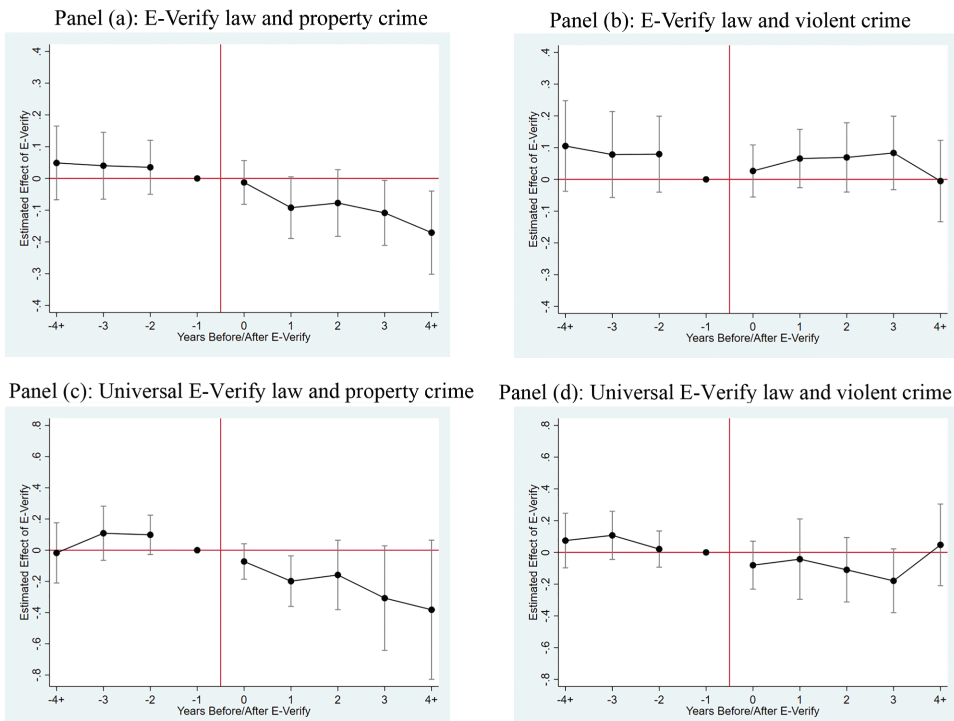
In Table 3, we explore the sensitivity of our estimates to including state- or county-specific time trends. Column (1) reproduces the estimates from column (6) of Table 2. The inclusion of state-specific linear time trends (column (2)), county-specific linear time trends (column (3)), and county-specific quadratic time trends (column (4)) do not change our main finding. Across these specifications, we find that E-Verify laws are associated with a 6.2 to 8.3% reduction in property crime incidents involving Hispanic arrestees.

Heterogeneity in Effects of E-Verify Mandates on Criminal Incidents Involving Hispanics

Tables 4 and 5 explore heterogeneity in the effects of E-Verify mandates by age, gender, breadth of mandate, and offense type. With regard to age, we find that our estimated effects are concentrated among Hispanics ages 20 to 44, a demographic group for whom E-Verify mandates have been shown to have relatively larger labor market effects (Amuedo-Dorantes and Bansak 2014). By contrast, we find no evidence that E-Verify affected property crime incidents involving younger (16–19) or older (45–64) Hispanic arrestees (panel I, column (1)). We find that the property crime-reducing

[16]Table A.7 shows coefficient estimates on lead and lagged effects of E-Verify laws on criminal incidents involving a Hispanic arrestee. In Table A.8, we explore whether our findings are driven by any particular state's E-Verify law. Across samples, our results provide consistent evidence of a 5.1 to 8.7% decline in property crime incidents involving working-age Hispanic arrestees. Estimated effects on violent crime are much smaller and statistically insignificant in all cases.

Figure 1. Event Study Analysis of E-Verify Mandates and Criminal Incidents Involving Hispanic Arrestees, NIBRS 2004–2015



Notes: Poisson estimates are generated using agency-level data drawn from the 2004–2015 National Incident-Based Reporting System (NIBRS). All estimates control for the covariates listed in Table A.2 and state and year fixed effects. Bar lines in panels (a) and (b) show 95% confidence interval generated using standard errors, clustered at the state level. Bar lines in panels (c) and (d) represent 95% confidence interval generated using wild cluster bootstrapped standard errors. The central vertical line delineates the years prior to (*left*) and after (*right*) E-Verify enactment.

effects are concentrated among males (panel II), a population with substantially higher crime rates.

In panel III of Table 4, we explore heterogeneity in the impacts of E-Verify by mandate type: those that apply only to public employers or allow employers an alternate means of compliance with E-Verify (*Public or partial E-Verify*), and those that are mandatory and apply to both private and public employers (*Universal E-Verify*). Our findings suggest substantially larger declines in property crime incidents involving Hispanic arrestees following the passage of mandatory, universal E-Verify mandates (23.3%). Because this estimated treatment effect is identified from only two NIBRS states, South Carolina and Utah, we conduct placebo tests on untreated states to generate a permutation-based p value (Buchmueller, DiNardo, and Valletta 2011; Cunningham and Shah 2018) of 0.07. An event study analysis of universal, mandatory E-Verify laws (panel (c) of Figure 1) suggests that the Hispanic property crime decline takes approximately two years to unfold.

Table 3. Robustness of Hispanic Arrest Effects of E-Verify to Controls for State- and County-Level Time Trends

	(1)	*(2)*	*(3)*	*(4)*
Property crime	–0.075**	–0.087**	–0.061*	–0.064**
	(0.036)	(0.041)	(0.032)	(0.032)
N	255,744	255,744	255,744	255,744
Violent crime	–0.007	–0.031	0.046	0.021
	(0.049)	(0.034)	(0.032)	(0.034)
N	255,744	255,744	255,744	255,744
Agency fixed effects	Yes	Yes	Yes	Yes
Year-by-Month fixed effects	Yes	Yes	Yes	Yes
State-specific linear time trends	No	Yes	No	No
County-specific linear time trends	No	No	Yes	Yes
County-specific quadratic time trends	No	No	No	Yes

Notes: Poisson estimates are generated using agency-level data drawn from the 2004–2015 National Incident-Based Reporting System (NIBRS). Each regression has controls for agency fixed effects, year-by-month fixed effects, linear time trends, and controls listed in Table A.2. Standard errors are clustered at the state level.
***Significant at 1% level; ** at 5% level; * at 10% level.

We fail to detect evidence that universal mandates affect violent crime involving Hispanic arrestees (panel (d) of Figure 1).

In panel IV, we explore whether the estimated crime effects we obtain can be explained by endogenous demographic composition changes of low-skilled immigrants and natives of Hispanic descent. The estimated effect of E-Verify on crime is largely unchanged with the inclusion of controls for the share of the state population that comprises working-age (ages 16–64), less-educated (attained a high school degree or less) Hispanic non-citizen immigrants and working-age, less-educated Hispanic natives. This result suggests that demographic composition changes to E-Verify mandates cannot fully explain their property crime-reducing effects.

In Table 5, we explore the property crime offenses that drove this decline (panel I), as well as examine whether particular violent crimes (panel II) or other criminal incidents (panel III) were affected by E-Verify. Our results show that the decline in property crime is largely driven by larcenies, which account for more than 80% of all property crimes. A substantial negative effect of E-Verify on motor vehicle theft is also seen, though the relationship is less precisely estimated.

We generally fail to detect statistically significant relationships between E-Verify mandates and violent offenses or other non-violent offenses. The only exception is for stolen property, for which we find that E-Verify is associated with a substantial decline, a finding consistent with economically motivated criminal behavior.

Finally, in Table 6, we examine whether E-Verify displaces Hispanic crime to other jurisdictions in close geographic proximity to an E-Verify mandate. We generate two measures: *Border-state E-Verify*, which turns on when a

Table 4. Heterogeneity in Hispanic Arrest Effects of E-Verify, NIBRS 2004–2015

	(1) *Property crime*	(2) *Violent crime*
	Panel I: Age	
Ages 16–19	0.034	0.078
	(0.035)	(0.052)
Ages 20–24	–0.090**	–0.038
	(0.038)	(0.058)
Ages 25–34	–0.166***	–0.031
	(0.054)	(0.054)
Ages 35–44	–0.109**	–0.054
	(0.055)	(0.050)
Ages 45–64	0.008	0.061
	(0.039)	(0.090)
N	255,744	255,744
	Panel II: Gender	
Men	–0.091**	–0.010
	(0.039)	(0.048)
N	255,744	255,744
Women	–0.047	0.045
	(0.039)	(0.062)
N	255,744	255,744
	Panel III: Type of E-Verify mandate	
Public or partial E-Verify	–0.068**	–0.004
	(0.034)	(0.048)
Universal E-Verify	–0.265*	–0.116
	[p value = 0.071][a]	[p value = 0.286][a]
N	255,744	255,744
	Panel IV: Controls for state-by-year share of population that are low-skilled immigrants and natives	
E-Verify	–0.081**	–0.008
	(0.036)	(0.052)
N	255,744	255,744

Notes: Poisson estimates are generated using agency-level data drawn from the 2004–2015 National Incident-Based Reporting System (NIBRS). Each regression has controls for agency fixed effects, year-by-month fixed effects, and controls listed in Table A.2. Standard errors are clustered at the state level.
[a]Because only two states identify Universal E-Verify, we generate our permutation-based p values from placebo tests on non-E-Verify states (Buchmueller et al. 2011; Cunningham and Shah 2018).
***Significant at 1% level; ** at 5% level; * at 10% level.

border state adopts an E-Verify mandate, and *Census-division E-Verify*, which turns on when a state within the state's own census division enacts E-Verify. In odd-numbered columns of Table 6, we restrict the sample to jurisdictions that have never implemented E-Verify and estimate Equation (1), replacing *E-Verify* with *Border-state E-Verify* (panel I) and *Census-division E-Verify* (panel II). In even-numbered columns, we pool all available jurisdictions and add *Border-state E-Verify* (panel I) or *Census-division E-Verify* (panel II) to the right

Table 5. Examination of Detailed Criminal Incidents Involving Hispanic Arrestees, NIBRS 2004–2015

	Panel I: Property crime			
	Larceny	*Burglary*	*Motor vehicle theft*	*Arson*
E-Verify	–0.078**	–0.017	–0.166*	–0.082
	(0.037)	(0.044)	(0.101)	(0.136)
N	255,744	255,744	255,744	255,744
	Panel II: Violent crime			
	Aggravated assault	*Murder*	*Rape*	*Robbery*
E-Verify	0.012	–0.090	–0.097	–0.077
	(0.050)	(0.122)	(0.073)	(0.074)
N	255,744	255,744	255,744	255,744
	Panel III: Other crime			
	Drug	*Stolen property*	*Weapon law violation*	*Sex offenses*
E-Verify	0.048	–0.288***	0.008	–0.020
	(0.041)	(0.060)	(0.055)	(0.046)
N	255,744	255,744	255,744	255,744

Notes: Poisson estimates are generated using agency-level data drawn from the 2004–2015 National Incident-Based Reporting System (NIBRS). Each regression has controls for agency fixed effects, year-by-month fixed effects, and controls listed in Table A.2. Standard errors are clustered at the state level. ***Significant at 1% level; ** at 5% level; * at 10% level.

side of Equation (1). In no case do we uncover evidence that property or violent crime was displaced to neighboring NIBRS jurisdictions.

Mechanisms to Explain Decline in Hispanic Property Crime

Table 7 explores the mechanisms through which E-Verify affects crime involving Hispanic arrestees using data from the CPS-BMS. We present results for all working-age individuals and then individuals ages 20 to 44, the age group for which we find the strongest evidence of crime reductions.

First, generally consistent with prior work (Amuedo-Dorantes and Bansak 2014), we find that the implementation of an E-Verify mandate is associated with a (statistically insignificant) decline in any employment among likely unauthorized male immigrants, on the order of 0.5 to 1.0 percentage points (column (1), panel I).[17] However, we also find that E-Verify is associated with a 2.2 percentage point increase in wage-and-salary employment among low-skilled, US-born Hispanics (column (3), panel II).[18] Given that most

[17]The findings in Table A.6, which use the ORG as compared to the BMS, show larger declines in employment for likely unauthorized Hispanic immigrants.

[18]In Table A.9, we show the estimates for naturalized citizens of Hispanic descent and all Hispanic US citizens. The results for naturalized Hispanics are more similar to non-citizen immigrants than to US-born Hispanics.

Table 6. Exploring Hispanic Crime Displacement in Jurisdictions Neighboring E-Verify States, NIBRS 2004–2015

	(1)	*(2)*	*(3)*	*(4)*
	Property crime		*Violent crime*	
	Panel I: Spillover to border state			
Border-state E-Verify	–0.023	–0.035	0.015	–0.016
	(0.083)	(0.036)	(0.076)	(0.038)
E-Verify		–0.078**		–0.007
		(0.036)		(0.048)
N	105,840	255,744	105,840	255,744
	Panel II: Spillover within Census division			
Census-division E-Verify	0.019	0.008	–0.107	–0.032
	(0.078)	(0.041)	(0.109)	(0.054)
E-Verify		–0.076**		–0.004
		(0.035)		(0.047)
N	105,840	255,744	105,840	255,744
Sample	Non-E-Verify	Pooled	Non-E-Verify	Pooled

Notes: Poisson estimates are generated using agency-level data drawn from the 2004–2015 National Incident-Based Reporting System (NIBRS). Each regression has controls for agency fixed effects, year-by-month fixed effects, and controls listed in Table A.2. Standard errors are clustered at the state level. ***Significant at 1% level; ** at 5% level; * at 10% level.

prosecuted Hispanic defendants are US citizens (Lott 2018; Landgrave and Nowrasteh 2019), this positive employment effect—in conjunction with the fact that immigrants (particularly recent immigrants) are less likely to be criminally prone than are natives (Butcher and Piehl 2008a; Chalfin 2013)—is likely an important channel to explain the net decline in Hispanic property crime.

In panel III of Table 7, we examine a sample of all adults and define the left-side variable as an indicator set equal to 1 if the respondent is a member of the demographic group listed in the column heading, and 0 otherwise. Thus, the estimated policy impact can be interpreted as the effect of E-Verify on the demographic composition of the state. We find that the enactment of an E-Verify law is associated with a 5.3 (–0.0009/0.017) to 5.7% (–0.0008/0.014) decline in the state population share of 20- to 44-year-olds who are less-educated Hispanic immigrants. We also find that E-Verify is associated with a 6.4 (–0.0007/0.011) to 10.9% (–0.0012/0.011) decline in the share of the 20- to 44-year-old state population who were US-born Hispanics.[19] This finding could suggest that mixed-status Hispanic families composed of both likely unauthorized immigrants and natives out-

[19]The results of event study analyses, shown in Figure 2, suggest little difference in pre-treatment trends in the respective demographic shares and evidence that the decline in the share of the state population made up of low-skilled Hispanics follows the enactment of E-Verify. Moreover, event study analysis from the ORG (shown in Figure A.3) displays a similar pattern of results.

Table 7. Exploring Employment and Demographic Composition Mechanisms, CPS-BMS 2004–2015

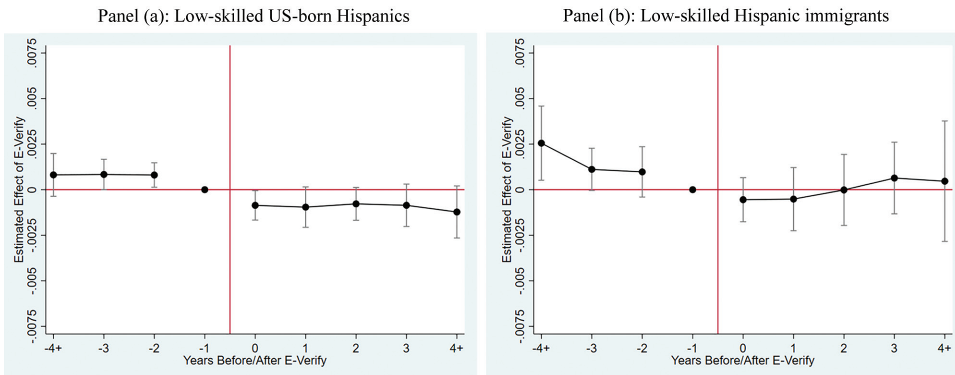
	(1)	(2)	(3)	(4)
	Hispanic immigrants		*US-born Hispanics*	
Ages	*Men*	*Women*	*Men*	*Women*
		Panel I: Any employment		
16–64	–0.007	0.003	0.010	–0.002
	(0.006)	(0.009)	(0.009)	(0.012)
N	249,080	214,237	225,720	223,643
20–44	–0.004	–0.003	–0.004	–0.007
	(0.008)	(0.012)	(0.014)	(0.010)
N	177,758	145,939	115,217	109,479
		Panel II: Wage-and-salary employment		
16–64	–0.010	–0.006	0.022*	0.001
	(0.010)	(0.011)	(0.013)	(0.013)
N	249,080	214,237	225,720	223,643
20–44	–0.005	–0.009	0.012	–0.004
	(0.011)	(0.014)	(0.020)	(0.012)
N	177,758	145,939	115,217	109,479
		Panel III: Demographic composition		
16–64	–0.0010	–0.0011**	–0.0019**	–0.0015*
	(0.0008)	(0.0004)	(0.0009)	(0.0008)
N	12,507,441	12,507,441	12,507,441	12,507,441
20–44	–0.0009	–0.0008**	–0.0012***	–0.0007*
	(0.0005)	(0.0003)	(0.0004)	(0.0004)
N	12,507,441	12,507,441	12,507,441	12,507,441

Notes: Weighted ordinary least squares (OLS) estimates are generated using individual-level data drawn from the 2004–2015 Current Population Survey Basic Monthly Survey(CPS-BMS). Each regression has controls for agency fixed effects, time (year and month) fixed effects, state-specific linear time trends, and controls listed in Table A.2. Standard errors are clustered at the state level.
***Significant at 1% level; ** at 5% level; * at 10% level.

migrate in response to E-Verify enactment. Out-migration is unlikely to be the only mechanism explaining the property crime reductions given that 1) we do not observe a similar decline in violent crime, 2) our property crime results persist after controlling for state shares of low-skilled Hispanic immigrants and natives, and 3) we fail to detect crime spillovers to neighboring jurisdictions without E-Verify laws. However, it is worth noting that we cannot measure international out-migration. If E-Verify mandates induce unauthorized immigrants to return to their native countries, it is possible that we do not detect crime spillovers because they occur in neighboring nations.

In Table 8, we examine the effect of *Universal E-Verify* mandates and *Public or partial E-Verify* mandates on labor market outcomes. While our findings are mixed, the results in panel II do suggest that the magnitude of the effect of E-Verify mandates on wage-and-salary employment for low-

Figure 2. Event Study Analysis of E-Verify Mandates and Hispanic Demographic Composition for 20- to 44-Year-Olds, CPS-BMS 2004–2015



Notes: Weighted ordinary least squares (OLS) estimates are generated using individual-level data drawn from the 2004–2015 Current Population Survey Basic Monthly Survey (CPS-BMS). We define likely undocumented immigrants as those who are less-educated and non-citizen immigrants of Hispanic descent. Bar lines show the 95% confidence intervals, generated using standard errors clustered at the state level. The central vertical line delineates the years prior to (*left*) and after (*right*) E-Verify enactment. Estimates control for covariates listed in Table A.2 and state and year fixed effects.

skilled male citizens of Hispanic descent is greater for E-Verify mandates that extend to private employers relative to only public employers. Event-study estimates in Figure 3 indicate positive employment effects of Universal E-Verify for low-skilled Hispanic natives. This finding is consistent with larger property crime effects for more expansive E-Verify laws.[20]

External Validity

Finally, given that our NIBRS results may not be nationally representative and thus not generalizable, we turn to the UCR. Our results in Table 9 show no evidence E-Verify mandates affected arrests among all adult whites or African Americans (panel I). In columns (1) and (2) of panel II, we interact indicators for the state-level proportion of white adults who are of Hispanic descent with the E-Verify law. The estimated coefficient on the E-Verify law can be interpreted as the effect for states with pre-treatment Hispanic populations in the bottom half of the treatment state Hispanic population distribution, while the remaining coefficients show the differential impact of E-Verify laws on white arrests for states in the 50th to 75th percentile and the 75th to 100th percentile of the Hispanic white population distribution (relative to states with pre-treatment Hispanic populations in the lowest quartile). Consistent with results from the NIBRS, we find that E-Verify enactment is associated with an 8.4% reduction in white property crime arrests in states with the highest shares (> 75th percentile) of Hispanic whites. An event study analysis in Figure 4, panel (a), shows that this decline

[20]Results from the ORG (shown in Table A.10 and Figure A.4) are qualitatively similar.

Table 8. Exploring Heterogeneity in Employment and Demographic Composition Effects, by Breadth of E-Verify Mandate, CPS-BMS 2004–2015

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Hispanic immigrants				*US-born Hispanics*			
	Men		*Women*		*Men*		*Women*	
	16–64	*20–44*	*16–64*	*20–44*	*16–64*	*20–44*	*16–64*	*20–44*
Panel I: Any employment								
Public E-Verify	–0.010*	–0.007	–0.003	–0.006	0.008	–0.004	–0.008	–0.012
	(0.006)	(0.007)	(0.009)	(0.012)	(0.009)	(0.014)	(0.014)	(0.010)
Universal E-Verify	0.006	0.022	0.000	–0.025	0.011	–0.002	0.036**	0.037
	(0.012)	(0.012)	(0.016)	(0.020)	(0.022)	(0.028)	(0.018)	(0.026)
N	249,080	177,758	214,237	145,939	225,720	115,217	223,643	109,479
Panel II: Wage-and-salary employment								
Public E-Verify	–0.013	–0.007	–0.015	–0.016	0.018	0.008	–0.003	–0.008
	(0.009)	(0.010)	(0.011)	(0.014)	(0.013)	(0.021)	(0.014)	(0.011)
Universal E-Verify	0.004	0.011	–0.003	–0.030	0.030	0.037	0.035	0.041
	(0.015)	(0.016)	(0.022)	(0.027)	(0.023)	(0.034)	(0.021)	(0.031)
N	249,080	177,758	214,237	145,939	225,720	115,217	223,643	109,479
Panel III: Demographic composition								
Public E-Verify	–0.0014	–0.0011*	–0.0009**	–0.0007**	–0.0022**	–0.0013***	–0.0017**	–0.0008*
	(0.0009)	(0.0006)	(0.0004)	(0.0003)	(0.0009)	(0.0004)	(0.0008)	(0.0004)
Universal E-Verify	–0.0010	–0.0005	–0.0002	–0.0011	–0.0014	–0.0006	–0.0012	0.0002
	(0.0025)	(0.0016)	(0.0013)	(0.0008)	(0.0009)	(0.0007)	(0.0007)	(0.0004)
N	12,507,441	12,507,441	12,507,441	12,507,441	12,507,441	12,507,441	12,507,441	12,507,441

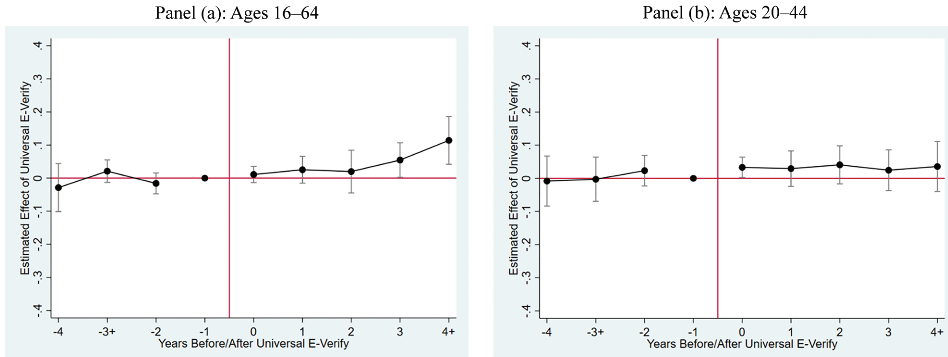
Notes: Weighted ordinary least squares (OLS) estimates are generated using individual-level data drawn from the 2004–2015 Current Population Survey Basic Monthly Survey (CPS-BMS). Each regression has controls for agency fixed effects, time (year and month) fixed effects, state-specific linear time trends, and controls listed in Table A.2. Standard errors are clustered at the state level.
***Significant at 1% level; ** at 5% level; * at 10% level.

in white property crime arrests in higher Hispanic-populated states occurred approximately one year following the adoption of E-Verify. We fail to detect any evidence of E-Verify-induced changes in white property crime arrests in states with lower shares of Hispanics (panel (c) of Figure 4).[21] We find no evidence that E-Verify affected white violent crime arrests in states with larger shares of Hispanic whites (panel II, column (2)), nor do we find evidence that African American arrests (panel II, columns (3) and (4)) responded to E-Verify laws. Finally, in Table 9, panel III, we find that E-Verify effects are somewhat larger for mandatory, universal E-Verify laws, but are not statistically distinguishable from zero at conventional levels.[22]

[21]Panels (e) and (f) of Figure 4 show event study analyses for property and violent crime arrests involving African American arrestees.

[22]We find little to no evidence that E-Verify mandates have an impact on labor force participation among African Americans and non-Hispanic whites, consistent with null crime effects reported above. We do uncover some inconsistent evidence that E-Verify may attract low-skilled, non-Hispanic whites to E-Verify states, consistent with attraction to low-skilled job opportunities.

Figure 3. Event Study Analysis of Universal E-Verify Mandates and Wage-and-Salary Employment for Low-Skilled US-Born Hispanics



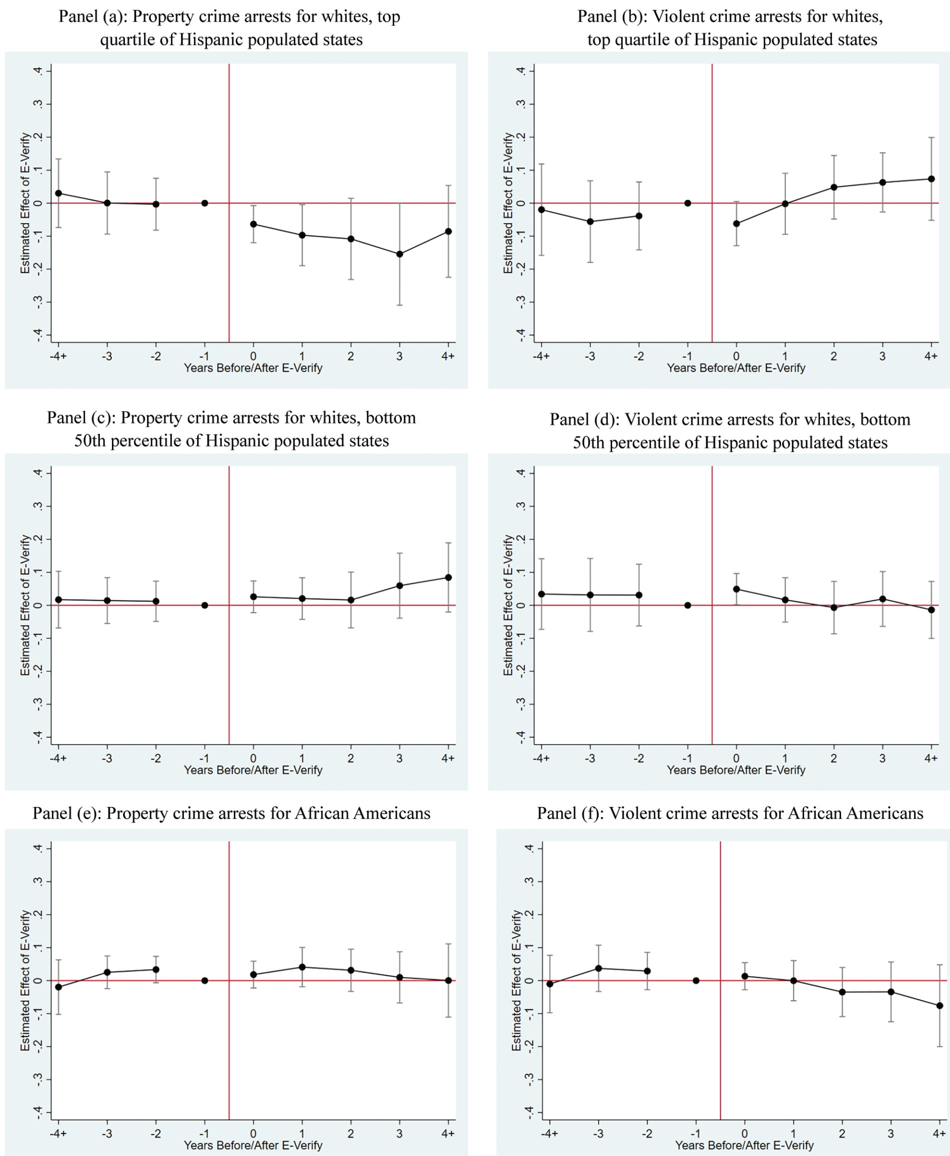
Notes: Weighted ordinary least squares (OLS) estimates are generated using individual-level data drawn from the 2004–2015 Current Population Survey Basic Monthly Survey (CPS-BMS). We define likely undocumented immigrants as those who are less-educated and non-citizen immigrants of Hispanic descent. Bar lines show the 95% confidence intervals, generated using standard errors clustered at the state level. The central vertical line delineates the years prior to (*left*) and after (*right*) E-Verify enactment. Estimates control for covariates listed in Table A.2 and state and year fixed effects.

Conclusions

On January 4, 2019, 17 Republican congressmen re-introduced the Legal Workforce Act of 2019. This legislation would mandate the use of an E-Verify system across all 50 states for public and private employers. Although comprehensive immigration reform has remained one of the most contentious issues in American politics, support is growing for a federal E-Verify law. In June 2019, Senator Mitt Romney (R-Utah) introduced the Permanent E-Verify Act, which would abolish annual Congressional renewal of federal E-Verify requirements, thereby making the federal law permanent. Mounting evidence suggests that E-Verify redistributes employment from likely unauthorized immigrants to low-skilled US citizens of Hispanic descent. This study comprehensively examines the impact of state E-Verify mandates on criminal arrests involving working-age Hispanics, as well as spillovers to African Americans.

Using data drawn from the National Incident-Based Reporting System, we find that E-Verify laws are associated with a 7.2% decrease in property crimes that involve working-age Hispanic arrestees. The effects are largest for males, for those who are age 20 to 44, and for E-Verify policies that extend to private employers. Supplemental analyses from the Current Population Survey suggest that increases in employment among low-skilled citizens of Hispanic descent and, perhaps, out-migration of younger immigrants, likely explain the net reduction in Hispanic crime. We find no evidence of property crime displacement in jurisdictions without an E-Verify mandate and no evidence that E-Verify affects violent crime among Hispanics. Consistent with our NIBRS-based results, analyses of the UCR

Figure 4. Event Study Analysis of E-Verify Mandates and Arrests Involving White or African American Arrestees, UCR 2004–2015



Notes: Poisson estimates are generated using agency-level data drawn from the 2004–2015 Uniform Crime Report (UCR). All estimates control for covariates listed in Table A.2 and state and year fixed effects. Bar lines represent 95% confidence interval generated using standard errors, clustered at the state level, and the central vertical line delineates the years prior to (*left*) and after (*right*) E-Verify enactment.

show that E-Verify reduces property crime arrests among whites in states with relatively higher shares of Hispanic whites.

Using the per-offense social cost of property crime reported in McCollister, French, and Fang (2010), our estimates suggest that E-Verify

Table 9. Estimated Effect of E-Verify on African American and White Arrests, UCR 2004–2015

	White		*African American*	
	(1) *Property*	*(2)* *Violent*	*(3)* *Property*	*(4)* *Violent*
	Panel I: Baseline results			
E-Verify	0.001	–0.008	0.012	–0.027
	(0.019)	(0.026)	(0.027)	(0.030)
	Panel II: Effect by state share of Hispanic white			
E-Verify	0.027	–0.000	0.012	–0.023
	(0.024)	(0.039)	(0.040)	(0.045)
50th–75th Percentile × E-Verify	–0.001	–0.078	0.010	–0.059
	(0.039)	(0.074)	(0.062)	(0.073)
75th–100th Percentile × E-Verify	–0.088**	0.034	–0.018	0.074
	(0.034)	(0.049)	(0.054)	(0.080)
	Panel III: Effect by type of E-Verify mandate			
Public E-Verify	0.003	–0.004	0.012	–0.028
	(0.019)	(0.026)	(0.027)	(0.029)
Universal E-Verify	–0.034	–0.065	–0.033	–0.075
	(0.047)	(0.068)	(0.049)	(0.058)
N	1,076,699	1,076,699	1,076,699	1,076,699

Notes: Poisson estimates are generated using agency-level data drawn from the 2004–2015 Uniform Crime Reports (UCR). Each regression has controls for agency fixed effects, year-by-month fixed effects, and controls listed in Table A.2. Standard errors are clustered at the state level.
***Significant at 1% level; ** at 5% level; * at 10% level.

generated $491 million (2018 dollars) in benefits from property crime reduction to the United States.[23] Of course, this estimate may overstate the benefit of crime avoidance to the western hemisphere if crime is displaced to border countries in North and Central America. Moreover, other important costs of E-Verify laws—such as increased compliance costs on firms, higher prices of consumer products, poorer quality job matches, and adverse health and human capital effects on immigrant families (including

[23]Data on property crimes committed from 2004–2015 are obtained using the FBI's Crime in the United States reports (https://ucr.fbi.gov/crime-in-the-u.s/2016/crime-in-the-u.s.-2016/topic-pages/tables/table-1). We then use the 2004–2015 UCR's Arrests by Age, Sex, and Race files to calculate the share of property crime arrests involving men ages 16 to 64. To generate an estimate of the number of crimes committed by men ages 16 to 64, we multiply the crime counts in the 2004–2014 period from the FBI's Crime in the United States report with the share of property crime arrests involving men ages 16 to 64 from the UCR's Arrests by Age, Sex, and Race files. Next, we estimate the number of crimes committed by Hispanic men ages 16 to 64 by multiplying the above crime estimate with the percentage of arrests involving Hispanic male adults (https://ucr.fbi.gov/crime-in-the-u.s/2014/crime-in-the-u.s.-2014/tables/table-43). Using our estimate from Table 2, column (6), we estimate 85,555 fewer property crimes following the enactment of E-Verify mandates. Finally, we used McCollister et al.'s (2010) $5,739 per crime cost of a property offense to obtain a total E-Verify-induced property crime benefit of $491 million.

many children who are US citizens)—must be weighed against the benefits of crime reduction.

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